The series *Boston Studies in the Philosophy of Science* was conceived in the broadest framework of interdisciplinary and international concerns. Natural scientists, mathematicians, social scientists and philosophers have contributed to the series, as have historians and sociologists of science, linguists, psychologists, physicians, and literary critics. Along with the principal collaboration of Americans, the series has been able to include works by authors from many other countries around the world. As European science has become world science, philosophical, historical, and critical studies of that science have become of universal interest as well.

The editors believe that philosophy of science should itself be scientific, hypothetical as well as self-consciously critical, human as well as rational, sceptical and undogmatic while also receptive to discussion of first principles. One of the aims of *Boston Studies*, therefore, is to develop collaboration among scientists and philosophers. However, because of this merging, not only has the neat structure of classical physics changed, but also, a variety of wide-ranging questions have been encountered. As a result, philosophy of science has become epistemological and historical: once the identification of scientific method with that of physics had been queried, not only did biology and psychology come under scrutiny, but so did history and the social sciences, particularly economics, sociology, and anthropology. *Boston Studies in the Philosophy of Science* look into and reflect on all these interactions in an effort to understand the scientific enterprise from every viewpoint.
COGNITION AND FACT
LUDWIK FLECK 1896–1961
COGNITION
AND FACT

Materials on Ludwik Fleck

Edited by
ROBERT S. COHEN AND THOMAS SCHNELLE

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INTRODUCTION

I

Within the last ten years, the interest of historians and philosophers of science in the epistemological writings of the Polish medical microbiologist Ludwik Fleck (1896–1961), who had up to then been almost completely unknown, has advanced with great strides. His main writings on epistemological questions were published in the mid-1930’s, but they remained almost unnoticed. Today, however, one may rightly call Fleck a ‘classical’ figure both of epistemology and of the historical sociology of science, one whose works are comparable with Popper’s *Logic of Scientific Discovery* or Merton’s pioneering study of the relations among economics, Puritanism, and natural science, both also originally published in the mid-1930’s.

The story of this book of ‘materials on Ludwik Fleck’ is also the story of the reception of Ludwik Fleck. In this volume, some essential materials which have been produced by that reception have been gathered together. We will sketch both the reception and the materials.

*Fleck’s Papers on the Philosophy of Science*

For years the reception of Fleck, as far as it existed at all, did not take notice of the fact that Fleck’s philosophical *œuvre* consists of a number of articles in addition to his monograph *Entstehung und Entwicklung einer wissenschaftlichen Tatsache* (Fleck 1935a). Articles were published by him in the late 1920’s, others in the mid-1930’s together with the monograph, and still more after the war in 1946 and 1947. The reason for the long neglect is clear: all but one were published in the Polish language, and thus difficult to reach for foreign readers.

It is therefore not surprising that these philosophical works became available only when more detailed knowledge about Fleck’s life was gained. This began only in 1977: W. Baldamus, at that time already retired from his chair at Birmingham (U.K.), and a refugee from Germany since the 1930’s, was able to stimulate the interest of his student Thomas Schnelle, who began systematic research on Fleck’s bibliography. Further, with the help of all co-

authors of Fleck's medical papers, Schnelle undertook research on Fleck's biography too. A detailed questionnaire was sent to 70 addresses, mainly in Poland and Israel, with an astonishing reaction: nearly 50 questionnaires were returned, many of them together with long, enthusiastic letters and detailed comments. Fleck, at that time already dead for 17 years, was warmly remembered by his former friends and students as an important, dominating, and fascinating figure in their lives. One important result led the way to direct acquaintance with Ludwik Fleck's widow, Ernestina Fleck, and to his son, as well as to his closest friends and colleagues who were still alive in Israel and Poland.

The most moving conversations were, as might be expected, those with Mrs Fleck, at that time (1979) nearly 80 years of age. For her, as well as for a number of others, it was not at all easy to talk to Thomas Schnelle, a young man who had studied Polish language and thus could address himself to her in her mother-tongue but who came to her from a country responsible for the terrible fate of her life: from Germany.

Information gained from the questionnaires was enlarged and deepened through these personal, unstructured interviews carried out during Schnelle's long study periods in Poland and Israel. Altogether there were about 30 such talks, part of a research project on Fleck supported by the Volkswagenwerk Stiftung in 1979. This project was directed by Lothar Schäfer at the University of Hamburg. Its task was to investigate Fleck's biography, and the philosophical and cultural background to the development of his thinking, and to publish his writings. The project lasted three years.

Work on the philosophical and cultural background to Fleck's development made quite clear that this development cannot be understood without also studying his articles in addition to his monograph. In our volume, the principal articles are given in English. Among them, we distinguish three phases in Fleck's work:

(1) The preparatory phase consists of two brief essays from the 1920's [1926a and 1929b]. They show how closely Fleck developed his philosophical thinking while looking at medicine. Step-by-step, reality is called in question by him, more and more radically. These works already contain the full breadth of Fleck's theories of science.

(2) In the main phase (to about 1935) there appeared, in addition to the monograph, two long articles [1935d and 1936] in the most respected Polish philosophical periodical, Przegląd Filozoficzny. These serve to complete the elaboration of Fleck's theory of cognition. Two short articles applied his analysis to medical issues [1934d, 1935b].
(3) In the post-war phase, the most important article is the one in which he analyzes his own observations concerning the theory of science for the case of a lay collective — indeed his lay collective which worked under the terrible conditions of a research laboratory on methods of producing typhus vaccine within the Buchenwald concentration camp [1946h; see also 1947e].

A final phase of Fleck's thought is represented by a paper he wrote shortly before his death: in it, he expresses his opinion with respect to a discussion about 'science and human welfare' in the American journal Science in July 1960. This impressive paper, not previously published, was rejected by four well-known periodicals as "of no importance" (1960b).

II

Thought-style and Thought-collective

Fleck's theory of 'thought-style' and 'thought-collective' represents an original amalgamation of philosophical and sociological theories of knowledge. Its philosophical side maintains that all empirical discoveries of 'scientific facts' contain, and depend upon, non-empirical elements — but these non-empirical elements are intellectual products, subjective fictions of the mind, that is they are 'styles of thought'. Fleck is thus an advocate of a radical nominalism and constructivism.

He joins this epistemological standpoint to a sociological argument which allows him to secure himself against the question: if that is the case epistemologically, why don't the scientists become conscious of this construed fictitiousness of their conceptual apparatus? Styles of thought, Fleck argues in reply, are collective phenomena, the results of socialization processes of closed communities. They are not visible to the members of such communities.

Three characteristics can be taken to designate his approach.

Fleck sociologizes the theory of knowledge. The collective character of scientific work determines not only the elaboration of new ideas but also their genesis. Thereby Fleck assumes an extremely anti-individualistic standpoint: a new idea, a new thought, can never be traced back to a particular individual. It arises, rather, from collective cooperation whose medium is communication of thought. Fleck distinguishes the intra-collective communication of thought, which continually confirms and thus stabilizes the thought-style of the collective, from the inter-collective communication, which brings in those influences from other areas which change the thought-
style. And this is so because individuals always belong simultaneously to different thought collectives. Therefore each member of a collective interprets a thought differently: understanding one another includes misunderstanding one another in a certain sense. Fleck emphasizes the role of language as the most important element of scientific communication, which he also, methodologically speaking, investigates sociologically; Fleck was probably the first to use comparative content analysis to investigate scientific literature. However, Fleck does not regard verbal communication alone to be sufficient to secure a stylistically adequate ('stylized') cooperation within the thought-collective. In his view, besides this, a practical experience, which cannot be formulated explicitly, is also necessary.

Fleck historicizes the theory of knowledge. The conception of scientific development as a cumulative and progressive process is replaced by that of development as a continual changing of the style of thought. Thought-styles are historically developed, sociologically conditioned and mutually interacting phenomena. The dynamic implied in this structure becomes the force of scientific development. 'Development' is not understood as progressive or evolutionary. Rather it means only that attention can be directed to new problems. At the same time, other problems lose their stylistically conformed character. They become irrelevant, they are no longer 'perceivable'. As new knowledge emerges, old is lost. Unlike with Kuhn, this development does not occur in leaps and bounds; rather the presuppositions of knowledge change continuously, mostly without the awareness of the scientists involved. However, certain 'proto-ideas' can 'survive' even throughout longer periods of time: they serve a number of thought-collectives and generations of scientists as heuristic guidelines. They live on because they are taken up and used again by each of the newly developed thought-collectives. They are reinterpreted within the framework of the new thought-style and in accordance with its changed presuppositions. Old and new merge. Thus we can speak of continuity in the succession of styles of thought.

Within this framework, Fleck reformulates the concept of a scientific fact. A fact is no longer something given independently of scientific activity, because the socially conditioned and historically developed thought-style forces itself on scientific knowledge, the presuppositions of which are actively posited by the thought-collective. What scientists are looking for, on the other hand, are the passive linkages which arise from these active postulations or 'settings'. Once certain presuppositions are chosen and accepted, the
collective can no longer decide on the passive linkages implied by them, but rather experiences them, perceives them, as 'laws of nature'. The scientist can relate to such a perception only 'passively' or 'reactively'. Fleck describes the process of knowledge of a fact as the development of such 'avisos of resistance', notices which restrict the freedom of the scientist to make arbitrary stipulations. If a thought-collective wants to integrate such a resistance into its previously developed system of thought or opinion, it develops it so that it becomes an increasingly clear constraint on thought — a 'force of thought' — and finally an immediately perceivable Gestalt. Although it is sensed as something 'objectively given', a fact is determined by the respective thought-collective which knows it through an actual thought-style.

III

Fleck's monograph was originally published in 1935 by Benno Schwabe, a Swiss publisher. It had the sub-title *Einführung in die Lehre vom Denkstil und Denkkollektiv*. In 1980 a second edition of the German text appeared from Suhrkamp (Frankfurt-a-M.); an English translation (without the sub-title) had already been published in 1979 by the University of Chicago Press. Now with the present volume of materials, Ludwik Fleck's other writings on the philosophy of science are for the most part available for the first time in English.

Reception

In 1961, the year Fleck died, a book went to press in Chicago which, by its phenomenal influence, was to prove the vitality and actuality of Fleck's thought: Thomas Kuhn's work on *The Structure of Scientific Revolutions*. In the Foreword to the English translation of Fleck's monograph, Kuhn writes that he finds many of his ideas were anticipated by Fleck, and indeed that it may be that the sociological aspects of his own study go back to his reading of Fleck. Perhaps we may see the structures of the thought-collective and thought-style pointed out by Fleck as models for the Kuhnian analogues, even though considerable differences can be observed too. In spite of all the intensive work on Kuhn's positions, their origins and affinities, yet their relationship to Fleck was not dealt with for a long time, perhaps because there is no reference to Fleck in the body of the text itself. Nonetheless, later on Kuhn's reference to Fleck in the opening acknowledgments of his book led several times to a reading of Fleck by a few alert readers.
The first to draw attention to Fleck, also stimulated by Kuhn's reference, was Baldamus, at Birmingham, in 1966. However, his paper, 'The Role of Discoveries in Social Sciences', did not appear in print until 6 years later (Baldamus 1972). Baldamus was able to awaken wider interest in Fleck only much later with his article on 'Ludwik Fleck and the Development of the Sociology of Science' (Baldamus 1977), and with his book The Structure of Sociological Inference (1976). Baldamus emphasized Fleck's conception of the evolution of thought by 'mistakes and accidents' as particularly fruitful for social science. In a later paper Baldamus showed how Fleck's concepts of exoteric and esoteric knowledge led him to completely new insights in the theory of scientific development (Baldamus 1979). A paper by Baldamus and Schnelle took up the discussion by Fleck of the historical endurance of scientific conceptions through processes of change in new styles of thought (Schnelle and Baldamus 1978).

IV

Difficulties with Translating

Fleck's language was unusual: although very fluent and innovative, it was not always grammatically correct. This is true for his publications in both German and Polish. No doubt, German, or rather an educated Austro-German dialect, was Fleck's second mother tongue next to Polish. These circumstances alone constitute a heavy burden for any translator who seeks to preserve the originality of Fleck's language. Translating into the English, there are problems of two different kinds. On the one hand Fleck uses expressions such as 'Sinn', 'erkennen', etc. There are no expressions in the English language which were equivalent to their understanding in the middle-European tradition. On the other hand Fleck invented some expressions himself, such as 'Denkkollektiv', 'Denkverkehr', 'Denkwang', 'Sinnssehen', 'Widerstandsaviso' etc. The translator of such expressions, which had been invented (or used with an innovative connotation), must try also to be innovative, and in a similar way. This is of course nearly impossible. A reader knowing that the text is a translation, not an original manuscript, will usually reject such suggestions as not being adequate. However, when in doubt, we think a translation of an expression should sound awkward rather than go too far from the structures contained in the original.

Unfortunately to our way of thinking, the English translation of Fleck's
monograph (Fleck 1979) is somewhat imprecise, particularly in its formulation of sociological concepts. For example, Fleck’s term ‘soziales Gedächtnis’ has been translated simply as ‘society’ (1979: 2). There seems to be no reason why ‘social memory’ would not have been adequate. Although Fleck seldom uses this particular term, it exemplifies the shortcomings of this edition: it fails to bring out adequately the aspect of social activity on the part of individuals bound up in social interactions. Another example: ‘Bedingtheit’ is better translated as ‘conditionality’ or ‘conditionedness’ rather than ‘dependence’ (1979: 9, although later on the expression ‘conditioning’ is also used). Similarly, ‘Koppelungen’ are ‘linkages’ (or ‘couplings’) rather than ‘associations’ or ‘connections’.

In view of this disagreement about translation, it seems useful to present here a glossary of the most important expressions employed by Fleck in Polish and German, together with our choice for their corresponding English translation:

<table>
<thead>
<tr>
<th>German</th>
<th>Polish</th>
<th>English</th>
</tr>
</thead>
<tbody>
<tr>
<td>Bedingtheit</td>
<td>uwarunkowanie</td>
<td>conditionality, conditionedness</td>
</tr>
<tr>
<td>Beharrungstendenz</td>
<td>–</td>
<td>tendency to persist</td>
</tr>
<tr>
<td>Bereitschaft</td>
<td>gotowość, przygotowanie</td>
<td>preparedness</td>
</tr>
<tr>
<td>Bestärkung</td>
<td>wmościenie, potegowanie</td>
<td>reinforcement</td>
</tr>
<tr>
<td>Denkkollektiv</td>
<td>kolektyw myśliowy</td>
<td>thought-collective</td>
</tr>
<tr>
<td>vs. Denkgemeinschaft</td>
<td>vs. zespół myśliowy</td>
<td>vs. thought-community</td>
</tr>
<tr>
<td>Denkstil</td>
<td>styl myśliowy</td>
<td>thought-style, style of thought</td>
</tr>
<tr>
<td>– an den Denkstil gebunden</td>
<td>– związany z stylem myśliowym</td>
<td>– tied to the thought-style</td>
</tr>
<tr>
<td>Denkstilwechsel</td>
<td>zmiana stylu myśliowego</td>
<td>style shift</td>
</tr>
<tr>
<td>Denkverkehr</td>
<td>poruszenie myśli, krążenie myśli</td>
<td>communication of thought</td>
</tr>
<tr>
<td>Denkzwang</td>
<td>przymus myśliowy</td>
<td>constraint on thought</td>
</tr>
<tr>
<td>Einweihung</td>
<td>wijemniczenie</td>
<td>initiation</td>
</tr>
<tr>
<td>Entdeckung</td>
<td>odkrycie</td>
<td>discovery</td>
</tr>
<tr>
<td>Entstehung</td>
<td>powstanie</td>
<td>emergence</td>
</tr>
<tr>
<td>Entwicklung</td>
<td>rozwój</td>
<td>development (in the sense of unfolding)</td>
</tr>
<tr>
<td>erfahren</td>
<td>doświadczalny</td>
<td>to experience</td>
</tr>
<tr>
<td>erkennen</td>
<td>poznawać</td>
<td>to cognize</td>
</tr>
<tr>
<td>vs. Wissen</td>
<td>vs. wiedza</td>
<td>vs. knowledge</td>
</tr>
</tbody>
</table>
**INTRODUCTION**

<table>
<thead>
<tr>
<th>German</th>
<th>Polish</th>
<th>English</th>
</tr>
</thead>
<tbody>
<tr>
<td>gerichtet</td>
<td>skierowany</td>
<td>directed</td>
</tr>
<tr>
<td>Gerichtetheit</td>
<td>–</td>
<td>directionality</td>
</tr>
<tr>
<td>Gestaltsehen</td>
<td>widzenie postaci,</td>
<td>seeing of Gestalts</td>
</tr>
<tr>
<td>Harmonie der Täuschungen</td>
<td>harmonia złożeń</td>
<td>harmony of deception</td>
</tr>
<tr>
<td>Irrungen</td>
<td>pomyski</td>
<td></td>
</tr>
<tr>
<td>Kollektivwunsch</td>
<td>–</td>
<td>collective wish</td>
</tr>
<tr>
<td>Koppelung</td>
<td>sprzężenie</td>
<td>linkage, coupling</td>
</tr>
<tr>
<td>– aktiv, passiv</td>
<td>– czynny, bierny</td>
<td>– active, passive</td>
</tr>
<tr>
<td>Kreis, esoterischer</td>
<td>krąg, ezoteryczny</td>
<td>circle, esoteric</td>
</tr>
<tr>
<td>– exoterischer</td>
<td>– egzoteryczny</td>
<td>– exoteric</td>
</tr>
<tr>
<td>Meinungssystem</td>
<td>system poglądów</td>
<td>system of opinion</td>
</tr>
<tr>
<td>sehen vs. schauen</td>
<td>widzieć vs. patrzeć</td>
<td>to see vs. to look</td>
</tr>
<tr>
<td>Sinnsehen</td>
<td>–</td>
<td>the seeing of Sinn</td>
</tr>
<tr>
<td>stilgemäß</td>
<td>stylowe</td>
<td>style-adequate</td>
</tr>
<tr>
<td>stilvoll</td>
<td></td>
<td>stylish</td>
</tr>
<tr>
<td>Stimmung</td>
<td>nastrój</td>
<td>mood</td>
</tr>
<tr>
<td>– intellektuelle</td>
<td>– intelektualny</td>
<td>– intellectual</td>
</tr>
<tr>
<td>Tatsache</td>
<td>fakt</td>
<td>fact</td>
</tr>
<tr>
<td>Urdee</td>
<td>pramysł, praida</td>
<td>proto-idea</td>
</tr>
<tr>
<td>Voraussetzung</td>
<td>przypuszczenie</td>
<td>presupposition</td>
</tr>
<tr>
<td>Wandlung (des Denkstils)</td>
<td>transformacja (stylu myślowego)</td>
<td>transformation (of thought-style)</td>
</tr>
<tr>
<td>Widerstandsansivo</td>
<td>awizo oporu</td>
<td>advice of resistance, recalcitrance-advice</td>
</tr>
<tr>
<td>Wunschtraumerfüllung</td>
<td>–</td>
<td>fulfillment of wishful thinking, of a wish-dream or of a pipe-dream</td>
</tr>
<tr>
<td>zwangsläufig</td>
<td>przymusowy</td>
<td>inevitably-constrainedly</td>
</tr>
</tbody>
</table>

**Ludwik Fleck – a Philosopher or a Sociologist?**

At the beginning of his reception Fleck was first of all regarded as a classical figure in the sociology of science and knowledge, as one of the founding fathers of this discipline. It was only in recent years that he was also seen as a philosopher, as the author of a philosophical theory of reality. Reading his papers presented in this volume, it seems to us to be irrelevant which is considered the better choice: Fleck’s approach is unique and touches both disciplines at the same time.
The essays on Ludwik Fleck’s theory of knowledge and science which have been gathered in this book are the results of two scientific meetings organized to honor him: the first took place during three days in 1981 in Hamburg. This ‘Colloquium Ludwik Fleck’ was organized by Lothar Schäfer and Thomas Schnelle within their Fleck research project. The second was a symposium arranged by Robert S. Cohen at the Wissenschaftskolleg zu (West) Berlin in 1984. There it was possible to initiate a discussion with physicians concerning Fleck’s philosophical reflections on medical themes (see, e.g., the papers by Zalc and Löwy).

These two meetings marked a turning-point in the reception of Fleck as a philosopher and sociologist. We are justified in saying that, despite some considerations by Chwistek and Dambska (refer to pp. 458–9), as long as Fleck was still alive there was no adequate reception of his work at all. In the thirties there was only a single review of his monograph in a philosophical journal (Lauriers 1937). All other reviews, about 19 altogether, appeared in medical or popular journals or newspapers.

Independent of the initial recovery of interest in Fleck by Baldamus and Schnelle, there was another and highly significant rediscovery of his monograph by Thaddeus J. Trenn and Robert K. Merton (cf. Trenn 1976; Merton 1977, Trenn and Merton 1979). Their effort led to the English translation of Fleck’s monograph of 1935.

In West Germany, it was Lothar Schäfer of Hamburg who mentioned Fleck first. He was then (1977) mainly interested in the implications of Fleck’s work upon the understanding of the dynamics of theories. Schäfer also was concerned to show the connection between Fleck’s case studies in medical history and Fleck’s conception of collective knowledge (cf. Schäfer 1982). When Thomas Schnelle, the student of Baldamus, joined Lothar Schäfer for the Fleck research project, their first result was republication of Fleck’s monograph in the German language (1980). At that time, in East Germany, Dieter Wittich of Leipzig prepared a Marxist commentary and critique of Fleck’s historical sociology of science (Wittich 1978).

In Poland also it was only recently that the philosophical sources of Fleck’s work were discussed: Zdzisław Cackowski edited a biographical essay by Irena Rubaszko (Rubaszko 1979) as well as a methodological article by Ewa Pirozniak (Pirozniak 1979), and, with Maria Tuszkiewicz, he is preparing a Polish translation of Fleck’s monograph. But in Poland, despite neglect in the literature, Fleck seems to have been somebody one knew about: Robert S. Cohen recalls that both Kazimierz Ajdukiewicz and Adam Schaff discussed
Fleck with him in the early 1960’s, and Stanislaus Lem wrote about him (Lem 1968).

Now interest has grown rapidly. An Italian translation exists (1983), and a French one is in preparation. In the course of our conferences there were some 70 scholars participating — from Great Britain, the United States, the Netherlands, France, Israel, Poland, Italy, the Federal Republic of Germany and the German Democratic Republic. We are glad that we have been able to publish most of the contributions in this book.

VI

Many persons have helped throughout the research on Fleck’s biography, and with publication of his writings. Among them first of all his widow Mrs Ernestina Fleck, and his son Mr Ryszard Arie Fleck (both in Petah Tikwa, Israel) should be mentioned. Together with them, Professor Marcus A. Klingberg (Tel-Aviv), administrator of Fleck’s will and papers, helped with our publication. Further we would also thank: Danuta Borecka (Warsaw), G. Dickmann (Tel-Aviv†), Władysław Kunicki-Goldfinger (Warsaw), Eugen Kogon (Falkenstein, West Germany), Hugon Kowarzyk (Wrocław), Irene Lille (Paris), Teres Małeckaw (Warsaw), Janina Opieńska-Blauth (Lublin), J. Parnas (Copenhagen), Irena Rubaszko (Lublin), Izabella and Piotr Sałustowicz (Cracow and Bielefeld), W. Stein (Lublin†), Mieczysław Subotowicz (Lublin), Maria Tuszkiewicz (Lublin). Dr Felix Lachman (London) was of immense help with the preparation of translations from the Polish. And the VW Stiftung generously subsidized most of the costs of the translations.

VII

The Monograph of 1935: Genesis and Development of a Scientific Fact

In his monograph of 1935, Fleck develops his approach from the perspective of a determination of the concept of a fact, as the title clearly states; he does this theoretically and empirically with an extensive case study of the historical development of the scientific concept of syphilis. Both aspects are given equal space; both are dealt with in two parts. First of all, Fleck investigates the historical genesis of the concept of syphilis, which he pursues back into the Middle Ages. The theoretical reflections evaluating this presentation deal above all with the historical conditionedness of knowledge. The inten-
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tion is to demonstrate to what extent even 'scientific facts' are dependent on history and culture. Then Fleck investigates again as a case study the (at the time) latest historical phase in the development of the syphilis concept: the research of the physician Wassermann and his co-workers at the beginning of this century in Berlin, which led to the first diagnostic proof procedure for syphilis (the so-called 'Wassermann reaction'). In the evaluation of this part of his empirical presentation Fleck concentrates on the social conditionedness of knowledge. Here, he points out the collective nature of research and its interlacing with the social network of the scientists who carry it out. However, the separation suggested here of the historical and social grounding of knowledge cannot be held on to and above all is not by Fleck himself. They mutually condition each other and form together the basis upon which 'reality', 'truth', and 'fact' are reformulated. In the following presentation we shall also consider the two articles of Fleck's which were published at the same time as the monograph (1935d, 1936) – which are also published in this volume – since they were obviously composed in close connection with the monograph.

Thought-collective and Thought-style

Knowledge cannot be considered cut off from the people who create and possess it. All activities devoted to attaining and preserving knowledge are bound to social interactions among persons. Along with empirical and speculative elements of knowledge, above all sociological elements must be considered: to be concrete, the common convictions developing in the communication of thought of the scientists with one another.

Fleck coins the terms thought-collective and thought-style as conceptual instruments with which to grasp this property of knowledge. The thought-collective denotes the social unit of the community of scientists in a field. With this, the traditional picture of science as the result of individual achievements is corrected from the start. Science is, on the contrary, tied to the cooperation of people. Thus he ascertained in the analysis of the historical development of the discovery of the Wassermann reaction, which after all took several years after 1906, that it is by no means Wassermann to whom this achievement can be attributed. Rather, he only stood at the head of a research group which was dealing with syphilis. The members of the group participated in the work with different contributions. One member could build on the contributions of the other – the separation of the individual
contribution is completely impossible even after a short time.

Fleck defined the thought-collective explicitly as:

a community of persons mutually exchanging ideas or maintaining intellectual interaction(1) we will find by implication that it also provides the special 'carrier' for the historical development of any field of thought as well as for the given stock of knowledge and level of culture. (1979: 39)

Since the "intellectual interaction" is also interpreted by Fleck as "mutual understanding" (1936: 84–85), the thought-collectives thus correspond to the social differentiation of people according to their ability and inability to understand one another.

With the concept of thought-style Fleck attempts to grasp, on the one hand, the intellectual presuppositions upon which the collective erects its edifice of knowledge and, on the other hand, at the same time the intellectual unity of the stock of knowledge developed by a collective. The thought-style is the precondition of collective work in as much as the communally accepted norms are formulated in it — not only about knowledge already examined and taken as secure but also about the methods used in examination, about the criteria for judging what is to be taken as scientifically proven and what as unproven. Furthermore, the thought-style structures the foundation of the work practice of a thought-collective; that is, both what can and must be considered as a scientific problem, and how this problem is to be dealt with.

But this does not mean that the members of the collective are conscious of the determination of their interest by the thought-style (1935d: 76–78). What the thought-style dictates is accepted as a matter of course by the members of the collective. But in fact it is never more than a conglomerate of shared convictions, on the basis of which views are developed which conform to them, i.e. 'stylized' views, which when pursued far enough take the shape of an internally consistent "system of opinion".

Knowledge is thus according to Fleck only possible if postulated assumptions about the object are presupposed. These presuppositions of thought cannot be justified on the basis of the knowledge acquired with their help — they can only be made understandable as the historical product of an active thought-collective, as the mirroring of a reality it has itself created.
INTRODUCTION

VIII

Seeing of Gestalts

Fleck arrives at an interesting interpretation of the Gestalt psychology developed in the early decades of this century by pointing out the collective nature of science precisely in the seeing of Gestalts. For only those who appropriate the thought-style of a collective not only theoretically and cognitively but also in terms of practical experience are in the position to 'see' in the process of knowledge. The inexperienced person, on the other hand, cannot 'see'—he 'looks'. He perceives at first only a 'chaos'. The picture can be interpreted this way or that way. There is no base from which the articulation of the perceived could proceed. This is opened up only to members of the collective, to those initiated into the style of thought. Their thought-style allows them to set fixed points in what is to be perceived. Only if a collective provides firm structures of thought can the picture take on order and contour. In order to advance from "vague initial looking" to "developed direct seeing of Gestalts" one must learn to see (cf. also Fleck's example of the perception of microscopic specimens, 1935d: 63-67). Learning to observe, means to Fleck making the appropriate thought-style one's own. The thought-style takes care that "a readiness for stylized (that is, directed and restricted) sensation" (1979: 84) arises. For Fleck there can be no observation free of presuppositions and prejudices. It is always dependent on the respective thought-style and thought-collective and thus on the historical and social conditions. 'Thought-style' and 'seeing' are therefore practically defined in terms of one another by Fleck.

Talk about 'good' and 'bad' observation is thus meaningless, only agreement with the respective thought-style can be ascertained. The thought-style determines which contours are interpreted into perception, that is, which direction the necessary abstraction takes. Only in this way can a relationship between defined units in the perceived object be ascertained as a 'fact'—it must be experienced as a message, an aviso of resistance to the Gestalt-less and arbitrary looking. Perceiving is thus a directed activity, which forces its Gestalt on the perceiver. Fleck characterizes stylized seeing as constraint on thought. He illustrates these relationships vividly and repeatedly with practical examples from his own bacteriological studies. It becomes clear in the examples that the increasing ability to perceive necessarily involves a mounting loss—namely the loss of the ability to see at all things which contradict the thought structures.
What Is 'Scientific Progress'?

For Fleck scientific research is only imaginable as organized in collectives. However, he does not rest at this insight into the significance of the collective nature of research, rather he goes a step further: it is only through the conception of scientific work as a thought-collective that it can be made understandable that concrete results arise from research endeavors. For, in general, the hypotheses proposed at the beginning of research activity do not lead to results; the original goals are not achieved. Thus Wassermann and his co-workers at the beginning of their research were above all looking for a diagnostic demonstration procedure for syphilis with the help of which antigens could be demonstrated in luetic organs and blood. In spite of initial successes, it turned out to be impossible to accomplish the program. Only with the failure of this notion did the group begin to concentrate on the demonstration of antibodies. This was successful at first in only 15–20% of the cases and was therefore felt to be uninteresting. Decisive progress occurred with the development of the original, highly unspecified experiments into procedures which delivered dependable results in at least 70–90% of the cases. But it is impossible to reconstruct this development. It has even disappeared from the consciousness and memory of the scientists engaged in the research. A chronicle would only be possible if one could include the large number of unnamed co-workers in the investigation, who achieved an adaptation of test and diagnosis by trying out technical tricks, making minor changes in the reaction conditions, and by changing the interpretation of the data.

Such a development can only be achieved by a collective whose members working on a unified foundation try out modifications of this very foundation in individual directions. Unsuccessful endeavors are predominant, and the group continues trying out new modifications, working only on those which promise to be successful. Fleck therefore denotes the course of research as a zig-zag determined by accidents, false paths, and mistakes. Epistemologically, the original foundations of work are slowly transformed for the scientists — in retrospect, however, the collective knows nothing of this:

The following state of affairs [facts] is therefore firmly established and can be regarded as a paradigm of many discoveries. From false assumptions and irreproducible initial experiments an important discovery has resulted after many errors and detours. The principal actors in the drama cannot tell us how it happened, for they rationalize and idealize the development. Some among the eyewitnesses talk about a lucky accident, and
the well-disposed about the intuition of a genius. It is quite clear that the claims of both parties are of no scientific value. (1979: 76)

The shifts in the self-conceived contents of research occur in the collective unnoticeably for the individual. This goes so far, that according to the experimental descriptions of the Wassermann group their original experiments with the still high rate of unspecific results turn out to be irreproducible. Even the researchers who originally took part in them were later unable to repeat them. On the basis of the results found and their current theoretical explanation, the research endeavor appears to be a straight line from the first formulation of the problem to the tentative completion of research.

With this approach Fleck arrived at an entirely new conception of 'progress' to which the character of 'getting better' is usually attached. For Fleck, on the other hand, the progress of knowledge is the collective further development of the thought-style through which the presuppositions projected on the object of knowledge are shifted.

This is not 'progress' in the usual sense of the word because there can be no level from which a particular thought-style could be evaluated as more valuable than another. Knowledge differs according to the different thought-styles. In the transformation of a thought-style the point is not to be able to know more (or less). With the shifting of the presuppositions, knowledge changes: new knowledge is acquired — other knowledge can no longer be 'known' if the further development has so to speak removed the ground from under its feet.

X

The Historically Embedded Character of Knowledge

Fleck relativizes the understanding of science and fact not only through the demonstration of the sociological aspects of scientific knowledge. A historical component is added: science can never be understood cut off from its history of development. By his investigation of the genesis of the concept of syphilis Fleck can demonstrate that the conceptions of modern natural science, too, are historically developed products which would not be comprehensible at all without recourse to their development. Again with examples from his own bacteriological work Fleck shows that empirical, illustrative material alone does not allow the scientist to recreate the demarcations of the objects in his discipline. For these are always the products of the interpretations of past
generations of scientists and past epochs of knowledge.

Fleck introduces the concept of “proto-idea” or “pre-idea” as an instrument to grasp the embedded character of notions in their historical context of emergence. He designates with this term notions arising in the distant past which continue to exist in spite of all changes of thought-styles. When knowledge develops further in the course of time, that is when the presuppositions of the knowledge of a thought-collective change, they do not as a rule change completely but rather by mutations. In as much as a new epoch joins on to the stock of knowledge of the previous epoch, such conceptions lose their original contexts of emergence and justification. They are interpreted differently by the new epoch in accordance with its thought-style. The new value which they have for each epoch lies in the fact that their content is always being newly comprehended, so that they take on a heuristic function regulating research:

Proto-ideas must be regarded as developmental rudiments of modern theories and as originating from a socio-cognitive foundation. (1979: 25)

The historical connection of human knowledge can, according to Fleck, be demonstrated even where historically separated thought-styles cannot be compared with one another. Even individual isolated concepts contain in the specific coloring of their thought-style something of their original ‘primitive character’. [On the question of the survival of ‘old’ knowledge within the framework of ‘new’ knowledge, cf. Schnelle/Baldamus, 1978.]

Fleck mentions as examples of such proto-ideas the atomism of classical antiquity, the idea of elements and of chemical composition, and the principle of conservation of matter, which were all formulated long before their modern versions, and as proto-ideas influenced research in natural science. But he illustrates this relationship above all by retracing descriptively the history of the concept of syphilis over more than four hundred years. Four different strains of thought are amalgamated in the course of development up to the contemporary disease concept of syphilis, but they still live on as suggestive forces in modern medicine. One such notion, which proved to be especially important was the notion of the corruption of the blood of syphilitic patients taken from the doctrine of the mixture of humors. Although Wassermann and his group at first had a different goal, this notion prevailed in the course of their work. The ancient social wish was fulfilled: the proof of the ‘corrupted’ blood of the syphilis victim.
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XI

Knowledge and Social Context

Alongside the historical conditionedness of knowledge, that is, its vertical embeddedness in cultural history, Fleck also demonstrates the complementary property: the horizontal embeddedness of knowledge. This is the determination of knowledge by social factors. Expectations are attached to science from without, which it has to consider and deal with. This should, however, not be understood in the sense of a systems-theoretical control by factors external to science. Just as they are embedded in the history of science, so too are scientists elements of society. They are thus subject to the prevailing 'social moods'. Only an appropriate 'social mood' is at all sufficient to motivate such intensive research endeavors as those of the Wassermann group. The syphilis disease still represented at that time a problem that due to its characterization as the 'pleasure plague' (Lustseuche), received great social attention. It was for this reason that Wassermann's researches were initiated on the part of the state. The intention was to secure the German contribution to syphilis research. Thus as a further social motive, the competition of nations must be included. Tuberculosis on the other hand, which claimed many more victims at the time, was not made the subject of elaborate research work; it was only the 'romantic' but not the 'dishonoring' disease.

The Tenacity of Systems of Opinion

With the concepts of thought-collective and thought-style Fleck believed he had formulated conceptions in which the collective nature of science, its social as well as its historical character, can be grasped. Fleck then turned to a more exact investigation of those processes which allowed thought-collective and thought-style to become stable social units with their own weight. He found them first in the structure of the collective.

The core of a thought-collective is made up of a relatively small group of specialists of the particular field. This Fleck designates as the esoteric circle of the collective. It is surrounded by a larger, exoteric circle by which educated laymen partake of scientific knowledge. The two groups are joined in a specific manner: the basis of exoteric knowledge lies in the trust in the competence of the specialists — it simplifies, leaves out details, generalizes in order to be comprehensible to laymen. On the other hand, however, esoteric knowledge also depends on exoteric knowledge, which confronts it as popular
public opinion before which it must legitimate itself. This kind of mutual dependency of specialists and educated public acts as a stabilizing element on thought-collective and thought-style.

A further aspect was seen by Fleck in the training of the next generation of scientists, the importance of which he indicates for the first time here. Its task is namely to manage the 'introduction' of students into the thought-style of the collective. Only those who have 'understood' its basic presuppositions can participate in its knowledge. But learning to 'understand' means anything but a critical examination of the current stock of knowledge. On the contrary, the presuppositions of the thought-style must simply be accepted, taken for granted, by the student. He must make them his own as irrefutable 'articles of faith' as did his teachers. The training is thus 'pure authoritarian thought suggestion', a "gentle constraint" to the conceptual schemes of the collective. Fleck demonstrates this constraint empirically in textbooks. He also blazes new trails methodologically in so far as he employs — probably for the first time — comparative content analysis as a sociological research instrument. In a comparison of the theorems in a textbook of serology (which had appeared in 1907 and was thus already out of date at the beginning of the thirties) with the current state of serological research and conceptual development, the constituents presupposed by the earlier serological collective were made visible.

The theoretical acceptance of such postulates is not itself sufficient to enable someone actively to take part in the work of the esoteric core of a collective. To complete the 'initiation' into a thought-style, it is necessary, furthermore, to acquire practical experience. Fleck here tries to take the circumstance into account, that it is completely impossible to carry out practical research merely by following the instructions of a textbook or the descriptions of an experiment in a research report. It is apparent that a number of additional prerequisites of a pragmatic character, which cannot be expressed in words, play an equally important role. Just as occurs in the apprenticeship of every craftsman, these skills can only be acquired gradually by practical experience under the direction of a 'master'.

Not only the relationships of specialists to laymen, of teachers to pupils, but also those among 'equals' in a thought-collective contribute to the development of stabilizing relations of dependency. The scientists of a discipline mutually constrain each other into a "certain solidarity of thought in the service of a superindividual idea", which allows them to scrutinize one another for possible deviations. Thus,
the general structure of a thought-collective entails *that the communication of thoughts within the collective, irrespective of content or logical justification, should lead for sociological reasons to the reinforcement of the thought structure*. Trust in the initiated, their dependence upon public opinion, intellectual solidarity between equals in the service of the same idea, are parallel social forces which create a special shared mood and, to an ever-increasing extent, impart solidity and conformity of style to these thought structures. (1979: 106)

Stabilizing themselves by such processes and completing themselves as social units, thought-collectives build up systems of opinion which claim to be able comprehensively to explain their field of application. The stronger such styles of thought are, the more suggestive is their power over the members of the collective. If they are able to acquire so much ‘suggestive force’ that they can disguise their dependency on presuppositions, then they construct a *harmony of deceptions*. Contradictions to the propositions of the system of opinions seem to be excluded. Events which don’t fit the system remain unseen or are even consciously ignored. But the more common case is that observations are interpretatively forced to fit into the system of opinion. Empirical evidence of this kind is found in the scientific literature. Illustrations, for instance, never agree with the observational possibilities of the reader; it is always the case that the ‘important’ details for the respective thought-style are especially highlighted, and those that are ‘unimportant’ are most often simply left out. They suggest objective relationships, which are supposed to underline the claims to validity of the system of opinions. Parallel to the stability of the collective, thought-style and system of opinion develop their *tenacity* against everything that contradicts them:

The *tenacity* of systems of opinion shows us that, to some extent, they must be regarded as units, as independent, style-permeated structures. (1979: 38)

XII

*The Transformation of Thought-styles*

With such a variety of structural mechanisms tending towards stabilization and tenacity, how does it come about that thought-styles develop further at all, that scientific knowledge develops? Every scientist alongside his special thought-collective belongs at least to the exoteric aggregate collective of everyday life; but as a rule he will also be a member of other scientific and non-scientific thought-collectives. These competing orientations of the indi-
viduals cannot simply be dazzled or blinded in scientific work. On the contrary, they enter into the communication of thought of the collective (1979: 109ff.; but especially 1936: this volume). In this way mechanisms come into play which counteract the ossification of thought-collective systems of opinion. In the communication of thought, as the collective research work has already been characterized, the thoughts and ideas which arise in the course of work ‘circulate’ from individual to individual. Every ‘receiver’ of such thoughts in the collective understands them somewhat differently, joins different associations to them, and thus interprets them with a diverging sense.

As Schäfer (1982) has stressed, Fleck attributes a function to language which goes considerably beyond communicability: “Fleck recognizes the importance of language as an institution...which has a positive function for the development of science even in the ‘misunderstanding’ (= shift of meaning) which occurs in every communication” (cf. 1936).

That which I express is always different from that which I think. In the same way, that which is understood is always different from what I have said... (1934d: 205)

Science is thus continually subject to changes whose tempo depends on the given balance of power between the forces of tenacity and those pushing onward. Fleck shows how, from this dynamic, historical phases of the dominance of epochmaking thought-styles arise which differ pointedly from their predecessors and successors. He provides an illuminating demonstration of such differences of the thought-styles of different epochs by a comparison of their textbooks. A mutual comprehension among supporters of different styles of thought is impossible, for their concepts — even when they sound the same — have no common ground. They are untranslatable and are meaningful only within their own stylized complex of meanings.

In contrast to the opinion common since Kuhn (1962), Fleck does not consider the transition between such epochs of differing thought-styles as ‘revolutions’, as clear divisions. Rather he sees them as occurring gradually in a succession of many small steps often not even perceived by the participants.

Journal and Handbook Science

Fleck’s analyses of the structure of modern science are grounded on the
sociological instruments which he had developed. These allowed him to make a number of further observations, some of which have been taken up and made known by others. Some of them, however, still await evaluation.

One example of this kind is the tension between the tendency to stabilization implicit in the thought-collective and the further development of the thought-style. Fleck investigates this problem empirically by analyzing the structure of scientific literature, which he divided into journal and handbook literature. The handbook science formulates the genuine core of the system of opinions of a discipline. It is impersonal, presents itself as secure and substantiated knowledge, which is to be taken as the basis for all further work of the thought-collective. The journal literature, on the other hand, is tentative; it is personally formulated as knowledge which has yet to be confirmed by the collective.

All journal science strives towards the handbook. It wants to be accepted in the collective and to be counted in the basic corpus of its discipline. It can increase its chances by connecting up plainly to the previous state of handbook knowledge, that is, by once again confirming it.

The handbook does not however originate as a mere compilation of the journal literature. The process of systematizing is located in the "esoteric communication of thought". It originates during discussion among the experts, through mutual agreement and mutual misunderstanding, through mutual concessions and mutual incitement to obstinacy. When two ideas conflict with each other, all the forces of demagogy are activated. And it is almost always a third idea that emerges triumphant: one woven from esoteric, alien-collective, and controversial strands. (1979: 120)

What Is a 'Fact'? The Reformulation of the Concepts of Truth and Reality

The concepts of thought-collective and thought-style represent the foundations of Fleck's philosophy of science. With these he hopes to have formulated categories with which those factors determining science which are grounded in the social interaction of individuals can be caught up. At the same time the historical and social embeddedness of knowledge is to be taken into consideration. Furthermore, Fleck claims here to have provided categories of general validity, with the help of which knowledge having other social foundations can be as adequately investigated as can scientific knowledge.

On this basis a number of phenomena connected with science appear in a
new perspective. The role of textbooks, training, or even language as a means of communication, which appear in a completely new light, has already been pointed out.

The most important of these innovations, the reformulation of the concept of fact, must now be taken up — and with it the closely connected concepts of ‘truth’ and ‘reality’.

Fleck conceives the process of knowledge as polar. On the one hand, because it is based on presuppositions, it projects actively that which is to be known into the object. Fleck calls these thought-style specific stipulations *active linkages*. They are not understood as *a priori* categories of the understanding, but rather as historically and socially developed products. On the other hand, on this basis the perceptions are formed which then take on concrete shape vis-à-vis the knowing subject. That is, the autonomous laws implicit in the active linkages produce interconnections, which follow necessarily from them and which Fleck accordingly calls *passive linkages*. They oppose the arbitrary freedom of thought and offer resistance to knowledge in as much as they form themselves into concrete *Gestalts* in thought. Understood in this manner, to ‘know’ means to ascertain the ‘inevitable results’ of given active linkages. Within the framework of a particular thought-style there can be no qualitative alternative to them — Fleck therefore speaks of a constraint on thought, to which the member of a collective is unavoidably subjected.

It is the constraint of the concrete *Gestalt*, its resistance to non-stylized, arbitrary perception, that the researcher is looking for in the act of knowing. The empirical reference of the natural sciences is not denied by Fleck: it is only relieved of its apparent objectivity; knowledge is only possible on the basis of active stipulations whose interrelationships, however, can confront the ‘passive’ scientist. They present themselves to the members of a collective as objectively ascertainable reality. The goal of every scientific thought-collective is to maximize such passive linkages while minimizing the necessary active postulations:

The general aim of intellectual work is therefore maximum thought constraint with minimum thought caprice. (1979: 95)

It is important to note that with this conception of fact Fleck moves from considerations of the sociology of knowledge to those of the logic of knowledge. The concept of ‘constraint on thought’, with which a fact is presented as irrefutable, is interpreted by him in both directions. Thus, sociologically speaking, the individual member of a collective is not free to be scientifically
active otherwise than on the basis of his stylized socialization. His thought must start with the presuppositions created by the collective. But independently of these historically and socially developed conditions, they also represent epistemological determinants of scientific knowledge: they permit — logically — only the knowledge of interrelationships specific to them. Facts, such interrelationships necessarily arising from the postulated assumptions, are therefore not 'discovered' but simply ascertained.

An objective concept of truth loses all meaning in this perspective. But it is also taken by Fleck as "not 'relative' and certainly not 'subjective' in the popular sense of the word":

It is always, or almost always, completely determined within a thought-style. One can never say that the same thought is true for A and false for B. If A and B belong to the same thought-collective, the thought will be either true or false for both. But if they belong to different thought-collectives, it will just not be the same thought! It must either be unclear to, or be understood differently by, one of them. Truth is not a convention, but rather (1) in historical perspective, an event in the history of thought, (2) in its contemporary context, stylized thought constraint. (1979: 100)

Just as the concept of truth, so too the concept of fact is closely bound to that of thought constraint:

In the field of cognition, the signal of resistance opposing free arbitrary thinking is called a fact. (1979: 101)

A thought-collective will always accept something as a fact if it is formulated as a passive linkage appropriate to its thought-style and can be embedded in the 'fact-system' of its thought-style. For no fact occurs independently of the other passive linkages connected with it. With these it is firmly integrated into the 'system of opinion' of the collective:

A universally interconnected system of facts is thus formed, maintaining its balance through continuous interaction. This interwoven texture bestows solidity and tenacity upon the 'world of facts' and creates a feeling both of fixed reality and of the independent existence of the universe. (1979: 102)

Reality is understood as a 'network' of chains of thought joined together in 'facts' (1979: 79); it is something which cannot be separated as something that 'exists' from what is 'thought' (1979: 181).

R. S. COHEN
T. SCHNELLE
INTRODUCTION

NOTES

1 The complete results of this research are published in Schnelle (1982).
2 All of the papers collected here are now also available in German translation [cf. Fleck (1983a) in the bibliography]. Aside from these, Fleck also published two announcements of his monograph (1934d, 1935b). A little later he reacted in Przegląd Filozoficzny (1937a) to a short dismissive review of his approach by Izydora Dambeka. A few days before the outbreak of World War II there appeared in a popular Polish journal a polemical discussion between Fleck and the biologist Tadeusz Bilikiewicz on 'Science and Environment' (1939b).
3 For more detailed information, cf. Schnelle 1982.
4 The remainder of this Introduction was prepared by T. Schnelle.

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For references to Fleck, cf. the bibliography of Fleck’s publications at the end of this volume (pp. 445–457).

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INTRODUCTION


PART I
Ludwik Fleck was born in Lwów on July 11, 1896. His father Mauryce ran a middle class painting establishment there. Fleck grew up in the atmosphere of the relatively extensive cultural autonomy of Galicia: Since 1867 the Polish culture which had developed during the long period of Polish division had found favorable conditions here in the Austrian-occupied territory (cf. e.g., Hartmann, 1962, 1966). The multinational state of Austria-Hungary allowed its regions a certain measure of cultural independence. There were Polish schools, and the city boasted an old university at which classes had again been held in the Polish language since 1879 (Dobrowolski, 1960). It was precisely because of these liberal politics that the culture of German-speaking territories met with great understanding: science and culture here were closely related to those of Vienna — which was also the case for the period following Poland’s becoming independent in 1918.

Fleck graduated from the Polish Lyceum in 1914, but spoke German perfectly in addition to his Polish mother tongue. In 1914 he enrolled at the Jan Kazimierz University to study medicine, a course of study which he completed, after an interruption for military service, by earning his degree as a general practitioner [doktor wszecz-nauk lekarskich].

Even when he was a student, Fleck was particularly interested in the problems of microbiological research. In 1920 he became an assistant in the research laboratory for infectious diseases run by the famous typhus specialist Rudolf Weigl in Przemyśl (about 50 km from Lwów). When Weigl was appointed to the chair in biology at the medical school of the University of Lwów in 1921, Fleck went along with him as his assistant. He remained at the university until 1923. From then to 1939 he was not able to return to a university position. This may have been due equally to personal factors and to the politically volatile situation of the city: great tensions existed between its Polish, Polish-Jewish, and Ukrainian populations up until the second world war. Instead of working at a university, Fleck worked at first, until 1925, in the department of internal medicine of the General Hospital [Szpital Powszechny] in Lwów, where he was in charge of the bacteriological
laboratory research; then, in the same hospital, he worked as director of the bacteriological laboratory of the skin and venereal diseases department. In 1927 he spent six weeks in Vienna, studying with Professor R. Kraus at the Government Institute for Serotherapy. After returning to Lwów, Fleck, in 1928, became director of the bacteriological laboratory of the local social assurance [Ubezpieczalnia Społeczna]. From 1935 on, he worked only in the private bacteriological laboratory which he had founded in 1923.

In spite of the great amount of routine work connected to these positions, Fleck used every free minute for research, which he carried out in his private laboratory. During the period in which he worked as an assistant, his serological diagnostic typhus research was most important — while working with Weigl he discovered and developed a procedure for testing skin reactions to diagnose typhus which he called the ‘exanthin-reaction’ (1923b); later, he also worked on improving the diagnosis of syphilis (1930a, 1935c), on tuberculosis (‘Lupus erythematoses’, 1927b, 1931a), and on pemphigues (1937b, 1939c). Even at this time, however, his primary interest lay in general questions of serology. In 1931 he published his observations on the composition of leucocytes in blood preparations which deviated from the predicted values of the theories accepted up to that time (1931c). In 1939, together with the well-known mathematician Hugo Steinhaus, he attempted for the first time to strengthen this phenomenon by the as yet unknown clotting together of certain leucocytes (1939d, e). Fleck took up this question again after the war. Between 1922 and 1939 he published a total of 39 scientific writings. In Poland his essays appeared in all the important professional journals (Polska Gazeta Lekarska, Wiadomości Lekarskie, Medycyna Doświadczalna i Społeczna, among others); in other countries they appeared in such reputable medical journals as Klinische Wochenschrift, Zentralblatt für Immunitätsforschung und experimentelle Therapie, Krankheitsforschung, Zentralblatt für Bakteriologie, Dermatologische Wochenschrift, British Journal of Dermatology and Syphilis and Comptes rendus des séances de la Société de biologie.

In 1923 Fleck married Ernestina Waldman; a year later their son Ryszard was born. Besides them, there are only a few people still living who knew Fleck before the outbreak of World War II. According to them, Fleck at this time had a difficult character and was very aware of his intellectual qualities. This was shown, e.g., by the fact that even with persons close to him he only spoke about those topics of his scientific work concerning which those persons were professionally qualified to have some understanding. This is the reason why there are no living witnesses who remember having spoken with
Fleck in the twenties or thirties about his science-theoretical work. Since practically all of the family’s personal belongings were lost in the war, it is impossible to ascertain anything with certainty about the concrete background of his thought.

According to the memories of his wife in particular, however, it is at least clear that Fleck was never “only a doctor”: he grew up in an atmosphere which appreciated the scholar competent in his own field but also knowledgeable in others more than it appreciated the specialized professional who was only highly specialized. Thus Fleck was also above all a student of philosophy during his medical studies. During the twenties and thirties he regularly devoted his evening hours to reading in philosophy, sociology, and in the history of science. His choice of literature, however, so far as we can ascertain it today, appears to have been adventitious rather than systematic. According to his son’s recollection, Fleck worked for two or three years on the monograph which appeared in 1935. If this is correct, we are concerned here with the period between 1931 and 1934. What is uncertain is whether Fleck did indeed first write his monograph in Polish and only afterwards work it out in German as well, due to his inability to find a publisher for it. In any case he did discuss the expressions to be used in German at great length with a Jewish refugee from Germany.

Fleck’s first publication in the philosophy of science (1927a, pp. 39–46 in this volume) refers back to a talk which he had given a year earlier to the ‘Lwów Society of Friends of the History of Medicine’ (1926b). It was entitled ‘Some Specific Features of the Medical Way of Thinking.’ We do not know what brought Fleck to work on this topic nor when he began working on it. What is clear is that this talk represents a preparatory stage to his more thoroughly worked out theory. The significance of this fact that Fleck’s philosophical reflections on science are based on the analysis of the discipline of medicine becomes very clear here.

In his second work concerned with philosophy of science, an essay entitled ‘On the Crisis of “Reality”’ (1929b, pp. 47–58 in this volume), Fleck generalizes his statements on medicine to cover all of the natural sciences. The predominant question here is that of the ontological status of reality: Fleck emphatically rejects the possibility of an ‘absolute’ reality independent of experience.

In 1935/36 Fleck presented his theory of thought-style and the thought-collective [Denkstil and Denk Kollektiv] in a monograph and two long articles. The monograph was entitled Genesis and Development of a Scientific Fact [Entstehung und Entwicklung einer wissenschaftlichen Tatsache, 1935a]. As
stated above, the monograph was published in German, by a Swiss publisher. Both essays, on the other hand, were published in Polish, in the leading Polish philosophical journal Przegląd Filozoficzny [Philosophical Review]. The first was entitled 'Scientific Observation and Perception in General' (1935d, pp. 59–78 in this volume) and deals with Fleck's conception of the seeing of Gestalten as a 'stylistic perception' (stilgemäße Wahrnehmung), and with its epistemological significance. The second, published a year later (1936, pp. 79–112 in this volume), is entitled 'The Problem of Epistemology'. It develops the 'theory of thought-style and thought-collective', particularly from the viewpoints of the 'communication of thought' (Denkverkehr) and its significance for knowledge, its historicity and the inner structure of the thought-collective. In addition to these, two shorter essays from this phase of Fleck's work must be mentioned, with summarize the results of his efforts and which appeared in medical journals (1934d, 1935b). Here, too, finally, belongs a short reply by Fleck to a criticism of his theory (the only one of this period) which appeared in Przegląd Filozoficzny (1937a). It is not worthwhile to reprint these short notices he wrote concerning his published works.

THE QUESTION OF SOURCES

If we want to know about the intellectual sources which inspired Fleck in his reflections on philosophy of science, the first place to look is to the literature he cites and confronts in his own publications. In the reactions to Fleck's work since his death, which began in 1966 with W. Baldamus,¹ it is above all his sociological innovations which have been emphasized. The suspicion has been repeatedly voiced in this connection that Fleck was very familiar with recent European sociology of knowledge, and that he was particularly influenced by it in his own conceptual development (as I, too, have stated; cf. Schäfer and Schnelle, 1980). This is suggested even in his concepts, which are reminiscent of those of W. Jerusalem, Scheler, and Mannheim. In fact, Fleck discussed the sociology of knowledge explicitly in only two places in his publications: in his monograph (1935a: 62–67, Engl. 45–49) and in the essay 'The Problem of Epistemology' (1936: 80). All told he refers to only two publications in this respect: to the German translation of Levy-Bruhl's Thought of Natural Peoples (Denken der Naturvölker, German edition, 1921) and to the volume edited by Scheler (Versuche zu einer Soziologie des Wissens). From this, he cites only Jerusalem's contribution: 'The Sociological Conditionality of Thought and Thought Forms' (Die Soziologische Bedingtheit
des Denkens und der Denkform, 1924). He discusses Gumplowicz (1885) with reference to quotations included here, and also Durkheim with reference to quotations which Jerusalem gives in his prefatory note to the German edition of Levy-Bruhl’s Thought of Natural Peoples, which he published. It is these sources alone to which Fleck refers in discussing the ideas of the authors mentioned: Durkheim’s ‘Compulsion of Social Structures’ (Zwang der sozialen Gebilde), which makes these structures appear as objective facts to the individual, Levy-Bruhl’s ‘collective representations,’ Gumplowicz’s statement that it is the ‘social community,’ and not the human being alone, which ‘thinks in the human being,’ and Jerusalem’s statements about the social conditionality of the interpretation of sense perceptions, the mutual corroborations and social consolidation needed by knowledge. Fleck criticizes these authors for “a characteristic error: they exhibit an excessive respect, bordering on pious reverence for scientific facts” (1935a: 65, Engl. 47). Their assertion that it would be possible to ascertain objective data with social differentiation (the development of ‘independent individuals’ in Jerusalem’s terms), i.e., to liberate oneself from the social influences on knowledge, is for him untenable.

The literature in the sociology of knowledge to which Fleck refers here is of a very narrow breadth. Other works which he could perhaps have used to greater advantage, e.g., Mannheim’s, appear to have been unknown to him. However, this is not surprising if we consider the conditions in which Fleck developed his theory. Kept very busy by his tasks as a doctor, he had only a small amount of time available for this work. A real sociology was not to be had in Lwów, let alone a sociology of knowledge. Znaniecki (from 1920 to 1939 professor in Poznań) was certainly known there, but probably not very warmly received. In spite of these adverse circumstances, however, Fleck at least had the two monographs mentioned in his possession. How he came upon them is not known. It is possible that it was due to his reading a 1929 article which provided an overview of this field. Under the title ‘Science and the Forms of Social Life. Some Questions from the Border between Sociology and the Philosophy of Science’ (1929), Paweł Rybicki reported on developments in French- and German-speaking areas. In so doing he dealt extensively with publications Fleck has quoted in his work. The essay appeared in the noted journal Nauka Polska (Polish Science), published by the ‘Mianowski Foundation’ in Warsaw. Its function was to teach Polish scientists from all disciplines about the developments in other fields.

Reading the literature on the sociology of knowledge was not the only source from which Fleck could have become acquainted with its conceptual-
izations. When asked who of Fleck's acquaintances were persons with whom
he spoke regularly, his wife could only remember one name: Jakób Frostig.
Today there is little known about Frostig. After the war his name does not
appear in works about Polish science. It is therefore to be assumed that
Frostig, born like Fleck in 1896, died in the second world war. Before the
war, however, he appears to have been the undisputed leader in the field
of psychiatry in Poland. He published a two volume treatise on the subject
in 1933. And he was not only a psychologist or psychiatrist but also a phi-
losopher: in 1929 he published a study in German entitled Schizophrenic
Thinking: A Phenomenological Study of the Problem of Nonsensical Propo-
sitions (Das schizophren Denken. Phänomenologische Studie zum Problem
der widersinnigen Sätze). In Frostig's preparatory remarks we find a concep-
tion of experience according to which experience is understood relative to
the 'collective storehouse' (Kollektivbestand) of knowledge of one's social
group.

Unfortunately we do not know in precisely what period Fleck met regu-
larly with Frostig. Several remarks on Frostig's work, however, allow us to
form at least a few conjectures concerning the content of their discussions:
In his study Frostig wanted to investigate the seemingly 'nonsensical proposi-
tions' of the schizophrenic by comparing them with 'sensible' ('meaningful')
propositions. The analysis of the 'nonsensical' is thus supposed to be based
upon 'meaningful' propositions. For this reason he begins the study with
fundamental conceptions, concerning linguistic expressions in general. Lin-
guistic expressions, says Frostig, are always related to objects which are
imagined to exist independent of the person. The a priori character of objects
which is involved here, however, is only an apparent one: "the 'a priori' basic
principles of knowledge are those which must be common to all contents of
consciousness, otherwise they could not be given 'prior to all experience' "
(1929: 14). They are thus not subjective but collective principles. Linguistic
expressions denote "objective structures" which are "collectively founded"
(1929: 20). In this sense all knowledge is relative, indeed to the collective
knowledge of the group: "To determine the correctness of a statement we
must refer, as a test object, to our entire previous store of knowledge" (1929:
23). A truth criterion independent of this is impossible. The following lengthy
quotation from Frostig will show us his philosophical orientation better than
any summary could:

We know that our knowledge is always expanding, that ethical and aesthetic principles
are subject to change, that healthy common sense, our beneficient and life-sustaining
function, is one resulting from our geographical location, our economic situation and the
mental stage of organization of the given social group. (By a group is not to be understood only a solidly organized, social amalgamation of people, or a class, folk, situation, or state. We mean by this expression any group of people bound together by a common 'intention'. Thus the group of mathematicians is a group with a very special, mathematical mentality. It is clear that one and the same person can with reference to the object at hand belong to several very different human groups. In this way our mathematician can have banal, petit bourgeois views in the aesthetic or artistic realm.) Thus the group’s collective storehouse ever changes and with it the criteria for truth. If someone had dared fifty years ago to make the statement that "he would simply take malaria and inject it into a paralytic to make him well", the statement itself would have been judged paralytic. At the time it corresponded to no objective state of affairs; it was ungrounded; it could not be placed straight away into the outlook of the knowledge of the time. Today it is a well-grounded statement; we comply with its contents without contradiction. . . . It is only in relation to the collective store of the group in question that we can denote a state of affairs as correct or as a personal error. . . . Reality as we have used it in our investigation means reality as it is given to a group (1929: 25ff.).

In the course of his investigation Frostig then characterized the ‘nonsensical propositions’ of schizophrenics precisely by their lacking any reference to collectively set meanings. It is for this reason that these propositions remain ‘meaningless’. Fleck might have strongly disagreed with Frostig about this characterization of the speech of schizophrenics. He finally says explicitly:

The fruitfulness of the thought-collective theory is revealed especially in the facility which with its compare enables us to primitive, archaic, naive and psychotic types of thinking and to investigate them uniformly. It can also be applied to the thinking of a whole nation, a class or any group no matter how it is constituted (1935a: 70, Engl. 51).

Except for this disagreement, we are reminded here a great deal of Fleck’s notions and conceptions: thus the relativity of knowledge to the fund of knowledge already acquired or of reality to the experience of a social group. It is of course very possible that Frostig, too, developed his notions in response to the sociology of knowledge. He claims in fact that they go back to Husserl and Bergson. The introductory sentences of his preface refer to this as follows:

Following the postulates of Arthur Kronfeld, I have attempted to make some of the symptoms of schizophrenia phenomenologically more precise. I have Husserl to thank for the thought structures, Bergson for the manner of presentation (1929: 5).

During Fleck’s time, Bergson’s writings were very popular. In one of his essays he quotes long passages from the Polish translation of Bergson’s Introduction à la métaphysique (1903; Polish, 1910) in order to contrast Bergson’s concept of motion to Maxwell’s. A copy of the Polish translation
of Bergson’s *Matière et Mémoire* (1986; Polish, 1926) was found among Fleck’s collection after his death. Husserl may have been present in the discussions at Lwów due to his relation to Twardowski (both were students of Franz Brentano).

But let us return to the literature cited by Fleck. Besides the sociology of knowledge, the ‘philosophizing naturalists’ of the Vienna Circle represent the only other group with which Fleck involves himself in discussion. This discussion is both briefer and sharper than that carried on with the sociologists: the mistake of the Vienna Circle naturalists lies in an “excessive respect for logic and in regarding logical conclusions with a kind of pious reverence” (1935a: 69, Engl. 50). They did acknowledge the fact that the actual was always only a “(relation) governed by a more or less arbitrary reference system.” But they wanted to posit this system of reference as something absolute and unchanging in human thought. They thus fell into the error of assuming observations to be given without presuppositions. Besides Schlick, Fleck particularly mentions Carnap (1928, 1931, 1932/33), concerning whom he states with satisfaction that he (Carnap) “himself has already gradually relinquished this point of view” (1935a: 121n, Engl. 177):

What are these direct data if one has to look for them! In what way are they given directly if one has to argue about them to such an extent? It suffices to compare, in *Erkenntnis*, Vol. II (p. 432) (= Carnap 1931 – TS), and Vol. III (p. 215) (= Carnap 1932/33 – TS), in order to convince oneself to what extent Carnap became involved with his Protokollsätze and to establish the complete barreness of this matter. It leads necessarily to dogmatism or relativism, and in both cases it fails to provide any new research possibilities (1935d: 63).

Fleck states here that any further discussion would be superfluous, since “the adherents of direct elementary data do discredit themselves when they are not able to establish among themselves the nature of these directly given elements” (1935d: 63). In other texts he adds that even the recourse to multivalued logic or the logic of probability cannot solve the problems inherent to this approach (1936: 112n).

Even briefer than that of the Vienna Circle is Fleck’s criticism of Mach whose standpoint – according to which the ‘active linkages’ of knowledge are chosen according to economic viewpoints – he criticizes as being far-removed from practice (1935a: 14f., Engl. 9). He also mentions Kant and Wundt (1906), who are forced into ‘unthinkable ideas’ such as the unknowable ‘thing in itself’ by their maintaining an absolute notion of ‘existence’, ‘reality’ and ‘truth’ (1935a: 41f., Engl. 28).

Besides such negatively critical remarks, we also find remarks in which
Fleck reinforces his own opinions by citing other authors. These, too, are kept short, however, almost always consisting in picking out a random thought here or there. Thus, in a long footnote (1935a: 145ff., Engl. 179–181) he lists Simmel next to LeBon, McDougall, Freud and the political theorist Kelsen. According to Fleck, Simmel (1908) pointed out the change of thought value which occurs in the communication of thought. In LeBon (1895), we find what "(could) be considered the very as a paradigm of many discoveries. The mood-conforming gestalt-seeing and its sudden reversal: the different gestalt-seeing." But LeBon could not see the "advantages of socialization" in this connection. The matter lies differently with McDougall (1920): but he could not explain the "nonadditive element of the man psyche". The "man" according to McDougall is simply given the attributes of the individual by "organization." Freud (1921) on the other hand dissolved the "community of action and feelings ... into individual psychological elements." Kelsen (1922), finally, feared a "substantializing" of the "collective soul". To this Fleck raises the objection that the notion of 'substance' is out of date: 'Substance' is not without its own presuppositions and must for that reason be 'functionalized'.

Thanks to a brief notice in Die Naturwissenschaften by the psychologist Metzger (1929), Fleck became aware of the work of the linguist von Hornbostel (1927). This work enabled him to clarify his concept of the original fundamental or proto-idea, 'Urdee' as he called it: its strength diminished afterwards such that its verbal expression was experienced psychically as being in agreement with the corresponding experience.

Fleck also mentions Uexküll (1928) and his statements concerning the subjective conditionedness of our picture of the world – which appeared to Fleck to be contradictory, and not to go far enough (1935a: 138, Engl. 179). Repeatedly he quotes Bohr (1928) with the phrase: "The concept of observation has an element of arbitrariness, in that it essentially depends on what objects have to be included in the system that is being observed" (Fleck 1929b: 53, 1935d: 66, 1947e: 142). Schrödinger, whose lecture 'Is Natural Science Conditioned by its Milieu?' (Ist die Naturwissenschaft milieubedingt?: 1932) was published in Polish in 1933, is mentioned in one instance in the same breath with Bohr (1947e: 144). Fleck quotes Pascual Jordan's 'On the Positivist Notion of Reality' ('Über den positivistischen Begriff der Wirklichkeit,' 1934) with the statement that "scientific thought presupposes ... the entirety of pre-scientific experiences and concept constructions" (1936: 110).

The literature Fleck cites is thus little suited to answering the question of the intellectual sources of his thought. Its selection seems to have been
primarily due to chance: either thanks to its being mentioned by the philosophers of Lwów, Adjukiewicz and Chwistek, who were very familiar with the Vienna Circle, or to the reading of the journal *Die Naturwissenschaften*, in which several of the writings quoted by Fleck were published (Bohr, 1928; Metzger, 1929; Jordan, 1934). There was finally a series of authors quoted by Fleck who were very popular at the time thanks to wide-spread discussion: Bergson, Freud, LeBon and McDougall were 'on everyone's lips'. It was for this reason not at all unusual to quote them. What is more important with reference to the question at hand is that Fleck very obviously added the explicit references to the works of others after the completion of his own conceptual development. They did not serve as material with the help of which he worked out his own thought in order to formulate new ideas.

We are helped just as little in finding indications of Fleck's intellectual background by looking at the reviews of his monograph published in the thirties. In only 1 philosophical journal did anyone take notice of it: in the Belgian *Revue des Sciences Philosophiques et Théologiques* (Lauriers, 1937). In Poland, Leon Chwistek (1936) wrote a long review which was published in the cultural journal *Pion*. How the book came into the hands of Hans Reichenbach, at that time in Istanbul, is unknown. It is not very surprising, however, that the only mention he makes of Fleck (1938: 224n) is made in a context irrelevant to him. All the rest of the 19 reviews appeared either in medical (12) or in popular journals and newspapers, albeit in a broad geographical dispersion in German, Swiss, Austrian, English, Italian, Polish, Dutch and Swedish periodicals. (Cf. the bibliography added to this volume, pp. 445ff.) They are all positive concerning the richness of Fleck's expositions; in only a few cases do we also find critical remarks. With the exception of Chwistek, none of them can be related to Fleck in any way 'conforming to his style'.

On the contrary: some of the German reviewers expressed the opinion that Fleck had 'linked himself to our new German 'thought-style', which denies an 'absolute' science without presuppositions, and always sees science as a part of a total culture, in the presuppositions, ties and conditions of which it has a share' (Petersen, 1936: 239). Fischer (1936) misses the investigation of the 'positive role' played by the 'national community'. Kroh (1936: 164) suggests also taking the "racial determination of the thought-style into consideration". Stock draws the following conclusion from these statements in 1980 (!):

By adopting several additions it thus becomes possible to modify Fleck's theory such that it is possible to integrate it into the theories of national-socialist racial ideology without any contradictions (1980: 113).
Such statements cannot be reconciled with recent German history.

Of course the time of the monograph's publication by the Swiss publisher was unfavorable for political reasons: In Germany the Nazis could have no interest in the Polish Jew Fleck. The German-speaking centers of philosophy of science in Vienna, Prague and Berlin fell apart — their members joined the emigration.

Jakób Frostig, mentioned above, can, however, hardly have been the only source with whom Fleck was in contact about non-medical questions after his studies. During the period between the wars Lwów boasted a rich intellectual life. A number of excellent scholars were teaching in various departments of the university (as well as at the polytechnical college). Notable among them were the mathematicians of the Banach School, including Hugo Steinhaus in addition to Stefan Banach. Fleck worked with Steinhaus both before and after the war in interpreting the clotting phenomena of leucocytes in infections which he had observed (cf. Fleck, 1939d,e, 1947k). Stanisław Kulczyński and Józef Heller taught biology; Jakób Parnas worked in biochemistry. In medicine, the prominent figures were the microbiologist Weigl (with whom Fleck was an assistant, as mentioned above) and the pediatrician F. Groër. (Fleck was given scientific positions by Groër in 1941 and after the war in 1952 in the Institute for Mother and Child which Groër directed first in Lwów and later in Warsaw.)

In the social sciences it was primarily Franciszek Bujak, chief of the department of social and economic history, who caused a stir (on Bujak cf. Nowakowski, 1964; Madurowicz-Urbanska, 1976). Social economics itself was taught by Leopold Caro and Stanisław Grabski. The anthropologist Jan Czekanowski was a great influence on the youth studying in various fields, due to his regularly given interdisciplinary courses on statistics.

The philosophy of medicine primarily prominent in Warsaw between 1890 and 1920 was not found in Lwów in this form. In his survey of the philosophy of medicine, Szumowski (1949: 52f.) mentions two other doctors in Lwów, who were at least interested in this area, before he refers to Fleck: the internist Witold Ziembički (1874–1950), who would later receive an honorary professorship in the history of medicine, and Władysław Sieradzki (1870–1941), professor of forensic medicine, who taught medical propaedeutics and also touched on questions of the philosophy of medicine. It is interesting that Fleck had a special relationship to each of these two doctors: Sieradzki was the professor who had conferred Fleck's degree in 1922, and Ziembički was his immediate superior from 1922 until 1925 as the director of the second department of internal medicine of the general hospital.
Except for Szumowski's mention, there is little known about Sieradzki's work in the philosophy of medicine. There do not seem to be any publications on the topic from his hand. It is a different story with Ziembicki, however. From 1910 until 1935 he was director of the second department of internal medicine at the state hospital in Lwów; from 1929 on he lectured at the university on the history of medicine. After his Habilitation in this field in 1931 and his retirement in 1935, he received an honorary professorship in the field. His interest lay primarily in the history (and bibliography) of balneology and in the medical research of historical personalities. His interest in the history of medicine was also made evident in the scientific meetings which he organized for his co-workers and doctors of other departments from the beginning of his work at the state hospital. The 'Scientific Circle of State Doctors of the General Hospital of Lwów', which he directed, grew out of these meetings. Ziembicki did similar work for the Medical Society of Lwów, and was also the chairman of the Society of Friends of the History of Medicine in Lwów, the society to which Fleck presented his first work in the philosophy of science in 1926.

The scientific atmosphere in Lwów was definitely interdisciplinary. In addition to the scientific societies mentioned above, there were the all-inclusive Scientific Society of Lwów and the Ossolineum Foundation, which supported scientific research through stipends and publications. There were also a number of variously active discussion groups where in particular young scientists of various disciplines came together. Fleck was one of this circle. Professor Józef Heller (Warsaw) told me for example of a discussion group of biologists with various specializations.

Fleck also appears to have maintained close connections with the philosophical school of Lwów. The representative of this group from 1895 to 1930 was Franz Brentano's student Kazimierz Twardowski (1866–1938), to whose own circle of students almost all of the well-known Polish philosophers of the following generation belonged. Above all these included Kazimierz Ajdukiewicz, Tadeusz Kotarbiński and Władysław Tatarkiewicz. The logician Jan Łukasiewicz was the first student whose dissertation was directed by Twardowski. Supported by Twardowski's students, the 'Lwów-Warsaw School' was created in the period between the world wars. The school is not characterized by any one idea or body of thought. Rather, its students concerned themselves with an entire span of philosophical themes, extending from aesthetics and child psychology to mathematical logic, and they developed completely contrary approaches in doing so. Due to its members filling nearly all of the chairs in philosophy in the Polish universities, however, this
school was able practically to dominate Poland's philosophical life until the fifties.

The element uniting the Lwów-Warsaw school was to be found less in Twardowski's own research than in his teaching: he passed his deep abhorrence of metaphysical, interpretative systems on to his students. He brought them instead to concern themselves, as he did, with narrowly limited and precisely defined individual problems. The major issue was always the clarification of these problems — looking for their solution was never the real goal. Twardowski applied a high methodological standard in this work, which was expressed above all in notional and conceptual clarity.

For someone living in Lwów who was even only slightly interested in philosophy it was almost impossible not to come into contact with Twardowski. His lectures covered the whole range of philosophical teachings. They concentrated, however, on the themes and questions of Twardowski's own research: he saw it as his task to further develop Brentano's program of descriptive psychology. His works were characterized by the problematic ontological ambiguity already found in Brentano, an ambiguity by which he differentiated himself from empirical positivism: whether an 'object' is a representation of some objective 'given' or rather a product of the act of representation remained Twardowski's main problem.

Students from all disciplines attended Twardowski's lectures, but so did academically educated auditors working outside the university. His enrollments during all of his teaching years were the highest of all of the professors of Lwów. "Twardowski taught in the largest hall of the university, but even this proved too small to accommodate all of those wishing to attend his lectures, and the university had to rent the largest concert hall in the city for his classes." Thus his student Ajdukiewicz (1959: 31) described the popularity which Twardowski enjoyed. In addition to his regular lectures Twardowski also offered a series of seminars intended both for students of philosophy and for students in other fields.

Although we have no biographical proof of any contact between Fleck and Twardowski, his knowledge of Twardowski's philosophy may thus be considered certain.

The second chair in philosophy in Lwów had been held since 1903 by Michał Wartenberg (1868–1938), Kant scholar and translator of Kant's works. He was known as a defender of a metaphysics which shows how our knowledge achieves a unified image of the world as the natural need of man. His significance for the intellectual life of Lwów was in no way comparable to Twardowski's.
Among Twardowski's students, who from their importance belonged to the best students, his son-in-law Kazimierz Ajdukiewicz (1890–1963) was the only one who remained in Lwów. From 1922 until 1926 he taught at the university as a Docent; from 1928 on he was professor of philosophy. It was Ajdukiewicz who with his early works most clearly represented the nominalist branch of the Vienna Circle. He was as such anything but a mere 'Polish scion' of neo-positivism: what he calls his 'radical conventionalism' claims nothing less than that "the scientific world view is conventional down to the last detail and that it can be changed by a corresponding change in the conceptual apparatus, whereby each of these world-views can make the same claim of being 'true'." (1934f: 158).

Ajdukiewicz, too, was a very active teacher. Most important, however, there is biographical evidence which proves Fleck's connection to Ajdukiewicz. The University Library in Warsaw contains a reprint of Fleck's essay 'Scientific Observation and Perception in General' (1935d) from the journal *Prezeglad Filozoficzny* which bears a dedication by Fleck to Professor Ajdukiewicz. Ajdukiewicz probably gave this reprint to the library after the war, to enable it to replenish its collection in the wake of the great degree of systematic destruction of all Polish libraries by the German occupation. The dedication, written on the first page of text, and not on the cover, reads: "To the right honorable Professor Ajdukiewicz, given by the author, who requests his kind acceptance. — 20.7.35."

Unfortunately we do not know how far Fleck achieved the acceptance for which he asked. At any rate the wording here makes clear that Fleck was very eager to be recognized by Ajdukiewicz. Thus Ajdukiewicz, too, may have known Fleck personally.

Considering these connections with Fleck, the question arises of why Twardowski's and Ajdukiewicz's school did not consider Fleck in its publications. Only at first sight does the argument seem plausible that the logically oriented Lwów-Warsaw school would be hostile to any introduction of sociological and historical categories. The only discussion with Fleck undertaken by a member of this school is found in the article 'Is an Intersubjective Similarity of Sense Perceptions a Necessary Presupposition of the Exact Sciences?' (1937), by Twardowski's student Izydora Dąmbska. In this article she rejects Fleck's theory due to its relativizing of empirical statements:

Thus it seems that no matter what thought-style one expresses oneself in: the philosophical, 'mystical', scientific, or some other, there is one thought-style that almost everyone is able to understand. The most enthusiastic prophet, poet or mystic finds in certain life situations a language held in common with the natural scientist and they belong, with
him, to one style. In which situations does this occur? In those in which they are not asleep and must seriously consider the conditions of life. It is this common thought-style, embracing all men, which is used in making perceptual judgments. One would know nothing about the world in which one lived — one would in fact die a miserable death — if in a consistent fashion one stopped paying attention to empirical theses. It is perhaps in this biological role of the empirical propositions that the source of their intersubjective value lies (1937: 293f.).

But Fleck’s relativism could hardly have been the real reason for his otherwise being totally ignored in Lwów. This view is also held by Wolniewicz (1985, pp. 217ff. in this volume): it is true that Twardowski, like Dąmb ska and a number of his other students, rejected any relativizing of truth and reality; at the same time, however, Ajdukiewicz, who also belonged to Twardowski’s circle of students, himself maintained an epistemological relativism in his ‘radical conventionalism’ mentioned above. The lack of attention to Fleck in publications is more likely attributable to Twardowski’s rigid maintenance of conceptual precision and the care he took to assure the analytical validity of his arguments.

The logical-methodological standard — that which it [the Twardowski School — TS] considered subject to discussion in epistemology — was uncommonly high. This sometimes resulted in a subtlety that worked almost like a logical neurosis. ... Fleck’s writings could not satisfy this logical standard, due to the fact that his ideas were not yet ripe enough to do so. (Wolniewicz, 1985: 219)

In the profusion of Fleck’s new concepts there were a number of unclarities and contradictory statements — and this was found unacceptable. One example of this is precisely the central concept of the thought-collective. It can refer to a researcher in a specific field, to a scientific discipline as a whole, or go beyond these, as the ‘thought-collective of the exact sciences’. One of the interpretations which crosses these delimitations is the designation of a team of researchers composed of scientists from different disciplines as a thought-collective.

Beyond Twardowski’s circle of students we find in the world of philosophy in Lwów both Leon Chwistek and Roman Ingarden. Ingarden (1893–1970), a student of Husserl at Göttingen, had a teaching position in Lwów. Chwistek (1894–1944), on the other hand, hardly allowed himself to be fit into any of the traditional philosophical orientations: he became world-famous thanks to his contributions to formal logic, but was just as much a philosopher, painter, poet and art theorist. The characteristic theme of his philosophy was his theory of the ‘multitude of realities’, all of which exist, with equal rights, contiguous to one another: these realities represent constructions erected on
the foundation of mutually incompatible axiomatic systems. They reflect in each case the kind of possible experience on which they are founded.

Until 1930, Chwistek lived in Kraków, and then he accepted the newly created chair for logic in the mathematics and science faculty in Lwów. Various of Fleck’s acquaintances told me that they thought it likely Fleck knew Chwistek. Probably the only witness who can still give us any information about this is the Polish cultural historian Karol Estreicher, a student and friend of Chwistek and the author of an excellent bibliography (1971). Estreicher told me (February 10, 1982) that “Professor Leon Chwistek was a very good friend of Professor Fleck. They were both extremely interested in Einstein’s theory.” Two further circumstances support this information: first of all, Chwistek was the only philosopher in Lwów who reviewed Fleck’s works. References to Fleck’s publications are found in his own writings on the philosophy of science. Chwistek also wrote a long review of Fleck’s monograph (1936). The second point is that Chwistek had been married since 1916 to Olga Steinhaus, the sister of the Lwów mathematician Hugo Steinhaus mentioned above. Chwistek was, as Estreicher (1971) tells us, also a friend of his brother-in-law. And Fleck worked both before and after the war with Steinhaus on the statistical evaluation of his microbiological observations.

Since we cannot refer to the literature he cited or to the reviews of his philosophy of science to clarify which were the intellectual sources of Fleck’s thought, we must look to the circumstances of his life, portrayed above, in the period in which he was writing his epistemological works. These are the years from 1926, when he gave his first philosophical talk, to 1934, in which he signed his monograph “Lwów, Poland, Summer 1934” (1935a: 2, Engl. xxviii). During this time Fleck lived continuously in Lwów, except for a 6-weeks stay to study in Vienna. Whether Fleck made any contacts in Vienna outside the serotherapeutic institute in which he worked, and which contacts these were, is not known. At any rate Lwów itself had a fine intellectual life to offer, one in which its philosophers played an especially prominent role.

On the basis of the biographical information cited, we may assume it to be certain that Fleck was in contact with Kazimierz Twardowski, Kazimierz Ajdukiewicz and Leon Chwistek. This information suggests that the three outstanding philosophers in Lwów had an influence on the development of Fleck’s epistemological reflections. But if there were such an influence on Fleck, one could object, then it was only in a very one-sided way: there was no published reaction to Fleck by either Twardowski or Ajdukiewicz. Ajdukiewicz, whose own philosophical thought was still under development,
was certainly not inspired to any specific conceptual developments by Fleck. With Chwistek, things lay somewhat differently: in his works we repeatedly find references to various of Fleck’s works.

Scepticism concerning any intellectual influence of these Polish philosophers on Fleck also results from two further circumstances: for one thing, Fleck never once explicitly mentions them. This argument alone of course is not so significant: Fleck would hardly be the only thinker to neglect to name the actual sources of his ideas. The other point is that the three personalities in question represented three very different philosophical orientations: Kazimierz Twardowski remained true to his teacher Franz Brentano’s program of ‘descriptive psychology’; from the early period of his philosophical work up until 1936 (and only this period is of interest to us here), Kazimierz Ajdukiewicz developed and held an epistemologically relativistic approach, his so-called ‘radical conventionalism’, which even today has not received much attention; Leon Chwistek, world-renowned for his contributions to formal logic, was not active in the traditional schools during the period in which he was working on his ‘multitude of realities’.

Any influence of these three on Fleck would then have to be proved for each of them individually. That this can in fact be done I attempt to show in a separate essay (cf. pp. 231ff. in this volume): it will there be argued that it was especially in these three philosophers that Fleck found the concepts which he then developed and the problems he attempted to solve. In doing so he systematically took up questions that remain unanswered in their works in order to put them on a new foundation, and lead them to a coherent solution. The questions are those of the grounding of reality, knowledge and truth.

THE SECOND WORLD WAR: ODYSSEY

With the outbreak of World War II Lwów became Soviet. The medical school of the university became an independent college, the Ukrainian Medical Institute. Fleck was named teacher and director of the microbiology department. At the same time he became director of the municipal sanitation and bacteriological laboratory and advisor for microbiology and serology of the Institute for Mother and Child under the direction of Professor F. Groër.

With Nazi Germany’s attack on the Soviet Union and the German occupation of Lwów in the final days of June 1941, Fleck was forced to vacate these positions. He was deported with his wife and son to the city’s Jewish ghetto.

Concerning the conditions and fate of the Jewish ghetto in Lwów, there
is one easily accessible report from Fleck himself: from December 1943 until the liberation on April 11, 1945, Fleck was detained in the same block in Buchenwald as Eugen Kogon (cf. below). Kogon, who immediately after the liberation was charged with the documentation of the concentration camps, quotes Fleck's report on the state of affairs in Lwów in his book on *The SS-State*:

Dr. Ludwig Fleck reports: "To begin with, the ghetto in Lwów was in a section of the city comprising no less than a fifth of the city's area. There were approximately 140,000 Jews in Lwów, or 30 per cent of its population. Each Jew had to buy his new ghetto dwelling, a circumstance in which both the Poles and the Ukrainians were able to exploit the positions into which the Jews were forced. For the dwellings and furnishings the Jews had left behind they received nothing. We were allowed to take bed linens, kitchen utensils and work clothes; the rest became the sacrifice of the masses. In this ghetto, there were a few shops with the simplest of wares, a village hall, two general hospitals and one quarantine hospital. Life was miserable, everything was expensive. The ghetto lasted from fall of 1941 to August 1942, its inhabitants subjected daily to the worst of treatments. It was a matter of course that when the SS or the Wehrmacht needed something they would simply demand it from the leadership of the ghetto, and it had to be provided free of charge. In August 1942 the anti-Jewish mass action began under the command of the SS-leader and general of police Kazmann. The first stage lasted about two weeks. Approximately 50,000 Jews, primarily the old, the sick and children, were deported to Belzec, where, as later leaked out, they were gassed, together with all of the patients, doctors, nurses and attendants of the hospital for contagious diseases. This procedure, carried out by an SS-Sonderdienst [special service], was repeated every few weeks. The ghetto was moved to the outskirts of the city where almost no houses were still standing intact. Two square meters living space were allotted for each Jew. There were no shops, only smuggled food. Sanitary conditions were horrible. About 70 per cent of the Jewish population contracted typhus. During the day, the SS robbed and plundered; at night individual tortures and murders occurred. A forced labor camp was established, to which young and healthy Jews were delivered. The old and sick and women and children were gassed in a concentration camp near Belzec. By the fall of 1942 there were still about 15,000 Jews left in the ghetto — into which new arrivals from the surrounding area came — and about 12,000 in the labor camp. Concerning the further fate of the Lwów ghetto, we know from reliable sources that its inhabitants languished away there under the greatest deprivation until in March 1943 all of the survivors were murdered and the buildings burned down."

The brutality of these actions is difficult to describe: hunting human beings with dogs was part of it, dragging seriously ill patients out from their homes or hospitals where they often lay after operations with open wounds or broken limbs, wrenching out the arms and legs of small children who were then tossed like bundles up onto the transport wagons. SS-leader and head of police Kazmann dared to do all this . . .

The forced labor camps Lemberg [Lwów]-Weststraße was set up as a murder camp. Those detained there lived an average of two weeks, after which they succumbed to hunger, illness, the knout or other tortures, occasionally they were shot. The sick were
regularly put 'behind the wires' and killed; many voluntarily did this to avoid a worse fate.

Shortly after the ghetto's liquidation the inhabitants of the labor camps were all shot with machine guns. . . . The commandant of the labor camp was SS-leader Wilhans. (Kogon 1946: 215ff.)

In the hospital of this ghetto Fleck continued his research under the most primitive of conditions. Since despite the typhus epidemic there was no medicine in the ghetto for anti-typhus injections, Fleck developed a new procedure in which he procured vaccine from the urine of typhus patients. After the war he published this work (1946d, 1947h).

Fleck writes here very painstakingly about this work. Since it speaks best for itself, I would like to quote a long passage in translation from the Polish:

"In the first months of 1942 I began research on the urine of typhus patients in the ghetto hospital in Lwów, to see whether, similarly to the urine of patients suffering from pneumonia, this urine contained certain substances which would cause the production of antibodies. In the case of positive results, I wanted to work out an experiment which would allow an earlier diagnosis than was possible with the Weil-Felix reaction. The first experiment, carried out with the serum of sick patients with a strong Weil-Felix reaction, produced a significant precipitation ring. In order to determine whether we were really dealing with a seriological reaction, I intravenously injected rabbits with the sterile urine of patients. . . . In light of these encouraging results I worked further on this project with my colleagues Dr. Olga Elster, Dr. Bernhard Umschweif and Dr. Anhalt. We wanted especially, besides the diagnostic attempts, to use the urine as a source of specific antigens in order to produce prophylactic vaccine, the lack of which we sharply felt.

By May, 1942, we had achieved such good results that I made them known at the meeting of the hospital doctors and informed Professor Groër about them. They also reached the newspapers. Several months afterwards my excellent colleagues Dr. Elsterowa and Dr. Anhalt, of unforgettable memory, were murdered, but I continued the work with Dr. Umschweif until our arrest. Dr. Umschweif probably shared the fate of many other of our inestimable colleagues in a concentration camp.

Of those who worked together, it was my fate to be the one to now recount our common work. . . . On the basis of a part of our notes which I was able to hide and salvage, I now give an account . . . of our results at that time" (1946d: 663).

Concerning the injection of people with the vaccine obtained from sterilized urine, Fleck adds:

'With this vaccine I injected myself and my closest relatives, after which Dr. Elster injected 32 volunteers' (1946d: 663). Fleck injected himself on August 28, 1942. In his article in English on this subject he writes (1947h: 171): "Experiment on human subject (L. F.); August 28, subcutaneous injection . . . ; August 29, large area of local edema and redness; August 30, the reaction vanishes." – Concerning the injections of [other] people, Fleck says further: "The local reaction was slightly painful, no general reaction
was observed. . . . After this we gave approximately 500 injections in the fatal camp on Janowska Street in Lwów [the above-mentioned labor and concentration camp — TS], aided by Dr. Kurzrok. As far as we were concerned, the sole purpose of this action, carried out with the permission of the Germans, was to provide medical help which the camp did not have at the time. Dr. Kurzrok managed with this help to set up a small hospital in the camp, in which we observed and treated the patients suffering from the typhus, and we collected urine. Dr. Kurzrok was later murdered at this post, together with all the personnel. Of the persons injected, a large number fell ill. The death rate of those in the camp who were not injected was at the time almost 100%. The statistics on those who were injected have unfortunately been lost. I know only the fate of my family and of two other persons, five all told who in spite of the injection came down with typhus. My son and I became infected in Auschwitz in March 1943, i.e., about seven months after being injected. Our illness lasted seven days; the outbreak on the skin was clear. I was able to do physical work the entire time, my son was only in bed a few days. The course of the disease was thus very mild. My wife fell ill two months later, the outbreak on the skin was typical and the course of the illness more difficult due to a complication of dysentery. In spite of this she began to get well after three weeks. One of my colleagues fell ill two months after being injected, the course of the illness was unusually mild, the outbreak on the skin clear, . . . the illness lasted six days. The fifth person also experienced abortive typhus, and did not fall ill in spite of living for several months in a threatening environment and very bad conditions; other prisoners, who had not been injected, often fell ill with typhus within the first month of their being in the camp. The mortality rate at the time was approximately 30% (1946d: 665)

Fleck’s work was also known to the German occupiers. It made it clear to them that Fleck belonged to the leading European specialists on typhus. (To the question asked by one of the Germans, “Can this vaccine also be used for Germans?” Fleck is said to have answered “that that was doubtful, since Germans were after all another race, and the vaccine had been made from the urine of sick Jews. . . .” Thus Fleck told the story, according to statements at Yad Vashem, [Israel: memorial and museum archive — Ed.] Document Nr. 0–3/650.) For this reason Fleck and his family were arrested in December, 1942 and deported to the ‘Laokoon’ pharmaceutical factory.

Szende, himself deported to the Jewish ghetto of Lwów, wrote about Fleck in his work The Last Jew from Poland (1943: 215):

At that time a Jewish doctor named Fleck managed to produce a new serum against typhus . . . Dr. Fleck produced his serum at the risk of his life and injected as many Jews as possible. When the German authorities learned of this, they arrested Dr. Fleck and his assistants. They forced those arrested to instruct several German doctors in the production of the new serum, at which point the discoverers of the serum disappeared from the city.

In the factory which had been commandeered, one where organic medicines
were made, a number of Polish medical scientists were working on various problems. Janina Opińska-Blauth, a colleague of Fleck's in Lublin after the war, reports (1979: 81ff.) that it was her task to investigate ways of removing the bitterness of chestnuts so that they could be used as food if necessary. She also mentions Fleck's work in this factory, which consisted in producing typhus serum. Concerning the living conditions of the Jews in the factory she wrote:

One of the orders [of the occupying German director — TS] was the command that all workers of Jewish origin had to live with their families on the grounds of the factory. This was no doubt commanded in order to protect them from the roundup of Jews in the streets and dragging them out of their houses, but this was not dictated by any interest in the fate of these people; rather it was due to an interest in the factory (1979: 81f.).

Fleck's stay in the 'Laokoon' factory did not last long. At the end of January, he and his family were arrested again; after a short detainment in the prison of Lwów, they were sent to the Auschwitz concentration camp on February 7, 1943. In an article in the journal Po prostu, read primarily by the academic youth, Lutowski (1950) quotes Fleck on his arrest with the following words:

In January 1942 ... I was brought to the jail on Tański Street. From there it was a straight path to Auschwitz. I began my career in the concentration camp with physical labor. That lasted until I fell ill with typhus. While I was convalescing one of the SS men kicked me and broke a rib. I fell ill again [with pleurisy — TS]. After this I was brought, unexpectedly enough, to the laboratory of the sick bay.

The physical labor in question was probably that of a so-called 'caretaker' in Block 20, which belonged to the infirmary. Fleck managed to hide his typhus (as well as his son's). He attributes this to the effect of the vaccine he had produced in the ghetto of Lwów, with which he had injected himself, his family and many others. I was given more precise information about Fleck's fate in Auschwitz from Herman Langbein, who was himself taken to Auschwitz and is today secretary of the Comité International des Camps in Vienna. According to Langbein, Fleck was first detained in Block 20, which belonged to the hospital and put to work in the Hygiene Institute of the Waffen-SS which was located at that time in Block 10. Later he was moved to Block 10, to live there together with his family — "an unusual exception", as Langbein wrote (August 1, 1978). This block was isolated. It was here that the infamous sterilization experiments of Clauberg and Schumann were performed (Fleck comments on this in Kogon (1946: 264)). Other statements corroborate this, in particular those of Dr. Anna Seemann (Paris), who
worked with Fleck in the 'Laokoon' factory and in Auschwitz, and Dr. Hautval (Grosley, France), a doctor deported from France who also worked in Block 10 for two months in 1943. She wrote to me (April 2, 1980): "In the same block there was a laboratory of the 'Hygienic Institute' in which four prisoners worked, headed by Dr. Fleck."

This was a serological laboratory. Dr. Seemann was one of the other prisoners who worked here. According to her (in a letter of April 2, 1980) it was Fleck's task to diagnose syphilis, typhus, and other illnesses using serological tests such as the Wassermann, Well-Felix and Gruber-Widal reactions. Dr. Hautval says on this point:

I would also like to mention how very helpful the team of this laboratory was to us. They were always willing to do any tests necessary for my patients and were able to cleverly fudge the public results to make them look harmless because the actual results determined the fate of the patient.

In August 1943 the Hygiene Institute of the SS in Buchenwald set up a laboratory for the production and study of production methods for typhus serum. At the command of the SS economic administration headquarters in Berlin, Fleck was deported here in December 1943.

In his work Der SS-Staat, Eugen Kogon, who as a prisoner had been secretary to SS-director, Dr. Ding-Schuler, ever since the founding of the Institute, wrote about this block. Dr. Ding-Schuler was also the director of the clinical station of the Typhus and Virus Research Division of the Hygiene Institute of the Waffen-SS, which had been founded in the late fall of 1941 and moved to Block 46 in 1942. It was here that experiments on people were carried out, through which the effect of various kinds of typhus serum was to be evaluated. To this end, a large number of prisoners were artificially injected with the blood of patients suffering from typhus. Block 46 served as the isolation ward for all those in the camp suffering from typhus. It was only its leader that Block 50 had in common with Block 46. Kogon writes:

In Block 50, typhus serum was produced from mouse and rabbit lungs according to a procedure developed by Professor Giroud in Paris. . . . The best available specialists in the camp, including doctors, bacteriologists, serologists and chemists, had been chosen for this work, the most notable of them being the Lwów teacher Dr. Ludwig Fleck, whom Ding-Schuler especially had let come through the SS-WVHA [= SS economic administration headquarters – TS] send from Auschwitz to Buchenwald. A clever prisoners' politics had from the very beginning contrived to bring together for this assignment comrades of all nations who were especially endangered, since the SS had as much respect for Block 50 as it had for Block 46. (This refers to the fear of the SS camp
leaders of becoming infected with typhus, since they believed that one could also be infected by contact, through the air, or by coughing. For this reason they did not enter Block 46. Cf. Kogon (1946: 175 – TS). This tabu fear on the part of the SS was heightened by SS director Dr. Ding-Schuler as well as by the prisoners, for different reasons. (There were, e.g., warnings posted on the separate barbed wire fence setting off the block.) Candidates for death such as the Dutch physics professor Van Lingen, the Dutch physical culture official Jean Robert, the Dutch architect Harry Fleck and other Dutchmen, the Polish physician Marian Ciepiewiński, who was put in charge of production, Professor Dr. Balachowsky of the Pasteur Institute in Paris, the author of this report [Kogon] who had been an Austrian journalist and seven Jewish comrades all found refuge and protection from immediate actions and death transports with the full knowledge of Dr. Ding-Schuler and on the basis of appropriate petitions to the headquarters of the Reich’s Security Office, always suggested, composed and presented for a signature. ‘Ultimatum refugium judaeorum’ – the ultimate refuge for Jews – Dr. Ding-Schuler once jokingly, but not incorrectly, called Block 50. The detail was composed of 65 men, including twelve Russians. The valuable instruments, apparatus, microscopes and such came for the most part from France, either as ‘booty’ or having been ‘purchased’ from French firms without subsequent payment.

The cultivation of the strains to produce typhus (Rickettsia prowazekii) was effected by giving 2 cc of blood from the typhus patients in Block 46 to guinea pigs. Officially, two kinds of serum were produced: a standard serum for the combat troops of the Waffen-SS and another, which looked somewhat turgid and was for that reason set aside, for the prisoners. In fact, and without Dr. Ding-Schuler’s knowledge, a relatively limited quantity of top quality serum was produced for the prisoners in exposed positions and another large batch of a lesser quality for the SS, a batch which did no harm but which did no good either.

The block was a true enclave in the camp. The prisoners who worked there, exclusively the political prisoners, had a privileged existence in many material ways until the end, although they lived in constant danger of laboratory infections and had a very difficult chief: Each had his own bed, which was unheard of for the overwhelming majority of the camp, fresh bed linens, a bright clean work place, offices, an extra allowance in the form of 80 grams of sugar, 64 grams of fat and 400 grams of bread a week and, when they didn’t refuse it, illegally the meat of the typhus-infected rabbits which had been heated to 120 degrees Celsius and which should have been burned after the removal of the lungs (1946: 176f.).

The sabotage by this laboratory’s prisoners, reported by Kogon, is also well-documented in other ways, Łużyński (1950) reports, according to Fleck, the group also had to send control samples of the serum they produced to other institutions. Via Kogon, however, who took care of all of Ding-Schuler’s paper work, they were able to send high quality vaccine in these shipments. It cannot be doubted that all of these activities – of which the Germans remained ignorant until the end of the war – were only possible thanks to the direction of the serological scientists in the block: Fleck, the Polish doctor Ciepiewiński and the French professor Waitz.
It did happen twice that others became aware of the ineffectiveness of the serum. Barbarski reports on this in the article 'Sabotage in a Capsule' in the journal Przegro[ó]j (1947): "typhus vaccine was at the time being produced according to Giroud's method, using infected mice; in Block 50, however, they were using the brains of guinea pigs. It has also been reported, in agreement with this, that the SS leader, Dr. Ding-Schuler, had no professional knowledge and had in fact received his medical diploma through party relations (cf. also on this point, Kogon's statements). He first became suspicious when he received a work by the Romanian doctor Combișcu, according to whom the production of vaccine in this way was impossible. Dr. Ciepielewski was able to convince Dr. Ding-Schuler otherwise, and wrote an article to that effect for Dr. Ding-Schuler which was supposedly sent to the Zeitschrift für Hygiene und Infektionskrankheiten, but which to my knowledge was not published, unlike other, earlier pseudo-scientific works of this kind by Dr. Ding-Schuler.

In any case, after the war the work fell into the hands of a large pharmaceutical firm, which was not able to produce effective serum using the method described. Barbarski reports that they wrote to Dr. Ciepielewski about it, asking for an explanation. Barbarski says that approximately 600 liters of the ineffective vaccine was produced, with which around 30,000 SS men at the front were injected. The effective serum produced amounted to six liters (thus writes Fleck in an unpublished manuscript, 1958d).

 Upon learning of Fleck's fate during the war, we are at first surprised to learn that he published an article in 1946 in which he gave a full epistemological evaluation of his observations in the laboratory of Block 50. We can understand this as a sign of the fact that Fleck was trying from the very beginning to survive spiritually: he ruggedly maintained his research interests — not only in serology, as we have already shown, but in epistemology as well.

 The basis of Fleck's description seems to be the error by which, according to Kogon, the ineffective vaccine was first produced in Block 50. Kogon said at the Nürnberg-trials (on January 7, 1947 according to Bayle (1950: 1178f.)) that the prisoners first learned from Fleck that their vaccine was ineffective. In agreement with Fleck, however, they agreed to continue the production as sabotage:

The first stage of the vaccine production was purely experimental. We had a method which had been more or less taken from the Pasteur Institute in Paris [i.e., Giroud's method — TS], and we had to test its efficacy in Block 50. The animal experiments took about four months; we were always pressed by Ding, who wanted tangible results very quickly. We then decided, along with the bacteriologist and the director of production, Marian Ciepielewski, to produce a harmless, light vaccine. The cause of typhus, Rickettsia prowazekii, is not yet certain. It is difficult to know what form definitely constitutes the cause of the illness, i.e., the typhus germ. This fact allowed us to take the course we wished to take.

When Dr. Ludwig Fleck came to Block 50 at Buchenwald he told us, after having seen the typhus germs which we had produced from the lungs of rabbits, that we did
not have Rickettsias but another type of germ. We asked him not to convey this to Ding but to experiment with us to help us get out of this difficulty. During the two years that he worked with us Dr. Fleck kept the secret. It was only when the Cracow Institute furnished us with mice lungs and with infected material coming from mice intestines that we were able to be sure that our animal material contained *Rickettsia prowazeki*; after this we produced a vaccine which was without any doubt very effective, but we could only produce small quantities of it.

Since Ding was asking for large quantities of vaccine, we produced two kinds: one, which had no value and was perfectly harmless, we produced in large quantities; this vaccine was sent to the front; the other, produced in very small quantities, and very effective, was used in special cases—for example for ourselves and our comrades who were working in dangerous parts of the camp. Ding-Schuler never heard us speak of these arrangements. Since he had no real bacteriological knowledge he did not discover the secret of our production. He was entirely dependent on the reports given to him by the experts in Block 50. Besides that, it was due to his audacity that he obtained visible outward success: when he saw thirty or forty liters of vaccine to send to Berlin he was happy. He was very preoccupied with the vaccination of the SS troops and with the possibility that they might fall ill in Russia and die. The ineffectiveness of our vaccine should then have been discovered and the outside experts which the SS had at their disposal should have investigated and assured them that the real vaccine had scarcely been produced at all. None of this happened, however, and the adventure continued until March of 1945.

According to Fleck's description, however, it seems at the least unclear whether the error mentioned was connected to the production of the vaccine or to research conducted for Dr. Ding-Schuler (he was hoping to do his *Habilitation* with the aid of the work done by Block 50). Perhaps Fleck is referring to a second error, different from the one mentioned by Kogon. In any case, Fleck uses this fact as the point of departure for his proof that the 'thought collective' of serological laymen in Block 50, like every other scientific collective, constructed a self-consistent edifice of knowledge due to social factors at work within it related to the cooperation of its members. In comparison to 'real' serological knowledge it is based on an error, but since it was systematically taken up into the conceptions of the collective, it did not lead to any internal contradictions.

The article in which Fleck described these observations was published under the title 'Problems of the Science of Science' (1946h, pp. 113–128 in this volume). In it Fleck assigns the responsibility for not noticing this error to the 'sociological conditions' of the laymen's collective: the group was isolated from the proper thought-collective of microbiologists. There were thus no contacts which could have contradicted the mutual self-verification by the group's members of the correctness of their discovery. Under the pressure of the situation, a specific collective attitude developed which
allowed the group to construct an independent, closed system of opinion.

The group was only forced to acknowledge the shakiness of its scientific
edifice when it had to use 'real' infectious material delivered from outside
the camp. The result, however, was by no means the rapid collapse of the
system — the social forces internal to the group were much too strong for
this. Instead a long process of adaptation began, under the pressure of the
'recognized authority' of the originating institute whence the material came,
a process in which the concepts of the group were adjusted to those of the
professionally proper microbiological thought collective. So the edifice
constructed by the isolated collective of laymen finally proved untenable:
it rested on a systematic mistake — but on one which was a 'mistake' from
only one standpoint, which was located outside this lay collective. In itself
the system of opinion was consistent. On the basis of its active linkages, it
was able to make corresponding discoveries.

Fleck tells us (1958d) that in the last days before the liberation of the
Buchenwald concentration camp by the American army on April 11, 1945,
he was hidden by the leadership of the communist underground, like many
other Jews and political prisoners, in order to protect him from being evac-
uated. Fleck's wife came from Auschwitz to Birkenau for two months, from
whence she was evacuated to Ravensbrück and free on April 30, 1945. His
son Ryszard on the other hand was taken first to Groß-Rosen and then to
Buchenwald after the evacuation from Auschwitz and Birkenau. All of Fleck's
other relatives died during the war.

It was only after several months in the hospital, first in Buchenwald itself
and then in Bolesławiec, that Fleck was able to return to Poland in July
1945. In 1948 Fleck travelled to Nürnberg as an expert in the trial of the IG
Farben concern. He testified there concerning the experiments in Block 46
of Buchenwald with typhus sera produced by IG Farben on prisoners who
had been artificially infected.

In spite of various published documents concerning Fleck's fate during the
war, some of which have been cited here, there were numerous rumours in
the fifties about some sort of collaboration on Fleck's part. They were
primarily based on a statement, quoted in Bayle's report (1950: 1158ff.),
by Alfred Balachovsky (Pasteur Institute, Paris), a prisoner like Fleck in
Block 50 of Buchenwald from May 1944 on. Balachovsky gives the impression
there that observations concerning serological materials which Fleck had
related to Ding-Schuler had resulted in the latter's ordering additional exper-
iments on the people in Block 46. This would have gone against the rule
followed by all of the prisoners to never take any scientific initiatives that
could lead to such consequences. This statement was in no way corroborated by Fleck's and Balachovsky's fellow prisoners. Fleck defended himself against the charges in a manuscript written in Israel and sent to Warsaw in 1958 (1958d; the manuscript was never published; but is contained in the Archive of Professor Konopka at the Główna Biblioteka Lekarska, Warsaw). Balachovsky's formulation of the material allegedly passed along to Ding-Schuler does not even make sense scientifically, Fleck states. If there ever had been such statements made, Balachovsky would not have been able to understand them anyway due to his lack of knowledge of German.

AFTER WORLD WAR II: FLECK'S ACADEMIC CAREER

After his return to Poland, Fleck went to Lublin, where in October 1945 he became the director of the Department of Medical Microbiology of the Medical Faculty of the Marie-Curie-Skłodowska University. After his Habilitation with Professor Ludwik Hirszfeld in Wrocław, he became an assistant professor at the 'Academy of Medicine', which had in the meantime become independent. Already in 1950 he became full professor. Fleck remained there until 1952, at which time he moved to the Department of Microbiology and Immunology of the Mother and Child Institute in Warsaw (under the direction of Professor F. Groër) in order to improve his possibilities for doing research considerably. In 1954 Fleck was admitted to the Polish Academy of Sciences, first as a corresponding and then as full member. A year later he was already elected to the Presidium of the Academy, in which function he founded and built up the sixth division of the Academy, the medical division.

At the heart of Fleck's research during these years was the question of the behavior of leucocytes in infectious and stress situations. Already before the war he had observed deviations in the composition of leucocytes in the blood which varied from normally predicted values. In 1939 he had expressed his suspicion that this could be explained by the clotting together of certain leucocytes. In publications after the war, Fleck refers to 1942 as the year in which he first discovered this phenomenon. He does not usually mention the further circumstances of this discovery; as mentioned above, Fleck was in the ghetto of Lwów at that time. During his detention in the concentration camps, as can also be seen in his own statements, he again had the opportunity of continuing his observations on this topic. After the war he did research on this phenomenon in great detail, in Lublin and also in Warsaw, with the help of a large number of assistants. He called the phenomenon 'leucergy': a defense mechanism which occurs in practically all cases of
inflammation as well as with infections, pregnancy, heavy blood loss and a series of other stress situations. The white corpuscles (leucocytes) cluster themselves together in these situations to form cytologically homogenous, adhesive groups. Leucergic leucocytes have a higher number of glucose and phosphate elements and are characterized in particular by greater kinetic ability and phagocyte activity. The confirmation of leucergy by means of the 'Fleck test' soon showed itself to be a quick way to prove the existence of an inflammation or infection at an early stage. Beyond this, Fleck worked in particular on the investigation of possible connections between specific leucergic conditions and specific infectious agents and conditions of inflammation. Again he collaborated with the mathematician Steinhaus, as he had done before the war, since he especially hoped to produce statistical results. He also worked to explain the specific way in which the clotted leucocytes worked to destroy bacteria in various diseases.

Leucergy is a phenomenon recognized by the medical world, even though it has only met with lesser recognition in the West due to the somewhat isolated consideration paid to Polish medicine in these countries. Another reason for this lesser recognition lies in the fact that the interest in microbiology here after the war was concentrated on the methods of molecular biology and biochemistry due to American influences and the development of new mechanical and analytical instruments making these methods possible. Fleck did not look favorably on this reductionist orientation: his starting point was the observation of the interaction of the various elements involved in an immunological resistance process. He sought after an integrated synthetic description instead of a mechanical-analytical explanation. In contrast to the western indifference to leucergy (for example, only a few of our medical dictionaries contain the concept), in Poland it is a realm of investigation in which work is still being done in different quarters (even if it is no longer a central theme in medical research). Soviet scientific interest is indicated by several dissertations written on leucergy in Moscow and Alma-Ata in the sixties (Maz, 1965; Sosonkin, 1970; Usbekova, 1968). Recently at the University of Tel-Aviv there has been a research group working on an extensive research program in various hospitals and on animals, to see whether it is possible with the help of leucergy to distinguish at an early stage between viral and bacterial infections and different kinds of inflammations. It is hoped that the leucergy test can be used as a medical decision procedure in hospitals.

In addition to this central theme of his research, Fleck concerned himself in the years after the war with a series of other questions involved with important unsolved medical problems: in particular with work on the cause
and prevention of diphtheria, on leucocytosis, on the Wassermann test for syphilis and on the diagnosis and immunization for typhus discussed above.

The years between 1946 and 1957 were the period of Fleck's most intensive medical research: in both Lublin and Warsaw he had groups of assistants to help him, totalling 20 scientific and seven technical assistants. He directed approximately 50 doctoral dissertations during this period as well as a number of Habilitationen. He published 87 medical and scientific articles, in Polish, French (Sang, Annales de l'Institut Pasteur), English (The Lancet), American (Texas Reports on Biology and Medicine, Journal of the American Medical Association, Archives of Pathology) and Swiss journals (Schweizer Medizinische Wochenschrift, Acta Haematologica, Vox Sanguinis). He travelled for congresses and lectures to Denmark, France, the U.S.S.R., the U.S.A. and Brazil, among other countries. In 1951, Fleck was awarded the National Prize for Scientific Achievements, second degree; in 1955 he was distinguished with the Officer's Cross of the Order of the Renaissance of Poland.

After the war Fleck also continued his work in the philosophy of science. During his teaching and research in microbiology in Lublin and until 1952, Fleck was a member of the Lublin Philosophical and Psychological Society, and regularly took part in their meetings. During this period he published two further publications in the philosophy of science: the report cited above on his observations in Buchenwald, 'Problems of the Science of Science' (1946h, pp. 113–128 in this volume) and the essay 'To Look, To See, To Know' (1947e, pp. 129–152 in this volume).

After this Fleck published nothing more on the philosophy of science. We can, however, assume with certainty that he maintained an interest in this area in addition to his microbiological work. Thus we find a short notice about his work in the Trybuna Luda, written in 1957, in which he announces the soon-to-be completed manuscript which is to be the second volume of the book published in 1935: "I must confess something else: I am working on the methodology of the natural sciences and am preparing an extensive work in this field. This will be the work's second volume: the first volume was published in Switzerland in 1935" (1957f.).

The manuscript mentioned has as yet not been found, and it appears doubtful whether it (still) exists at all.

Fleck's character during this period of intensive scientific work is described as being totally changed with respect to the pre-war period. All witnesses have agreed in discussing his great human compassion and constant concern for his colleagues, students and family. All of the intolerance with which he reacted before the war to things displeasing to him had vanished, converted into its
opposite. But none of the horrible phases of his fate during the war were able to destroy his awareness of his own intellectual strength.

The year 1957 meant yet another turning point for Fleck: for one thing his health began noticeably to worsen. After a heart attack (infarct) which he had suffered in 1956, it was discovered that Fleck was suffering from lymphosarcoma, a cancer originating in the lymph nodes. Another change came in that Fleck emigrated to Israel in this year with his wife. In spite of his prominent place in Polish science, he decided to take this step in order that he and his wife could again live close to his son. The son had been living in Palestine since the end of the war. In Israel a position was created for Fleck at the Israel Institute for Biological Research in Ness-Ziona, a position which allowed him to continue his research as director of the Department of Experimental Pathology. His final publications from this period are concerned with questions about leucergy. In 1959 Fleck was made visiting Professor of Microbiology at the medical school of the Hebrew University in Jerusalem. His effectiveness was severely limited, however, by his difficulties with the Hebrew language and his worsening health. For the same reasons he could not accept Nathan Rotenstreich’s invitation to offer a seminar in the philosophy department of Hebrew University on problems in the philosophy of science. He tried one final time to draw attention to his theory of ‘thought-style’ with a short English manuscript which he had written as a reaction to a discussion on ‘Science and Human Welfare’ in the July 1960 issue of Science. But all the journals to which he offered this manuscript (Science, American Scientist, New Scientist, The British Journal for the Philosophy of Science) turned it down. It is published in this volume for the first time (pp. 153–158).

Ludwik Fleck died in Ness-Ziona on July 5, 1961, at the age of 64, of a second heart attack.

ACKNOWLEDGEMENTS

For all biographical information I have to thank a large number of close friends, assistants and pupils of Fleck in Poland, Israel and several other European countries, but first of all his widow, Mrs. Ernestina Fleck, and his son, Mr. Ryszard Arie Fleck, both in Petah Tikwa, Israel.

Biographical documents have been shown to me in the Archives of Prof. Stanisław Konopka, ‘Główna Biblioteka Lekarska’ in Warsaw, in ‘Żydowski Instytut Historyczny w Polsce’ (The Jewish Historical Institute in Poland) in Warsaw, in ‘Polska Akademia Nauk’ (Polish Academy of Sciences) in Warsaw, in ‘Yad Vashem – Martyrs’ and Heroes ‘Remembrance Authority’
in Jerusalem, in the archives of Prof. Marcus A. Klingberg, 'Israel Institute for Biological Research' in Ness-Ziona and from the personal possession of Mrs. Ernestina Fleck.

NOTES

1 On the reception of Fleck during the last twenty years cf. Schnelle 1982, p. 70f.
2 On the scientific and intellectual life in Lwów cf. Dobrowolski (1960), Mańkowski (1934), Markiewicz (1985) (p. 223 in this volume), Minerva – Jahrbuch der gelehrten Welt (several volumes).

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PART II

LUDWIK FLECK’S PAPERS ON THE PHILOSOPHY OF SCIENCE
SOME SPECIFIC FEATURES OF THE MEDICAL WAY
OF THINKING [1927]*

LUDWIK FLECK

Medical science, whose range is as vast as its history is old, has led to the
formation of a specific style in the grasping of its problems and of a specific
way of treating medical phenomena, i.e. to a specific type of thinking. In
substance such separateness of the way of thinking is nothing extraordinary.
One has only to realize the difference between the way of thinking of a
scientist and that of a humanist, even if the subject in question is the same:
for example, how great is the difference, and how great is the impossibility
of a direct juxtaposition, between psychology as science and as a branch of
philosophy. Even the very subject of medical cognition differs in principle
from that of scientific cognition. A scientist looks for typical, normal phe-
nomena, while a medical man studies precisely the atypical, abnormal,
morbid phenomena. And it is evident that he finds on this road a great
wealth and range of individuality of these phenomena which form a great
number, without distinctly delimited units, and abounding in transitional,
boundary states. There exists no strict boundary between what is healthy
and what is diseased, and one never finds exactly the same clinical picture
again. But this extremely rich wealth of forever different variants is to be
surmounted mentally, for such is the cognitive task of medicine. How does
one find a law for irregular phenomena? – this is the fundamental problem
of medical thinking. In what way should they be grasped and what relations
should be adopted between them in order to obtain a rational understanding?

One begins to look for types among the phenomena which at first appear
to be atypical. For instance, the normal, typical action of the heart has such
and such characteristics. There exist individual differences as regards the
duration and intensity of each component of that action and the sequential
rhythm of these components. However, these differences are physiologically

* A lecture delivered at the 4th meeting of the Society of Lovers of the History of
Medicine at Lwów. Archiwum Historii i Filozofii Medycyny oraz Historii Nauk
Poszydłończych 6 (1927).

minute. It is only the morbid action of the heart that yields a tremendous wealth of pictures which are more and more different. It becomes unavoidable that we broaden the observations to include peripheral vessels, capillary vessels, skin, the endocrine glands, the vegetative system, the development relations, etc.

A tremendous wealth of material is produced. It is the task of medicine to find, in this primordial chaos, some laws, relationships, some types of higher order.

In principle, this goal is attained. We know, from the calculus of probability, that even an accidental case, even events lacking mutual relations, can be embraced in certain laws, and so one should not wonder that even these abnormal morbid phenomena are grouped round certain types, producing laws of a higher order, because they are more beautiful and more general than the normal phenomena which suddenly become profoundly intelligible. These types, these ideal, fictitious pictures, known as morbid units, round which both the individual and the variable morbid phenomena are grouped, without, however, ever corresponding completely to them — are produced by the medical way of thinking, on the one hand by specific, far-reaching abstraction, by rejection of some observed data, and on the other hand, by the specific construction of hypotheses, i.e. by guessing of non-observed relations. In this case we use the statistical juxtaposition and comparison of many such phenomena, i.e. that which I would call simply statistical observation, which is the only method of finding the type among a number of individuals. The role of statistics in medicine is immense. It is only numerous, very numerous, observations that eliminate the individual character of the morbid element, and in such abstruse fields as pathology and sociology the individual feature is identical with an event and ought to be removed. However, the statistical observation itself does not create the fundamental concept of our knowledge, which is the concept of the clinical unit.

There come into play here many elusive — as far as logic is concerned — imponderable factors which enable one to foresee (in a way to forebode!) the course of problems which determine the development of a given field of thought and create its style peculiar to the epoch. I venture to call this factor the specific intuition. I am unable to dwell here in more detail on the problem of intuition, as this only becomes possible in the light of the history of science; however, I have to stress here that, without this concept, i.e. if we admitted that the development of science is only a matter of time, technical possibilities and accident, we would never understand science; in the first place we would be unable to grasp why the developmental stages possess
a specific style of thinking, why a phenomenon which is accessible to everybody had been observed at the given moment for the first time, and even almost simultaneously by several researchers. Thus, in a certain developmental stage, there arise certain definite clinical units, and this way of their genesis explains some of their specific features. Nowhere outside medicine does one find so many qualifications, pseudo- and para-, e.g. typhoid — para-typhoid, psoriasis — para-psoriasis, vaccine — para-vaccine, anaemia — pseudo-anaemia, paralysis pseudobulbaris, pseudo-croup, pseudo-neuritis optica, pseudoptosis, pseudo-sclerosis, pseudotabes; next, meningitis — meningismus, Parkinson — Parkinsonism etc.. These specific names are found in medicine, because, with the progress of medical knowledge, it became necessary to single out, in the definite idealistic clinical type, the individual sub-types, e.g. typhoid-para-typhoid which sometimes proved to be completely unrelated: tabes — pseudotabes. The further does medical knowledge progress the more such definitions, such proofs of departures from the original way of dealing with a situation, do and will arise, since the original approach is found to be too abstract, too ideal.

As regards the role played in medical thinking by intuition, even in simple diagnosis, this can be seen best from the fact that we really lack almost always a pathognomic symptom which, by itself, would suffice to determine the clinical state: even the typhoid bacillus cultivated from feces does not prove that the given individual suffers from typhoid fever; the individual may be only the germ carrier. It is only the combinations of symptoms, the habitus, the entire status praesens of the patient that is conclusive. Why even the best diagnosticians are most frequently unable to give a specific basis for their diagnosis; they only explain that the entire appearance is characteristic of such or another disease.

As soon as the medical thinking has found a certain ideal type in an finite plurality of apparently atypical morbid phenomena, it faces a novel problem: how to reduce them to a common denominator, to obtain, by way of analysis, certain common elements, some component bricks from which the observed phenomena could be reproduced. In this way elements of morbid anatomy and morbid physiology arise. However, combinations of the motifs obtained in this way and repeating themselves again and again (inflammation, degeneration, atrophy, hypertrophy, hypofunction, hyper-function etc.) never do adequate justice to the entire wealth of the individual features of the disease. The specific, most characteristic features remain always outside such handling, and they prove that the elements of morbid anatomy and physiology are too general.
This is again the specific feature of medicine. Nowhere outside medicine, in any other branch of science, have its species so many specific features, i.e. non- analysable features that cannot be reduced to common elements. In this way the abstracting process that has been carried very far produces the notion of the species whose fictitiousness is considerably greater than in any other field of science, and a notion of the element (or property) with an equally specific generality. This results in a characteristic divergence between theory and practice in medicine. I have here in mind the divergence between book knowledge and live observations, but not the divergence between medical art and science, because in chemistry also one witnesses a certain incommensurability between science and applied art. However, there no observation can be incompatible with theory, or even be included in it. On the other hand, one can use in medicine the celebrated saying: "In der Theorie zwar unmöglich, in der Praxis kommt es aber vor."

In practice one cannot do without such definitions as 'chill', 'rheumatic' or 'neuralgic' pain, which have nothing in common with this bookish rheumatism or neuralgia. There exist various morbid states and syndromes of subjective symptoms that up to now have failed to find a place and are likely not to find it at any time. This divergence between theory and practice is still more evident in therapy, and even more so in attempts to explain the action of drugs, where it leads to a peculiar pseudo-logic. Not long ago the administration of camphor in the case of hemoptysis was forbidden — and a reason for it was found. Today camphor is recommended, and a 'logical' motivation has been found. Every therapeutic method, including homoeopathy and psychoanalysis, has a 'strict, logical, almost mathematical' motivation, mostly the more exact the shorter its life. It is nowhere easier to get such a pseudolog-ical explanation than in medicine because the more complex the set of phenomena the easier it is to get a law verifiable for a short term, and the more difficult it is to reach an embracing idea. It is in medicine that one encounters a unique case: the worse the physician the 'more logical' his therapy. The point is that, in medicine, one is able to simulate almost everything, which proves that, up to now, we have indeed failed to explain anything.

Beside these fundamental notions of species and element, medical thinking possesses also the equally specific notion of the relationship of morbid phenomena. This extremely complex field presents an epistemologically unique picture. Along with the natural sciences, medical thinking recognizes causal relations (though it is generally accepted that a physician says always 'afterwards', but almost never 'because of that'). Just as in biology, the conditioning of phenomena in medicine can be developmental, correlative,
substituting, synergetic and antagonistic. A completely specific factor, which explains the morbid phenomena, one finds in medical thinking in the notion of internal disposition and of external substrate, i.e. of conditions which, as if in potentia, comprise the given morbid phenomenon. Besides, we have the epidemiological grasping of morbid phenomena and, by far not the last — teleology. Thus medical phenomena are mutually related by means of a tremendous number of relations, as the result of, and compensation for, their original atypical character.

However, this plurality whose elements are so multiply conditioned is irrational if we examine it as a whole and consistently from the same standpoint. We admit causal relationships, but the result is never proportional to the cause, nor is it always the same. The action of the pathogenic cause is a resultant of its intensity and disposition, i.e. to the causal relationships are added the dispositional factors which are incommensurable with the former. However, even if both of these active series are taken into account, one cannot deduce anything in medicine, since an antagonistic reaction may appear. For example, 'dermographismus albus' points, according to some, to the hyperfunction of the suprarenal gland, while, according to others (with equal logic), to the hypofunction, in view of the antagonism between skin and intestines. Schultz's law of the action of stimuli, the different action of the small and medium doses of atropine, the variable reaction of pupils in the case of anaesthetization — such are further examples of this irrationality.

Even a thorough familiarity with the anatomy and physiology of the bladder and a thorough familiarity with the tubercular processes would not enable one to foresee the interesting phenomenon, viz. that bladder TB recedes after resection of the tuberculous kidney. Similarly, even familiarity with the physiology of speech would not enable one to deduce the fact that one can learn to speak even after the complete removal of the larynx. According to the classical theory of the Wassermann reaction one may deduce that, when working with an active serum, one would obtain more negative results, whereas in reality a contrary result is obtained. In this case medicine has its own motivation which, however, does not lie along the line of classical theory, but requires a change in mental attitude.

This is what one encounters in the case of any medical problem: it becomes ever and ever necessary to alter the angle of vision, and to retreat from a consistent mental attitude. Only in this way does the world of morbid phenomena, which is irrational in its entirety, become rational in its details. Just as, on the one hand, the far-reaching abstracting action enables medical thinking to find types among atypical phenomena, so also, on the other hand,
it is only the renouncing of consequences that enables one to apply a law to irregular phenomena. This results in the incommensurability of ideas which develop from the varying ways of grasping morbid phenomena and which gives rise to the fact that a uniform understanding of morbidity is not possible. Neither cellular nor humoral theory, nor the functional understanding of diseases alone, nor their 'psychogenic' conditioning, by themselves will ever exhaust the entire wealth of morbid phenomena.

However, much as it is impossible to get in medicine an idea that would embrace the entirety, like atomism in chemistry or energetics in physics, yet one witnesses that a new methodical idea, a certain keynote for grasping medical phenomena, comes to the fore. This is a specifically temporal and dynamic grasping of morbid phenomena. The object of medical thinking — illness — is not an enduring state, but a process which changes continually, and which has its temporal genesis, its course and decline. This scientific illusion, this fiction, this individual entity created by abstraction based on statistics and intuition, the entity called the disease which is virtually irrational, elusive and undefinable univocally, becomes a substantial unit only when grasped temporally. Never a status praesens, but always only the historia morbi actually create the clinical unit. The former yields at the very most a syndrome of symptoms, such as Banti syndrome or Horner's syndrome, which modern medical thinking does distinguish carefully from the disease. This historic, temporal nature of the notion of the disease is unique. Since the disease is a change, which develops in time, of life functions which have likewise their temporal course, it is obvious that, being a sui generis variety of life variations, it is doubly dependent on the instant. If one may use a comparison from a distant field, the disease has a relation to normal functions just as acceleration has to velocity. Life as such has its temporal course. The course of the disease takes place within that course, being somewhat independent of it. A child develops in accordance with a known pattern. Simultaneously, its TB develops in its own tempo and according to its own laws. Thus this disease obtains its double, or practically quadruple genesis.

Thus in the first place the pathogenesis of a single definite case; its disposition, diathesis, constitution or habitus, its infection, original symptom, the origin of allergy, the development of pathological symptoms etc. I would call this the detailed ontogenesis of the disease. Next the general pathogenesis of the single TB case, i.e., for example the disposition factors and the progress of TB or typhoid or uric acid diathesis in childhood, puberty, climacteric, etc. This I would call the general ontogenesis of the disease. Thirdly, the
independent history of the disease in a certain social or geographical environment, the history of a certain epidemic or of a certain degeneration. This I would call the detailed phylogenesis of the disease. Finally, the independent history throughout the ages of the disease, its appearance in mankind and its changes. This I would call the general phylogenesis of the disease. I do not know any other field of scientific thought in which the fundamental idea would allow of so many different genetic investigations. Embryology or palaeontology, history or sociology — recognize only a development in one direction. In pathology, two developmental series are combined: the ontogenetical and phylogenetical development of the living creature and the development of the disease. This historic formulation of the disease idea becomes more and more clear-cut.

I wish to point to two relevant, fully modern and fertile ideas: that of 'hygieogenesis' and of the latent infection ('inapparente Infektion' — Weil); and also to the idea of the latent disease, e.g. *lues latens*. The relevant processes can be numbered neither among the former ideas of health nor among the ideas of disease. In their light, health is a certain mutual attitude of the patho- and hygieogenetic processes, and any other attitude in any direction is a disease. Since the most different organs and glands can replace one another, and certain diseases compensate one another, producing a more advantageous state, one should, to all intents and purposes, define, or specify, health consistently, though paradoxically, as an illness which is the most profitable at a given moment. Thus a specifically dynamic grasping of the subject does arise, where, instead of constant causes, we have mutually influencing processes. The relations between these processes are different and incommensurable, depending upon the always necessary change of the viewpoint. If one adds to it the specific abstract nature of the idea of the clinical unit, we obtain a general picture of the medical way of the formulation of the problem.

Let me use a figurative comparison: medical thinking differs in principle from scientific thinking in that it uses Gauss's coordinate system, while the latter uses the Cartesian system. Medical observation is not a point but a small circle. It is placed in the system of coordinate straight lines inclined to one another at a constant angle, but in a system of optional, mutually intersecting, curves which we do not know closely.

A certain correction is introduced into this picture by the fact that, strictly speaking, the multiplicity of medical phenomena can be only approximately rendered by means of Gauss's system since its points are not univocally determinable. To all intents and purposes, scientific thinking uses, for small
ranges, the Cartesian system, and for large ranges Gauss's system (as in the theory of relativity). On the contrary, medical thinking uses Gauss's system for small ranges, while in the entirety it does not find any consistent and rational way to grasp phenomena.
ON THE CRISIS OF ‘REALITY’ [1929] *

LUDWIK FLECK

In exploring the sources of cognition (Erkenntnis), we frequently commit the mistake of regarding them as much too simple.

We forget the simple truth that what we are acquainted with (Kenntnisse) consists rather of what we reach by learning (Erlerntes) than of what we arrive at by knowing (Erkanntes). Yet this is a momentous fact, since in the short span from the teacher’s mouth to the student’s ear the content of the knowledge transmitted is always slightly distorted. Thus, in the course of decades or even centuries and millenia, divergences develop to an extent that it sometimes becomes doubtful whether anything of the original has been preserved at all.

In the circumstances the content of knowledge is by and large to be evaluated as a free creation of culture. It resembles a traditional myth.

Unfortunately, however, we characteristically regard old, habitual trends of thought as particularly self-evident, so that no proof is required or even admissible for these. They constitute the firm foundations on which further construction is allowed.

In addition to this, the physiology of our cognition (Erkenntnisphysiologie) has a second, important characteristic, by virtue of which every new cognitive activity depends on the previously accumulated store of knowledge, since the weight of that which is already known changes the internal and external conditions of newly acquired cognition.

In this manner three systems of factors come into being, that contribute to every process of cognition (Erkennen), are interrelated and interacting: the burden of Tradition, the weight of Education, and the effect of the Sequence of the acts of cognition.


These are social factors, and any new epistemology must, therefore, be brought into a social and cultural-historical context, lest it seriously contradict the history of cognition and the everyday experience of teacher and student.

At no time do we resemble a blank page, nor are we in a state of a tabula rasa as is the screen before a film is projected on it. Cognition has no discernible beginning, certainly not at the moment of birth or even in the womb, because the capacities for feeling, and feeling as such, originate in a parallel and synchronous way through interaction. It is equally impossible to establish the phylogenetic beginnings of cognition.

In individual life there take place not just one but numerous epistemological births and embryonic developments. We undergo rebirth for each new situation and bring with us a more or less complete mechanism of birth and more or less ready-made predispositions that are decisive for our reactions and for the contents of our cognition.

Wherever and whenever we touch on something, we are always in the very midst and never at the beginning of cognition. Therefore I am at a loss to see how one could possibly construct epistemology out of sensations as building blocks.

An experienced teacher has found that only a small minority of students independently notice something new without having their attention explicitly drawn to it, and that even then only a few see it immediately as it is shown to them. They first have to learn to see it. Even the adult, facing something new for the first time — perhaps an abstract picture, a strange landscape, or looking through a microscope — ‘does not know what he is supposed to see’. He is looking for similarities with something familiar, thus overlooking the new, which is incomparable, specific. He, too, must first learn to see. How many examples could be adduced here from the history of science! And yet, it is just this ‘seeing’ that one first has to learn, which makes for the progress of any science, the progress which thus is again and again given its social imprint.

If one wanted to solve the problem of the genesis (Entstehung) of cognition by the traditional method as the individual concern of a symbolic ‘human being’, one would have to subscribe not only to the proposition ‘nihil est in intellectu, quod non fuerit in sensu’ but also to its reverse form: ‘nihil est in sensu, quod non fuerit in intellectu’. And beyond this one cannot make any progress. Consequently I do not know why and wherefore should I make a difference between a first and a second reality, as Riezler, among others, describes them.
Just so, the social factor in the genesis of cognition must not be disregarded.

Thus every thinking individual, being a member of some society, has his own reality in which and according to which he lives. Everybody has even many, sometimes contradictory, realities: the reality of everyday life, a professional, a religious, a political, and a small scientific reality. And secretly he has a superstitiously fateful personal reality that renders the actual I exceptional.

Each cognition, each system of cognitions, each entry into the social realm has its own corresponding reality. This is the only correct position.

Otherwise, how could I understand that, e.g., a person with a humanistic education will never completely grasp the science of the naturalist? Or even the theologian? Should I regard the others as fools, as regrettably happens so often?

They encounter the greatest difficulty not in the solutions of the problems, but in comprehending the origins and the significance of the problems as such; not the concepts but their evolution and function.

Every knowing (Wissen) has its own style of thought with its specific tradition and education. Out of the almost infinite multitude of possibilities, every way of knowing selects different questions, connects them according to different rules and to different purposes. Members of different scientific communities live in their own scientific and also professional reality. In their daily lives these people can get along with each other in perfect harmony, for they may have a common everyday reality. There are cultures, as e.g., the Chinese culture, which in important fields, such as medicine, arrived at quite different realities from those of us occidentals. Shall we punish them for this with pity? They had a different history, different aspirations and demands that are decisive for their cognition.

For cognition is neither passive contemplation nor acquisition of the only possible insight into something given. It is an active, live interrelationship, a reshaping and being reshaped, in short, an act of creation. Neither the 'subject' nor the 'object' receive a reality of their own; all existence is based upon interaction and is relative.

Just as everything that is socially conditioned, that is perceived, has its own life, independent of the particular individual, so it has its special characteristics, its style in time and space, and consequently its own destiny.

Even the schizophrenic from whose asocial momentary reality spring utterances such as '1 – 2 – 3 this is pharmacy, this is boxhedge, Bucks, Rio de Janeiro' employs concepts of social origin. Yet his reality remains inaccessible to others – and probably in the next moment also to himself. It is probably of no enduring importance to anybody.
However, there exist realities with style (stilvoll) that are founded upon serious, continual work by large groups and great men, in the spirit of which people live (or for the sake of which they die). They develop, flourish, endure, waste away — leading their own lives, such as forms of government or social arrangements. The relative independence of the cognized from the individual is well illustrated in the fact that different individuals frequently make the same discovery or invention simultaneously but independently from one another. Cognitions are formed by human beings, but also conversely they form their human beings. It would be simply foolish to ask here what is 'cause' and what 'effect'.

Once upon a time there existed a Great Science that was related to almost every branch of knowledge in that period, that was based upon solid theoretical-philosophical foundations, and that had the greatest influence on political-economic and personal life. I believe no such universally predominant science had ever existed before or after; in all areas it explained the past, defined the present and predicted the future. This science was called astrology. Nowadays it leads only a pitiful existence in the thinking of some uneducated freaks, not comparable to its erstwhile greatness, similar in a way to that of our lizard to the dinosaurs. It was replaced by a differently constructed system of thinking by society, namely by the natural sciences. Surely there had always existed thinking typical of the natural sciences. It was to be found among the artisans, the seamen, the barber-surgeons (Wundärzte), the leather-workers and saddlers, the gardeners and probably also among children playing. Wherever serious or playful work was done by many, where common or opposite interests met repeatedly, this uniquely democratic way of thinking was indispensable.

I am calling the thinking which is typical of the natural sciences democratic, because it is based upon organization and control at all times, it rejects the privilege of divine origin and wants to be accessible and useful to everybody. Yet we have learned from experience that every democracy contains its little untruths, since what is wanted is an impressive, majestic government — not just a useful and wise one. This is why there are medals, titles, flags and presidents. Therefore the natural sciences have their own natural philosophy and their Weltanschauung.

When talking of natural sciences, one often forgets that there exists a living scientific practice and, parallel to this, an official Gestalt of Science on paper.

These two worlds, however, are frequently as different from one another as are the practice of democratic government and its official theory. Certainly
this cannot be helped, but this natural disharmony gives rise to serious misunderstandings. One mistakes the natural sciences as they are for the natural sciences as they ought to be, or rather as one would like them to be. But the practice of natural sciences cannot be learned from any book, for silence is kept about its ways and means. It contains the small 'divergences' which are not taken into account, the 'exceptions' that should only confirm the rule, the 'accidental', the 'unessential', the 'unavoidable mistakes'. These are the usual figures of speech that are always available when one wants and has to preserve the regularities.

These phrases are indispensable, even though one squeezes the rich, free stream of possibilities through the narrow passage of conceptual and material instruments (based on the responsibility of the fathers).

All this gives occasion for a distinct, though slight, change in comparison with what is officially demanded. The slight changes are integrated, and in this way they become larger, for they are not chaotic but bear the imprint of tradition, of the scientific moment, and of the researcher's personal style of thinking — as everybody knows in practice but forgets in theory. For the next generation they already become facts.

Everyday practice also teaches us that even the 'simplest' (i.e. today simplest) activity, such as measuring and weighing, is an art that has to be taught and that is sometimes never learned. Even the Wassermann reaction, so clearly worked out and frequently applied, is ultimately an art whose value depends much more on the practitioner than on the method by which it is carried out — as was recently stated by one of the leading serologists (Eisenberg).

Not only the ways and means of the solutions are subject to the scientific style, but also the choice of problems, and at that even to a higher degree. But the course of science is immensely influenced by the sequence of the solutions, for it determines the development of technical possibilities, the education of the researchers of the future, and the formation of scientific concepts and comparisons.

Here it is needless to adduce examples, since everybody knows thousands and could mention entire series of cognitions (Erkenntnisreihen) bearing the stamp of the epoch and the imprint of the scientist's personality on the method and style of their solutions. If the individual was strong enough, and his qualities were not only those of a pioneer but those of a leader, then his style became universally accepted into the body of science. In this way the scientific style and recognized scientific practices became codeterminant agents in the formation of scientific reality. How much conventionality, tact,
intuition is contained in this agency follows from the simple truth that there may exist too much consistency that leads to one-sidedness, and too much criticism that causes sterility. One must practice moderation; the goal of the research determines this. Even weighing and measuring are done in different ways, according to the purpose to be served. And even though the 'greatest exactitude' today appears applicable (though uneconomical) for all purposes, I still believe that quite a few laws, such as e.g. the law of Boyle and Mariotte, the law of the conservation of matter, or the laws of classical mechanics, would never have been discovered, had the inexactness of observation necessary for the discovery and measurement been impossible. But it is not indifferent whether a law was not formulated at all, or whether it is 'supplemented' or 'restricted' after it has for many years influenced the formation of reality and of men.

Even more striking is the purpose-dependency of scientific truths in areas in which nowadays one arrives at divergent and not exchangeable truths, depending on the purpose of the investigation: for example in bacteriology, where there exists a botanical-genetic and a medical-epidemiological aspect. As an illustration of the epidemiological standpoint I would mention Professor Friedemann's article on the problem of scarlet fever. (Klin. Wehr., (1928), No. 48, 2280). In the author's opinion the convalescents are no longer infectious after three consecutive negative bacteriological tests.

There have also been, though, some divergent pieces of evidence. At the Königsberg congress on scarlet fever Eikeles reported that three out of seven cases came from patients who had been discharged after three negative tests. I would venture to surmise that the explanation of this result which is at variance with our and others' experiences lies in the method applied by Eikeles. For it seems striking that Eikeles found haemolytic streptococci only in 84% of fresh cases of scarlet fever, whilst almost all other authors were able to prove the presence of haemolytic streptococci in nearly 100% of the cases.

Eikeles indicates that he regarded as haemolytic only those streptococci which showed an absolutely indisputable haemolytic ring on the microscopic slide. In view of the practical purpose of these examinations it seems to be more correct in doubtful cases rather to diagnose the presence of haemolytic streptococci than the absence. For if we disregard them, any error is liable to have serious consequences, and it appears to me likely that, as a result of his rigorous rejection of all doubtful cultures, Eikeles indeed disregarded scarlatina streptococci that were actually present.

Thus the overly rigorous and therefore one-sided position is useless for practical epidemiology. Here the 'unavoidable mistake' is compensated for purposefully and knowingly by another one.

Yet, in addition to this dependence on the special purpose of an investigation which has a contributing effect on the scientific (as on every other)
ON THE CRISIS OF 'REALITY'

reality, there also exists a general effect of observation and investigation as such:

The quantum postulate means that every observation of atomic phenomena constitutes a non-negligible interaction with the measuring instrument, and that no independent physical reality in the ordinary sense can be ascribed either to the phenomenon or to the means of observation. Generally speaking, the concept of observation has an element of arbitrariness, in that it essentially depends on what objects have to be included in the system that is being observed. (Bohr, Naturwiss. (1928), No. 15).

This statement applies to all observations of any phenomenon whatever, but the mutual relationship with the means of observation is relatively negligible in most cases. Yet, if the 'treatment' of the phenomena, with whatever instruments, goes on over centuries, will the effect not become significant? To observe, to cognize (erkennen) is always to test and thus literally to change the object of investigation.

This is the day-to-day praxis of science. Here the social and the historical-traditional element is predominant. In great creative moments, however, the newly emerging science is simply an artistic creation that one can only admire and never 'prove' and determine 'objectively'. For there never existed nor exists a scientific demand for fundamental changes because each moment already has a superabundance of fundamental content. And at the given moment a yardstick for greatness is never to be found.

I am thinking, for instance, of Vesalius' idea to dispense with a completely elaborated, a hundred percent consistent, highly respected science, and consistently to build a new one from confused, unstable, changeable, intertwined masses of flesh, such as the scholars of that time would have considered it to be beneath their dignity even to touch.

For a correct appreciation of this we should call to mind the moment when for the first time we stood before a corpse. Did not the medical examiner then appear to us as a sculptor who is just modelling the intended structure of the body, carving it out of the corpse, throwing away kilogrammes of 'unimportant' matter in order to bring out thin, hardly visible threads of tissue that he declares to be of sole importance, giving it high-sounding names and thus calling it into existence for the first time? Were we not at that time much more aware of our little bit of book-learning of anatomy than of this practical art of dissection?

Today's pathologist is just an imitator of his teacher. But Vesalius had no teacher. He had to perform his modelling according to his own intuition, battling against the far more obvious knowledge of the mighty scientists of
his time, against his own mystical dread of the corpse which is still discernible in the grouping of his figures, and against his deeply ingrained veneration for Galen and tradition, which sometimes obscures his judgment.\(^1\)

Thus he did his shaping and cut off everything that became inessential for a long time to come: the fatty and connecting tissue — and old emotional contexts that fell away as 'superstition' thanks to his work. Thus he formed the structure of the body and scientific concepts.

This was a creative act, unproven by bookish syllogisms or by intellectual reasoning. The reigning science had no demand for this, since it wanted to persist in the wealth of thought of its *Anatomia imaginabilis*. Thus, for instance, Bartholomaeus Eustachius wrote in 1516 that he would rather err with Galen than accept the truth from the innovators. Johann Phil. Ingressias (around 1600) wanted "*in quibus omnibus veteres defendere interpretando, elucidare atque excusare . . .*". And the ancients have been defended by thousands of cunning devices. Bauhin, e.g., attributed to Galen his own discovery of the Valvula — only in order to avoid taking a stand against him.

This was no struggle about details, about 'facts'; what was at issue was the familiar reality itself, the sacred faith that needed to be defended, not to be proven. Then comes an innovator, blasphemously relying on his own powers and through simple labour constructs, controls, and develops a science in place of the ready-made immutable doctrine of the divinely inspired master, which had so many deep connections with the totality of knowledge. As against this, how poor was the anatomy of Vesalius!

This was a battle for democratic values, and Vesalius had created for it the method, the style of thinking, and thus he had laid the foundations of the democratic general reality that was free of deep mysticism, of sentimental poetry, of grand affections.

*Natural science is the art of shaping a democratic reality and being directed by it — thus being reshaped by it. It is an eternal, synthetic rather than analytic, never-ending labour — eternal because it resembles that of a river that is cutting its own bed.*\(^2\)

That is the true, living natural science. One must not be oblivious to its creative-synthetic and social-historical elements.

Its official ideal image is different: it is naive and beautiful. The absolute, Riezler's third reality, belongs within this context. The first one is the life and work of the researcher, the second is his religion.

It is beautiful if an artist in the course of his work has a vision of his own creation in its unattainable perfection. But it is naive not to know that this vision is not something absolute but that it mainly depends on the subject
and on the moment of time. One must not forget that there exists no fully completed science but only one that is becoming. Every solution constitutes a fresh problem, just as conversely every formulation of a problem already contains part of its own solution. Several fields of natural science are lying fallow after years of intensive development, such as anatomy today, or astronomy that was so much more alive in Kepler’s and Tycho Brahe’s times. They appear to be done with, dead. But one day they will be revived, illuminated from a different angle, taken up again with new conceptions, desirable due to new requirements — and then they will be as fresh and “as marvellous as on the first day” (herrlich wie am ersten Tag).

We approach the ideal, ‘absolute’ reality not even asymptotically since it changes incessantly, renews itself and moves away from us at the same pace as we are advancing. It is an imaginary ideal whose content is solely determined by negation, by longing for something else. Does it not possess as little or as much reality as the ideals of beauty or goodness? Is it not just as dependent on time, place, culture and person? A few centuries ago something else was regarded as good as well as true than what we consider good and true today. Have we arrived at the terminal point of evolution today? Surely not. Fortunately not. But even if this were the case, our ideals would be conditioned by their historic development, therefore never absolute.

The striving for to know, to gain knowledge (Erkennen) of the absolute is based upon a strange misunderstanding: is it not the same as if one wanted to open up a pristine jungle, without changing its condition?

It is impossible to deduce an absolute reality from the laws of natural science, whose contents cannot be derived from the mere philosophically trained intelligence of the contemporary European. After all, there also exist ethical laws, commercial customs, political abuses that are not derivable from any contemporary intellect. Should I believe also here in an ‘absolute existence’, in a deus ex machina whose image is reflected in those laws and regulations? I see no principal difference, for there is no law without its exceptions, they are all conditioned by culture, therefore dependent on development, replaceable by others; they are sensible or senseless, according to the critic’s viewpoint.

Of what ought the absolute reality to be independent? If one wished it to be independent of man, one ought to consider that in this event it would also be of no use to man.

If one wished it to be independent of the individual, one should construe it as socially conditioned, and therefore dependent on the collaboration and communication of many individuals, as many as possible. One should
construct it democratically, taking into account that it would then become much less dependent on time, because the collective (die Masse) develops much more slowly, but also more consistently. This is the way of the natural sciences.

If one wanted to make it independent of so-called ‘appearances’, one ought to consider that all ‘appearances’ are nothing but the expression of the interrelations between a number of elements of cognition. The same expression, when magnified, will become what is called an ‘iron law’. There exists no fundamental difference between ‘appearance’ and ‘truth’, the difference is one of development (Entwicklungsunterschied).

If an object appears small from afar and large when close by, generally one should not ask how it is ‘in reality’. The natural sciences deduce from this perception of appearances the laws of perspective and solve the question through comparison with a yardstick at the same distance as the object. This is, of course, not the expected solution, for now one could ask how large really is a meter-stick, as large as I see it in the distance or as it appears close by? And this, just like all insistence on the ‘essences and things’, like all search for the ‘thing in itself’ (Dinge an sich), would not be natural science at all, because there can be no democratic, generally applicable uninfluenced (affektfrei) answer to it. This question demands the miracle of faith, the experience, related to the singular person (Als-Ich-Erleben); but scientific thinking does not provide for this, as otherwise it would become undemocratic and useless in everyday life.

Finally, if one wanted to understand ‘absolute reality’ as the most comprehensive sum-total of reality (Sammelwirklichkeit) from which every other reality could be derived, then one would either have to renounce the law of contradiction or to admit a general principle of ‘reciprocal uncertainty’. Our logic would have to be revised and the future will have to decide on that.

Therefore, I believe one should highly esteem, even love, the ideal of an absolute reality as a vision of the next working day, but it must never be applied as a yardstick for the previous day. For this we need rather an ‘image of knowledge’ (Wissensanschauung) than a Weltanschauung.

At the present time we are so fortunate as to witness the spectacle of the birth, the creation, of a new style of thinking. Let us give free rein to the creators, the experts!

Sooner or later much will change: the law of causality, the concepts of objectivity and subjectivity. Something else will be demanded from scientific solutions and different problems will be regarded as important. Much that has
been proven will be found unproven, and much of what was never proven will turn out to be superfluous.

Education for life will be different; life and art will be given a different form. A new, up to date reality will be created.

What is the use of awkward metaphysics, if tomorrow’s physics will transcend all phantasy?

Let us leave a free hand to the experts and reserve room in our thinking for the future!

NOTES

1 Cf. *M. rectus abdominis and Mm. scaleni* in Tables Nos. 3 and 6 of his *Anatomy*.

2 Vesalius’s example is very simple. Just compare with it the tortuous roads of the birth of chemistry (phlogiston!), of its development in the materialist age, and today. How much could have been quite different, if only the discoveries had been made in another sequence. Surely one could have formulated entirely different concepts, e.g. the concept of the element, could have connected them in another manner, i.e. could have built a different reality without getting into conflict with any ‘fact’.

Weight – so important for such a long time – was introduced by Lavoisier as something self-evident, without any ‘proof’ or substantiation, despite the fact that at the time Spielmann (1763) rightly rejected drawing any conclusion from a loss or increase of weight, “since up to now the cause of weight is still unknown to the physicists”. Also Sage was right, according to the state of science in those times, in that he regarded Lavoisier’s theory of the composition of water as untenable, because “then the inflammable air must be regarded simultaneously as the son and the father of water”.

Lavoisier simply created his own concept of an element – not at all the only possible one – and his own concept of composition. Subsequently both proved generally acceptable and have remained so to this day.

3 I.e. descriptive, not normative laws of the commercial or the political reality.
SCIENTIFIC OBSERVATION AND PERCEPTION
IN GENERAL [1935]*

LUDWIK FLECK

Until quite lately the following conviction prevailed among scientists, expressed in Poincaré’s sentence: “if a research worker had infinite time at his disposal, it would suffice to tell him: Look, but look well”. Our entire knowledge would allegedly emerge out of the description of his observations of all events.

This conviction includes a number of foundations which are impossible even today. Can observation indeed be only either ‘good’ or ‘bad’ (or, ‘better’ or ‘worse’) and does every ‘good’ observation lead to the same results? Is it sensible to talk of the ‘descriptions of all events’, just as if these descriptions were always fundamentally additive, and necessarily did yield, all of them, a certain whole representing some sense? Does the concept of ‘the whole of science’, ‘one general science’ have any sense at all? Is an isolated research worker at all possible, even if he had an infinite time at his disposal?

In matters of this kind the theorists rely mainly on the experience of the past century, predominantly on the experience of physicists. The problem of observation appeared at that time to be much more simple than it does at present. It was believed that, e.g., observation does not in principle affect the state of the observed object. Today, it follows from the quantum theory that every observation of atomic phenomena does influence their course. However, the complex nature of the problem of observation comes to the fore only in the biological sciences which are less deductive and less abstract.

My own profession makes me carry out daily observations of things which are very simple from a certain standpoint: of microscopic preparations. When I look at the microscopic preparation of, e.g., a diphtheria culture, then, to use common parlance, I see only a certain number of lines having a certain specific structure (or colour), a certain form and a certain arrangement. However, it would be futile on my part to try to describe these three

* Przegląd Filozoficzny 38 (Warsaw, 1935).

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elements of the image so as to render in words, univocally for the layman, the image of the characteristic form which is seen by the trained observer, but which the layman is simply unable to see at the beginning. Nevertheless, after a short period of time, almost all of the pupils acquire the ability to perceive it, and reach results which are consistent (at least to a large extent). Thus one has first to learn to look in order to be able to see that which forms the basis of the given discipline. One has to acquire a certain experience, a certain knack, which cannot be replaced with verbal formulae. Hence a completely axiomatic edifice of science is not possible, because no words or sentences suffice to render their complete contents. Such an edifice is understandable only for a specialist, and for a layman it is not the equivalent of the given branch of knowledge. The indispensability of distinguishing the specialist from the layman, the necessity of a certain experience and of the acquisition of a certain knowhow introduce a certain alogical factor into science.

Still more vivid is the necessity of a specific training to acquire the ability to perceive certain forms, e.g. in dermatology. In this field, a layman who is capable of carrying out good observations in other fields, say, a specialist in bacteriology, does not differentiate and recognize dermatological changes. At first he listens to the descriptions of dermatologists as if they were fairy tales, much as he has the object described lying in front of him.

Thus there exists a necessity of separate training in the perception of specific forms from various fields of science, and it is not possible to render these forms univocally by describing them with the aid of words of a certain general language. Hence one cannot talk in general about good and bad observation, but only about an observation which is consistent with a certain branch of science or an observation which is not consistent with it.

The art of observation is not a general one; it does not include all fields of science at the same time. On the contrary, it is always limited to one field only. I knew an eminent surgeon, specializing in the abdominal cavity, who needed only just a few looks and a few touches of the abdomen to diagnose the clinical state of the appendix vermiformis almost infallibly, sometimes in cases when other medical men 'did not see anything'. The same specialist could never learn how to distinguish under a microscope mucus strips from the hyaline cast. I also knew a bacteriologist who was an assistant lecturer in a large university; he perceived and recognized ever so minute morbid changes in inoculated animals, but was unable to tell a male mouse from a female one.

An observer who lacks training in a certain field is unable to provide a useful description. He will at best provide a longish description containing
very many details most of which are inessential or in general accidental, but will fail to give characteristic features and to stress the fundamental characteristics. The picture of his observation reminds one of an overexposed photograph: it is traced, without contrasts. The background is not empty or discreetly receding, the form does not stand out against it, it does not stand out in relief, does not 'emerge' from the background.

I purposely give here examples of people whose professional occupation consists in observing, but one would be able, using everyday life, to adduce a large number of examples of closely limited sharpness of sight, combined with a limited blindness with respect to some different phenomena. Dressy women perceive very minute features of the materials of clothing, and are often blind with respect to great natural or technological phenomena. In such cases taken from everyday life the foremost role is played by emotional factors which result from the entire mental life of the given person and which produce the directed readiness to certain perceptions. In the scientific observation there exists also a defined readiness toward some observations, but it is brought about in the first place by a certain training, by a certain scientific tradition.

One could believe that the hypothetical research worker of Poincaré, while having an infinite time at his disposal, would be simply a specialist of all trades, of all sciences, thus being able to perceive specific forms in all fields. However, this is psychological nonsense, since we know that the formation of powers of perceiving certain forms is accompanied by the vanishing of the faculties of perceiving some others. A physician who examines a sick person is often completely blind to his being dirty. This specific blindness — more or less intended — enables the medical observation to be carried out just as it prevents the beginning of disgust. When reading, we often disregard letters, being busy with words and sentences. When proofreading we disregard words, being absorbed by letters. A physician who is professionally trained in observing the ever-changeable and whimsical forms of pathology is, as a rule, a poor observer of continually recurring regular phenomena; but is not interested in them, nor does he notice them, nor ought he to notice them if he is to be a good pathologist. A scientist does not, as a rule, notice sociological phenomena which cannot even be demonstrated to some scientists. Thus there exists a continuous transition from the intended, making no mention of some forms, up to the inability to perceive them. To catch sight of any definite form from any field one has to be in the state of a specific mental readiness which is likewise composed of a more or less compulsory silence concerning the possibility of other forms. Every observer is, as a rule,
in the position of a man who is faced with a figure obtained from an ink-blot: one can arrange various forms from it, and one involuntarily arranges (sees) such forms as correspond to the specific readiness of the person who looks. We guess some details, we make no mention of others, and it is in this way that a definite image arises.

The Roman capital A may have various shapes. To identify it one has to have some experience. Its arms may be of an equal or unequal length, they may be less or more opened, straight or curved. The cross-bar may be placed higher or lower, it can be straight, longer or shorter, it can be composed of two parts forming an angle, or else it can be a curve. The arms can be two parallel straight lines joined at the top by a straight line or an arc; they can be also two straight lines diverging upwards, but joined by means of an arc or a straight line. In other words, one can introduce the most varying modifications in the structure of this letter, and still it will be the letter A. One cannot describe after what considerable modifications we shall cease to see this letter as A; this depends on the person who looks, and on the environment in which it will find itself: in a word which is composed of other, suitably stylized letters, we shall recognize it easier, because we shall be disposed towards the fact that we have to deal with a letter and that this letter is over-stylized. We would hardly identify it if it were isolated from other letters. To perceive a certain form we need therefore a special readiness whose basis is given by the usual education, while the excess required in cases remote from the ordinary ones must be added by special circumstances. It is important to note that the readiness has its own laws which we must obey during the process of perception: thus the letter A might have its cross-bar at various distances from the base, between 1/3 of the letter height from the top and 1/3 from the bottom. However, if this letter is composed of two arms which diverge upwards, and if these arms are connected at the top by a straight line or an arc, then the cross-bar must be placed in the upper half of the letter; if it is in the lower half then A will be recognized with difficulty or not at all.

Even the simplest observation, e.g. observing demonstration experiments in school, requires a certain mental readiness which, anyway, can be produced by a few gestures and a few sentences. Every teacher has been repeatedly convinced that, when asked “what do you see?”, the pupils describe often observations which are quite fantastic to the teacher, by connecting into a separate form that which the teacher believes to be accidental and inessential, and by leaving out precisely the essential and most important elements. A major part of the child’s education consists precisely in teaching it to see what
the adults do see, while at the same time losing the true child's 'many-valued' ability to see fantastic forms. Who knows what amount of future knowledge, how many observations which science will admit in the future, are hidden in these fictions? Some entoptic phenomena, such as *muscae volitantes*, are much more often observed and known among children than among adults, and one should admit that science has discovered here a phenomenon which, according to elementary education, ought to be ignored as a rule.

Somebody would object here that observing a definite form is not tantamount to the scientific observation proper, but, at best, a psychological necessary evil; that the research worker ought to perceive only the simplest elements of the picture, which are directly given, of which the form is automatically composed, or else is successively arranged as a hypothesis *sui generis* which is more or less subjectively coloured; that these simplest elements are incontestable for 'normal' people, that this can be described completely, and that their descriptions cannot contain any specifically tinged mental readiness.

I believe that it is aimless and unnecessary to discuss this 'atomistic' standpoint in principle. The adherents of direct elementary data in fact discredit themselves when they are unable to establish among themselves the nature of these directly given elements. What are these direct data if one has to look for them? In what way are they given directly if one has to argue about them to such an extent? It suffices to compare, *Erkenntnis* 2 (p. 432), with 3 (p. 215), in order to convince oneself to what extent Carnap became complicated with his *Protokollsätze*, and to establish the complete barrenness of this matter. It leads necessarily to dogmatism or relativism, and in both cases it fails to provide any new research possibilities. It is my opinion that the only theory that is of any value, is one which creates new fields of research, new mental possibilities, and not a theory that closes the path to future research.

Nevertheless I intend to consider specifically, using the example of the observation of diphtheria germ preparations, whether a univocal description, using expressions of some general language, is possible and in what way does science in reality solve the problem of observation and description in this case.

The picture of the microscopic preparation of a diphtheria-germ culture, dyed, e.g., according to Gram, is, to a layman having some general education and acquainted to a certain extent with the microscope, a set of dark lines on a bright background. The layman will also see any flaws in the glass, any points which are dye precipitates, any accidental figure arranged from these points, e.g. a jagged island. It is impossible to obtain from him a description
that would resemble the specialist’s description, without using leading questions [are the lines of the same thickness throughout the entire length? Are they straight? Are they dyed uniformly? Are they parallel to one another? etc.]. It is likewise impossible to divide the images which intrude themselves [just as the image of the island] without some suggestions [e.g., compare with other places, disregard this detail because it is accidental; this is a dyeing error, etc.].

The poorer the education of the observer-layman or, strictly speaking, the farther it lies from the education of the specialist in our field, the more distant is the picture he sees from what the specialist does see, the more distant also the description. A man not acquainted with the microscope will not perceive the picture at all, he will not look into the eyepiece, he will not catch hold of the light, he will not focus the preparation. Following the suggestion of the form of the microscope, he will search for the object on the stage, he will turn the mirror toward himself and look into it. If he knows that one must look into the eyepiece, he will see his own eyelashes, he will accommodate to the eyepiece surface, he will look obliquely and will see the dark inner wall of the tube or, finally, the ‘bright disc on a dark background.’

The closer the education of the observing layman to that of the specialist, the closer also will be the observed picture. But even a botanist who does research in general bacteriology and is acquainted from literature with the features of diphtheria bacilli, will not catch a glimpse of those features of the preparation upon which the specialist will base himself, and he will not be able to distinguish them, i.e. he will not find any correspondence between words known from the book and the observed features of the picture. Even the most thorough descriptions cannot compensate for the lack of practical experience.

Thus, when juxtaposing the results of observations by various observers, one obtains an entire gamut: from a picture perceived by the specialist, which is in agreement with official knowledge, throughout various ‘fancies’ down to the impossibility of perceiving the picture at all. Which of these results of observation is entitled to be expressed by that desired univocal, generally valid description?

(1) One would think that every one of them would. However, one is obliged to state that a certain portion of these ‘results’ of observation are really after all inexpressible. A completely unacquainted layman will not attain any result, any palpable form, because he experiences only a chaos of flickering, ever-changing impressions and moods, mutually inconsistent and
mutually annihilating. If we wanted absolutely to render his experience in words, the most appropriate slogan would be: 'I search' or 'I have a chaos'. No other words will represent his experiences.

(2) Some results are expressible more concretely. There emerges from the chaos a more or less palpable shape. In some cases this is a shape completely distant from that seen by the specialist, and which even has nothing in common with the preparation of diphtheria germs as understood by the specialist. It is related to the experiencing of observation in general or of the application of the microscope in general, etc. Thus even these 'observations' will fail to yield the simple record from which, by way of conclusions, one could attain knowledge of diphtheria.

(3) Finally, other results of observation approximate what the specialist believes to be the object of his research, i.e. the preparation of diphtheria germs. Unfortunately, one cannot determine exactly what does belong to the preparation of diphtheria germs and what does not belong to it any more [e.g. the features of the slide glass? A stain from the precipitated dye? Remainder of the medium in which the bacteria grew? Shadows of decomposed bacteria?]. Thus controversial problems can always arise: what is, and what is not, the feature of the object under research. [If we consider that the object of observation is not the preparation of germs but the germs themselves, the problem will be even more difficult: nobody knows at present what are the transitional forms and states through which the germ passes; hence it is not possible to limit in concreto what is and what is not a germ.] Therefore divergences must always occur in limiting the object of observation. These will be smaller if the observer has a more specialist education and experience, but they will never disappear completely. The observations in the field of Chlamydozoa or Pettenkoferia, which some research workers consider to be fancies, conjectures on the background of artefacts, prove that, even among specialists, such divergences do take place.

Hence we have two mutually related difficulties:

(1) the necessity of a certain standard education and training of the observer, without which the observation of the given subject is out of the question, and (2) the impossibility of a complete reconciliation of various, even educated, observers, as regards the range of the subject.

Both these difficulties are not appreciated by theorists. Their opinions implicitly comprise the idea that the 'professional competence' or, at least, the 'preparation for observation' are states which, firstly, are always attainable by a great majority of people, and, secondly, are in general capable of being determined or defined, or even possess some absolute, almost
metaphysical value. As regards the second difficulty: scientists know almost in general that "überhaupt enthält der Begriff der Beobachtung eine Willkür, indem er wesentlich darauf beruht, welche Gegenstände mit dem zu beobachtenden System gerechnet werden" (Bohr), but this leads them most frequently to epistemological conventionalism, since they think that this arbitrariness Willkür depends on the volition of the research worker and that, in his choice, he is directed by an aim, thus creating a tacit 'scientific agreement'. However, the research worker has no consciousness of choice; on the contrary, the choice is imposed on him directly and in a binding manner, following from his mood of thought, from the set of its mental readinesses, from his mental thinking practices — in short from what we call the thought-style (Denkstil).

The thought-style thus understood is the result of the theoretical and practical education of the given individual; in passing from teacher to pupil, it is a certain traditional value which is subjected to a specific historical development and specific sociological laws. Both aforementioned difficulties are reduced to the question of the thought-style: one should admit that each of the aforementioned observers had carried out observations in accordance with his thought-style. These styles are more or less different from each other; the more they differ, the more different are the results of observation. In the specialists we witness a fundamental community of thought-style, and only slight individual or 'directional' style differences do remain (according to the 'school'). Should a complete identity and immutability of thought-style occur, no discovery, i.e. no perception of anything new would be possible. Each new observation is an experiment: what is at stake is to apply the most suitable combination to the given conditions from among the stores of shapes at our disposal (if need be from completely different fields). The manifold nature of these stores is therefore a necessity, and the collective of research workers cannot be replaced by one research worker, had he even an infinite time at his disposal.

It is not only the limiting of the subject of observation that is determined (up to the complete shifting of the limits) by the thought-style of the observer. Stressing some elements and the downgrading others likewise depend on this style. One therefore has to say that two observers possessing fairly different styles have no common objects of observation; each of them observes in principle another object. As far as the records of their observations are concerned, the problem becomes more and more involved since the records will either use other expressions or else the same expressions in a different meaning. Hence it is out of the question to admit that, between these observers,
there exists an exchangeability of (observation) records in general, or even a partial exchangeability. Therefore any univocal description of the result of observation is impossible if one uses expressions of a general language.

(2) One would think that none of these psychologically conditioned results of observation is directly entitled to the privilege of supplying the records of a scientific observation, that the research worker — as previously mentioned — ought, by using critical analysis, to exclude from his result of observation all subjective elements, or at least the personal ones, in order to obtain what we call scientific observation.

But this critical analysis itself, is it not also conditioned psychologically and historically? From the standpoint of comparative epistemology it is nothing else but the stylization of observations: at first carried out consciously according to traditional prescriptions, it becomes later a mental habit of the observer and, finally, the experienced research worker simply cannot "observe uncritically". And yet this critical attitude maintains many individual features and, what is more, undergoes continuous evolution. Otherwise it would be impossible to perceive new details in old material, which always takes place in science. In the last elements of the identified system of science the style features are just as present as in crude observation.

Instead of embarking on the general analysis of such a critical, purified, impersonal scientific observation, let us consider, using our example of the diphtheria-germ preparation, what do the official scientific observation and a similar description look like?

In the well-known and generally accepted textbook by Lehmann-Neumann *Bakteriologische Diagnostik* (1927), II, p. 676 we read the following description of the system of diphtheria bacilli:

Die Lagerung ist sehr charakteristisch; neben dem grossen Formenreichtum (s.o.) ist die Lagerung meist durchaus unregelmassig (ungeordnet), so dass man sie schon mit chinesischen Schriftzeichen verglichen hat. Im Gegensatz dazu zeigt Pseudodiphtherie in Form und Lagerung weit grossere Gesetzmaessigkeit. Die Lagerung in Form ausgespreizter Finger oder einer römisichen V, weiterhin das palissadenförmige Zusammenliegen, ist mehr für die Pseudodiphtherie charakteristisch, kommt aber gelegentlich auch bei echter Diphtherie vor.

If a specialist reads this description, he is inclined to express his agreement. He consents that the arrangement of the diphtheria bacilli is characteristic, i.e. that a large part of the diagnosis of these bacilli can be based on it, and that the arrangement is not well-ordered, that pseudo-diphtheria more frequently shows a better-ordered arrangement, of the palisade type or recalling fingers spread wide apart.
If a so-called educated layman reads this description, he must feel a certain surprise. After all, he reads that the arrangement is characteristic, and immediately afterwards that there is no rule in it. Now, can a chaotic, disordered system be characteristic?

The point is that 'characteristic' means in this case 'specific in spite of the chaotic character, and containing a difference with respect to what is to be distinguished from in practice, i.e. from pseudo-diphtheria'. This difference consists in a greater chaotic nature of the system, in contra-distinction to the more regular system of pseudo-diphtheria. Now this more regular system is described in the following sentences of the Lehmann-Neumann description. Here the layman is again struck by the unexpected order of words, and by the word 'mehr' which gives the last sentence of Lehmann-Neumann a certain polemical character. One would expect that the two last sentences of the description would read as follows:

Im Gegensatz dazu zeigt Pseudodiphtherie in Form und Lagerung weit grössere Gesetzmässigkeit: man beobachtet die Lagerung in Form ausgespreizter Finger oder einer römischen V, weiterhin das paliadentförmige Zusammenliegen – obwohl freilich diese Lagerung auch bei echter Diphtherie vorkommt.

The two last sentences of Lehmann-Neumann sound as if the authors were fighting a contrary view. The point is that here they are carrying on a controversy with the past, and also with their own scientific past (about which see below).

At any rate the description cited consists in (1) a simple statement of the specific nature of the image, (2) an allowance for the practical necessity of the research worker (distinguishing between diphtheria and pseudo-diphtheria), but not in submitting a certain general picture derived from some basic elements which would be given directly to every observer. (3) Moreover it takes a certain standpoint with respect to the historical description from the past epoch. (4) It uses some comparisons (letter V, fingers spread apart, the palisade), which are unexpected to a layman, and are derived also from the history of the knowledge of diphtheria (see below).

In short, this description does not exceed the limits of the specific thought-style of a bacteriologist, his requirements, his historical evolution, his historical comparisons. We shall consider later whether it would be possible to arrange it in a different way.

In the likewise well-known and almost equally appraised textbook by Kolle and Hetsch (Die experimentelle Bakteriologie (1919), II, p. 669) we read:
Recht charakteristisch ist die Lagerung der einzelnen Individuen in gefärbten Präparaten, mögen diese aus Reinkulturen oder direkt aus Diphterieemembranen hergestellt sein. Die Bakterien lagen sich nämlich gern parallel nebeneinander, wodurch eine palisadenartige Anordnung zustande kommt. Bis zu einem gewissen Grade typisch ist ferner, dass die Diphteriebazillen, wenn sie in Gruppen vereinigt sind, mit dem einen Ende zusammenhängen, während sie am entgegengesetzten Ende divergieren. Es entsteht so das Bild gespreizter Finger.

The specialist will also consent to this description. The comparison with a palisade is softened by the word 'gern' (hence not 'always', but 'willingly'), with the fingers by the words 'bis zu einem gewissen Grade typisch' (hence again not 'absolutely', but only 'to a certain extent'). True, the description of Lehmann and Neumann is incomparably more mature, but this one is likewise not false.

The layman must have the impression that this description is inconsistent with the previous one. In point of fact, only the definition of the system of diphtheria bacteria as a characteristic one is common, and furthermore the description of Kolle-Hetsch appears to be completely contrary to that of Lehmann-Neumann; one of them states that the palisade and 'digital' arrangement is typical, and the other — that it is not.


For every layman these descriptions are inconsistent, while for the specialist — not! — because he knows that they are to be taken cum grano salis: each of them suggests certain pictures which may be, but do not have to be found everywhere. The most important and most essential is the fact that the system is characteristic, which is stressed by everybody, whether clearly or not so clearly. To describe this characteristic nature they provide the traditional, customary comparisons which serve equally well when used to bring into relief the similarity between them and the picture watched, as when used for bringing into relief the differences. The system of diphtheria bacilli shows a specific form. One has to learn to see it; it is then 'specific' and
nothing else, and just as specific is the appearance of letter A, in spite of its variability. Before one learns to see this form, various comparisons obtrude themselves. The relation of this form to the one compared, and known otherwise, flickers in our eyes: we see once a similarity, and some other time a difference. It is in this way that we learn how to produce a new form. Once it has been strongly grasped, the comparisons become superfluous or else they have merely didactic value, i.e. for somebody who is learning to see.

Quite instructive is the consideration of the development of observation and of description in this field. In the 1920 edition of their textbook, Lehmann and Neumann (II, p. 554) write:


The authors then adduced their comparison with a palisade and with the letter V in a much more positive sense than seven years later. The word "charakteristisch" is repeated, but its explanation is different, it appears to be almost contrary.

In the 1899 edition (II, p. 371):

Die kurzen Formen sind mehr parallel gelagert, die langen mehr gekreuzt, fingerförmig in Rosetten abgeordnet und s. f. Nach Kurth wächst die Wahrscheinlichkeit, einen pathogenen Stamm vor sich zu haben, wenn es gelingt festzustellen, dass in Klatschpräparaten junger Kulturen (6 St. bei 35°) auf Köller-Serum gezüchtet, mindestens eine Anzahl langer Formen (7 mal so lang als breit) oder fünfer -- (V) -- förmige Gebilde vorhanden sind. Weiter legt Kurth Wert darauf, die jungen Stäbchen gelagert zu sehen, wie die Finger zweier übereinander gespreizter Hände.

The roughly contemporary description by Marx (Diagnostik etc. (1902), p. 129) reads:

... man erhält in Klatschpräparaten von 6-stündigen Kulturen Bacterienanordnungen, die M. Neisser treffend mit Bildern vergleicht, die entstehen, wenn man die Hände mit ausgespreizten Fingern in allen möglichen Lagen übereinander legt. Später liegen sie vornehmlich nebeneinander, pullisadenförmig”.

Besson: (Technique microbiologique (1898), p. 324):

Ces bacilles peuvent être disposés parallèlement les uns aux autres ou associés par deux bout à bout; souvent encore, ils sont unis par deux à angle plus ou moins aigu de manière à figurer un V ou un accent circonflexe.
The descriptions become mostly longer, they contain numerous comparisons, quote their authors, provide detailed conditions. As compared with them, the description of Lehmann-Neumann of 1927 is very meagre, especially if we leave out the polemical last sentence which virtually corrects earlier opinions. It is as if the authors had given up, with time, the hope of finding suitable comparisons: in 1927 they already confine themselves to writing that the system is disordered, chaotic like Chinese writing — yet still specific, characteristic. In many more recent textbooks, e.g. Kolle-Kraus-Uhlenhut *Handbuch der pathog. Mikroorganismen*, vol. V, part 1, p. 460 (article by Gins of 1928) one reads sentences which are very similar to the contemporary description of Lehmann-Neumann:

Wenn das charakteristische einer Löffler-Reinkultur im Tuscheanschrich bezeichnet werden soll, so ist es meines Erachtens die Tatsache, dass kaum zwei kongruente Stäbchen nebeneinander liegend angetroffen werden.

In the textbook by Calmette-Boquet-Negre (*Manuel technique* etc. 1933) the authors do not at all offer a description of the arrangement of diphtheria bacilli, although of course they do not question its characteristic nature. They have given up the description, for they know that the arrangement is of a specific nature, non-comparable with anything. They see its specificity directly, and any comparison is superfluous, even damaging.

The evolution of the observation of diphtheria bacilli, which has taken place in the official community of bacteriologists (a thought collective) looks therefore as follows:

About the year 1900, i.e. 16 years after the discovery of the germ, the specialists saw in the arrangement of these bacilli a series of pictures and figures otherwise known: spaced fingers, fingers of two superposed hands, letter V, *accent circonflexe, palissades*. Once one picture seemed to be the most suitable one, but in another case another one, and therefore one was pushed into the scrap-heap of pseudo-diphtheria in one case and, in another, other pictures. One tried to fix these inconsistent, oscillating forms in some way which suggested themselves alternately to the observer. The conditions of their formation were studied, and some authors did believe that these conditions had almost been established.

Later, about 1915—1920 the specialists began seeing the specificity of the pictures of arrangement of diphtheria bacilli. They stress that this arrangement is characteristic, but any similarity to the traditional comparative pictures begins to become blurred. These comparisons are no longer so detailed; some reservations and limitations do appear.
Finally, after 1925, they see directly the specific form of this arrangement, they know that its synthetic description is impossible, that any analysis leads only to its break-up into a disordered chaos. Thus a specific readiness has arisen for perceiving a certain separate form. The comparisons have only a historic or didactic value, since the new members of the collective body are introduced (i.e. this readiness is produced) in a historical way.

At the same time a parallel evolution of notions did occur: what should be considered the true diphtheria, and what the pseudo-diphtheria, as well as whether and what are the boundaries between true and alleged diphtheria. This evolution of notions causes the result that, at present, the meaning of the words 'diphtheria bacillus' and 'pseudo-diphtheria bacillus' differ from that in 1900. Hence it is impossible to understand 'in the modern way' the sentences dating back to 1900; even the authors themselves would lose the ability to have such an understanding of their one-time utterances which they had at that time. This is precisely a dependence of the views and observations on the epoch, and, without this dependence, the development of knowledge is impossible.

Where are these 'critical observations', these 'uniquely good observations', reliable, unchanging, valid forever observations, from which scientific knowledge would grow by pure apposition as if from small bricks?

There exists a certain collective of men possessing a common thought-style. This style develops, and is, at every stage, connected with its history. It creates a certain definite readiness, imparts it by sociological methods to the members of the collective, and dictates what and how these members do see. This picture appears first as a result of a thought-experiment sui generis: from the store of traditional pictures one fits on some pictures and their combinations; next some of them are rejected, others are stylized, then a battle takes place with the alternately intruding pictures — and, finally, a new readiness is formed, i.e. the readiness to see a new, specific form. This complex path is examinable: it can be studied by way of a theory of knowledge based on the sociology of thinking and on the sociological history of the development of science. This science, while comparing different thinking styles and studying the circulation of the thought within various thought-collectives, states that cognition passes through three basic stages: the discovery appears at first in the form of a feeble advice, noting the resistance which inhibits the alternate mental oscillations in the creative chaos of thoughts. By way of social stylizing, by this circulation of the thought, there arises from this advice a demonstrable thought, i.e. a thought which can be placed in the style-system. Further development transforms it into an
obvious thought — within the framework of the given style, into a specific, directly cognizable form, into an ‘object’ which the members of the collective must treat as a fact existing outside and independent of them. Such is the evolution of what we call ‘real’. This is one way in which cognition arises. Another way, which consists in the development of cognition from a vision *sui generis* of the collective, is not the subject of consideration of the present paper.

The example we used (observation under a microscope) might appear to be too complex and artificial. One can object to it that it is not the example of the observation itself, since a certain technical skill is required for observing under a microscope, that there also exist simple observations, simply ‘looking and seeing’. I do not think it useful to discuss in general and in the abstract whether such ‘simple looking’ does exist; a concrete example will be more helpful, one which, at the same time, will complete our previous consideration, by adducing not only the development of looking in a certain domain, but also its beginnings. The example of the diphtheria bacilli would be rather difficult to widen for non-specialists in this direction.

A specific traditional anatomy existed in Middle Ages, originating from Galen, but corrupted through a long chain of intermediaries, and reduced in both text and drawings to poor, often childish-primitive schemas supplemented with speculation. The origin of modern anatomy is usually represented as follows: Galen’s anatomy survived for centuries because the medieval scientists did not observe and, in particular, they did not carry out dissections. However, as soon as “he roused himself from his sleep and began to watch with his keen and open eye the anatomical forms, and then to fix what he had seen” (R. Sudhoff; *Tradition und Naturreobachtung* (1907), p. 3), medieval anatomy must fall down, and modern anatomy then did arise.

Now this legend is erroneous. In the first place they believed in Galen *not* because no observations were carried out, but because no observations in the present-day meaning of this word had been done, for there was no need to do it: Galen — and even a simplified Galen — completely satisfied the scientists of that age. However, one cannot assert that a medieval scientist did not have any positive relationship to observation. Of course, this relationship was, qualitatively and quantitatively, different from the present one, but it did exist. We read, e.g., according to the author of an illustrated MS from 1158, hence from the deep Middle Ages, that he describes veins, nerves and tendons one after another ‘*ne forte erret inspector eorum, sed agnoscat ea ita ut vider*’. Hence the watching, seeing, of the true state of affairs was even then taken into account. One also took into account the ‘deception of
senses', i.e. there existed a way of critical observation, though it was different from the present-day one, because criticism consists in stylizing, in adjusting to the thought-style. Looking and seeing at that time differed from the present-day, but it would be a sign of naïveté to think that a man of those days was asleep, and roused himself from his sleep only during the Renaissance. The technical instructions that could occur with respect to organs which are only demonstrated by anatomical dissection did not exist, e.g., as regards osteology: the 16th century could find bones in the neighbourhood of cemeteries and study them, but the Middle Ages simply lacked any intellectual need of such observations; when looking at a bone one could see only what one could find in books, without special looking. It was generally admitted that the male had, on the left side, one rib less than on the right side, as this was concluded from the Bible, and the man of that time simply believed, i.e. he would not, and could not, make sure that this was false. And yet this would be so easy for us: we, the people of the present time, are able, without any technical means, to check on ourselves or other people that the number of ribs is the same on both sides and for both sexes.

In the work of Berengar (about 1520) we read regarding the old controversy over the origin of veins: According to Aristotle, veins come from the heart, according to Galen — from the liver.

Dico tamen... quod venae non oriuntur nec a corde nec a hepate, nisi improprie et metaphorice, et dico eas ita metaphorice oriri magis ab hepate quam a corde et in hoc magis teneo cum medicis, quam cum Aristoteles.

It is obvious that today any logical discussion and any demonstration ad oculos would be helpless in relation to Berengar: we do not know the notion of 'the metaphorical and improper origin of veins'. We know only the morphological, phylogenetic or ontological [sic, perhaps ontogenetic — Ed] 'origin' of veins. To us, the organism is not a collection of such metaphors and symbols — much as we are unable to make known the logical reason why we have altered the thought-style. Not only was Berengar unable himself to see the relations which are known today, but it would have been impossible even to show them to him: what is of importance to us, is for him inessential, inexplicable, alien, just as, on the contrary, his own thinking is alien to us. One cannot, simply and immediately, see something new and different. First, the entire thinking style must be changed, the entire intellectual mood must be unsettled, and the brute force of the directed mental readiness must cease. A specific intellectual unrest must arise and a change of the moods of the thought-collective, which is the necessary
condition for creating simultaneously the possibility and necessity of seeing something new and different.

It can be demonstrated that the problem of the opposition to Galen and the tradition was independent of the anatomical details: some disapproved of Galen in principle, without being able to say anything different as far as details were concerned (Berengar); others defended him basically and absolutely, though admitting that the innovators were right in details. There were also others who occupied an odd standpoint, viz. that it is better to err with the ancient ones than to profess truth with the innovators. This intellectual unrest of the society of anatomists in the period preceding the new epoch is reflected in individuals: this is perfectly visualized by Vesalius’s precursor, Berengar, who is full of contradictions, ever retreating and terrified by his own discoveries. When reading papers from that period we begin to think that the respective authors suffered from a specific dizziness, that their eyes were jumping, that they saw alternately the medieval world and the path to the new world. Were it not for this period of unrest, Vesalius would have failed to find people who would listen to him: it would have been impossible to attune the society, i.e. to create a different intellectual mood which would enable new forms to be seen.

Thus the matters of the intellectual mood are the first conditions of a discovery. It is as if the first to arise were a tendency to a change, a certain indefinite ‘dissimilarity’ of views, only later to be followed by the crystallization of those ‘dissimilar details’, discoveries and observations which render this dissimilarity specific.

As regards the details: every detail, ever so minute, remains connected with general views, and its discovery depends on this relation. Just as the number of ribs, connected with the religious views, was not the same in the case of men and of women, so also later, in the dawn of modern anatomy, the new observations were, in their contents and their appearance, dependent on certain ideas or myths linked to them. The discovery of anatomical details did not take place in mechanical sequence, e.g. according to the regions of the body, the size or distinctness of the details; rather, of crucial importance was legend, the general view, the intrusive purposefulness etc. Similarly the contents of observations, i.e. what had been seen in the given place, depended on the prevailing thought-style: each new layer is to uncover its own style motivation, and all the details of a certain epoch have some common style.

Discoveries are accompanied by certain shifts of interest, and often, along with the emergence of some new details of the description, some definite old details do disappear. In the 17th century anatomy books we find long
chapters which describe and enumerate the so-called *ossa sesamoidea* which, in modern textbooks are disposed of with a few sentences: they are today, so to say, outside the osseous system and do not present anything of great interest from the ontogenetic, morphological or physiological viewpoint. On the contrary, at that time they were important because of certain old myths according to which from one of such bones there will develop *sicut planta ex semine* the complete body to appear at the Last Judgment. In the 16th and 17th centuries, when a name itself was not yet considered to be a conventional sign but rather the essential feature of an object, we find in the descriptions of anatomical details long etymological and pseudo-etymological considerations of the name. In an anatomy textbook of mid-17th century I found, in a passage dealing with the femur, 135 words relating to such an etymology of the word ‘femur’ and only 31 words providing the description of the modern meaning of this word.

It is important, and it can be followed in the old anatomical drawings, that, at first, one sees the general form of the organ, something like its symbolic representation, and only much later the representation of the elements of this form. Thus old anatomical drawings represent: not *ribs* of a certain quantity and certain shape, but the symbolic *‘ribbing’* on both sides of the chest; not the definite *loops* of intestines in the abdomen, but numerous spiral *lines* which symbolize them; not the definite *ganglia* of the *brain*, but a curly *‘coiling’* of the entire brain surface, etc. The sections of the eye look like sections of an onion, as they demonstrate not a certain definite number of layers in the eyeball wall, but its ‘multi-layeredness’. Later, from the symbols of a certain shape, schemas arise gradually which vividly underline some features. These drawings present their object in a specific *style perspective*, by stressing precisely the style features of the object. It can be shown that modern drawings also contain this perspective, that, in general, it is impossible to represent anything without this perspective, and that the naturalism of every epoch consists in a stressing of features such that they may be consistent with the style of a given epoch and given society, but invisible to its members.

Hence the new observation, i.e. the discovery, is carried out in such a way that, during the epoch of equilibrium, there arises a certain intellectual unrest and a tendency towards changes: a chaos of contradictory, alternate pictures. The picture, fixed up to now, disintegrates into blots which arrange themselves into different, contradictory shapes. From other fields, previously separated or neglected, some motives are added; historic connections, almost accidental, various intellectual relics, often also the so-called errors, mistakes and misunderstandings for their part, add other motives. At this creative
moment there becomes embodied in one or more investigators the mental past and present of the given thought-collective. All physical and mental fathers are with them, all friends and enemies. Each of these factors pulls to its side, pushes or inhibits. Hence the flickering chaos. It depends on the intensity of feeling of the investigator whether the fact, whether the new shape will appear to him within this chaos as a symbolic vivid vision, or else as a weak hint of a resistance which inhibits the free, almost discretionary choice between alternate pictures. In both cases it is necessary to defend the new shape against scattering: it must be separated from what, from that moment onwards, will be unimportant, accidental. It is necessary to create directional interests, and to destroy inimical interests. One has to create another mental readiness and to educate people to live in it. If one manages to do this, all of the participants will see the new form directly, with their own eyes, as if it were the only one, everlasting truth, independent of the people. It is only a subsequent tuning that will permit us to see that it had its own style conditioning and that it was a resultant determined historically.

Now where is this pure observation without bias? ... a 'good' observation, valid once for all, independent of the surroundings, their traditions and epoch? One does not find it anywhere in history or today, nor also is it possible as an ideal which one would be able to approach by analysis or criticism, since any 'identification' of the observation data is likewise subjected to the thought-style which is always likely to be among the last elements revealed of the logical structure of science.

A truly isolated investigator is impossible, and so also is an ahistoric discovery, or a styleless observation. An isolated investigator without bias and tradition, without forces of mental society acting upon him, and without the effect of the evolution of that society, would be blind and senseless. Thinking is a collective activity, just as choral singing or conversation. It is subjected to specific changes in time, and displays a historic continuity of these changes. Its product is a certain picture, which is visible only to anybody who takes part in this social activity, or a thought which is also clear to the members of the collective only. What we do think and how we do see depends on the thought-collective to which we belong. The pictures we see possess, in addition to the genetic historical conditioning, also the internal style determination. An example of such a style determinism was adduced before, viz. the relationship between the position of the cross-bar in the letter A, and the angle between its arms. In natural sciences which comprised a certain thought-style and led it to a system, we call the style-determinism the scientific reality. It develops along with the development of the scientific thought-style.
'To see' means: to re-create a picture, at a suitable moment created by the mental collective to which one belongs.

NOTES

2 Fleck: 'Zur Krise der Wirklichkeit'. *Naturwissenschaften* 17, 33. [This volume ch. 2.2, pp. 47–58]
THE PROBLEM OF EPISTEMOLOGY [1936]*

LUDWIK FLECK

I

The fundamental error in many discussions from the field of epistemology is the (more or less open) manipulation of the symbolic epistemological subject, known as 'human spirit', 'human mind', 'research worker' or simply 'man' ('John', 'Socrates'), which has no concrete living position, which does not basically undergo changes even in the course of centuries and which represents every 'normal' man regardless of the surroundings and the epoch. Thus it is to be absolute, unchanging and general.

One says for instance that a man's sources of knowledge are empirical experiences, but one does not think that, for a very long time, the source of almost the entire knowledge of every man was, in Europe, simply book and school. Now these books and schools also spring from books and schools, etc. Now even if we admit that this path is ultimately derived from somebody's empirical experiences, up to now we lack serious studies whether the transmission of the knowledge itself, its migration from man to man, from a scientific journal to a textbook, does not change its content, and particularly whether this change takes place in a specially directed way. Hence do there exist by chance any elements of knowledge whose genesis is neither empirical nor speculative, but exclusively sociological, i.e., which arise during, and as a result of, migration within the society? We also lack studies which would explain in what way a certain definite store of information affects the act of further cognition. Does cognition undertaken by a specialist from some domain of knowledge take place basically in a different way from the cognition by a layman? Does there exist any tuning up of new elements of cognition of the nature, the style of the old elements? Perhaps a developed branch of knowledge grows in accordance with other laws than a branch which only

* Przegląd Filozoficzny (1936).

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sprouts? Perhaps there exist elements of knowledge whose genesis is purely historical, i.e., which originated exclusively due to a historical coincidence?

These are fundamental problems. That symbolic 'human spirit' is an asocial and ahistorical being: being the only one, unique, hence solitary, it has no communication with anybody, it does not enter into discussions, does not cultivate the art of imitation, has no companions, friends or enemies. Hence the neglect of the sociology of cognition. The 'human mind', this fictitious representative of the minds of men, is moreover said to be, in its 'logical structure', everywhere and always the same, even in general the only possible one. Hence the neglect both of studies of the historical development of thinking and of the comparative science of thinking: all forms of archaic and exotic thinking are simply disregarded as being unworthy of being studied. There exists a narrow-minded fiction of a 'normal mind'; what is different is, to all intents and purposes, not capable of being studied, and it can be only adored as genius, or else treated with pity as madness.

One speaks too much about what cognitive thinking ought to be like, and too little about what it really does look like. Yet do we really know that much about what it ought to be? Do we know at least one example of perfect thinking, a thinking that would deserve fixing once for all, so as to prevent any further change? I cannot resist comparison with the speculative *anatomia imaginabilis* of the epigones of the Middle Ages, which consisted of a few poor traditional schemas and a large number of complementing speculations, and which tested not the real structure of the body, but rather what it ought to look like to satisfy the requirements of science: the traditional *epistemologia imaginabilis* is very similar to that anatomy.

The embryo of the modern theory of cognition is found in the studies of the school of Durkheim and Levy-Bruhl on the sociology of thinking and on the thinking of primitive peoples. Also Gumplowicz, Jerusalem et al. stress the "social conditioning of thinking and of its forms". However, these embryos lack consistency, because they did not succeed in extricating themselves from the prejudice that modern European scientific thinking presents a basic exception, being 'objective' and not subjected to the principle of social conditioning. Levy-Bruhl believes, e.g., in the objective features of phenomena, upon which he automatically directs attention, as soon as the mystical elements of thinking disappear. Similarly Jerusalem writes about the possibility of a purely 'objective' ascertaining of facts which is allegedly attainable by the individual once he is liberated from complete social dependence.

However, particularly striking is the almost complete absence of studies — even of the possibility of studies — of the archaic forms of thinking. We only
know how to smile ironically when we read, in old scientific works, the
descriptions of phenomena which are almost exclusively an analysis of the
name of this phenomenon, or when we find there a ‘mixture’ of the figurative
meanings of the word with its real meaning. The odd, often very complex,
symbolism of the old illustrations, the aim of which was to represent things or
events naturalistically; court sentences against inanimate things, e.g., against
the bell which honoured Savonarola; the inscriptions on houses during plagues
for the purpose of deceiving the plague, e.g., ‘that there are no children in
this house’: all this, and similar symptoms, are for us merely oddities to be
laughed at; yet we lack the standpoint from which we would be able to grasp
and study them uniformly. We also lack the possibility of formulating the
intellectual personality of old thinkers: in the history textbooks they are
geniiuses, but when reading their own works we often find primitive thinking,
unsettled views and naive theories. Tycho Brahe, whose fundamental argument
was that motion, being a more noble phenomenon, rather becomes the stars
than the ponderous earth, appears, in this double light, to be an irrational
phenomenon. Similarly Kepler, who believed that comets exist in order to
keep the space of the universe not too empty (just as God took a liking for
fishes in order to make the sea not empty), or Kant, who believed that “the
dignity of mankind was born at the moment when the first man said to the
sheep: the fur you are wearing was given by nature not to you but to me,—
and when he took off this fur from the sheep in order to put it on himself”.

The rationality or irrationality are not, however, the features of phenom-
ena, but a proof of the suitability, or otherwise, of the rational methods used.
It follows therefore that, up to now, we lack methods suitable for the testing
of the cited phenomena.

II

To my mind epistemology must result from three basic phenomena. The first
is the collective mental differentiation of men: people exist who can com-
municate with each other, i.e., who think somehow similarly, belong, so to
say, to the same thought-group, and people exist who are completely unable
to understand each other and to communicate with each other, as if they
belong to different thought-groups (thought-collectives). Scientists, philol-
ogists, theologians or cabalists can perfectly communicate with each other
within the limits of their collectives, but the communication between a
physicist and a philologist is difficult, between a physicist and a theologian
very difficult, and between a physicist and a cabalist or mystic impossible.
The subject of the conversation does not play a decisive role, because on an apparently identical subject, e.g., a certain disease or a celestial phenomenon, a physicist will understand a biologist, but will be unable to come to an understanding with a theologian or a gnostic. They will talk next to one another, but not to one another: they belong to different thought-collectives, they have other thought-styles. What, for one of them, is important, even essential, is for another a side issue, not worth discussing. What is obvious for one, is nonsensical for the other. What is truth (or 'lofty truth') for one of them, is a 'base invention' (or naive illusion) for another. Even after a few sentences there appears to be a specific feeling of strangeness, which signals the divergence of thought-styles, just as in other cases one experiences, even after a few sentences, a specific mental solidarity with our interlocutor, which proves an affiliation with the identical thought-collective.

To give an example, let us compare what the philosopher Bergson writes about motion with what the physicist Maxwell writes. Bergson: Introduction to Metaphysics (translated by T. E. Hulme, pp. 41–42):

Consider, for example, the variability which is nearest to homogeneity, that of movement in space. Among the whole of this movement we can imagine possible stoppages; these are what we call the positions of the moving body, or the points by which it passes. But with these positions, even with an infinite number of them, we shall never make movement. They are not parts of the movement, they are so many snapshots of it: they are, one might say, only supposed stopping-places. The moving body is never really in any of the points; the most we can say is that it passes through them. But passage, which is movement, has nothing in common with stoppage, which is immobility.

and p. 23:

When you raise your arm, you accomplish a movement of which you have, from within, a simple perception; but for me, watching it from the outside, your arm passes through one point, then through another, and between these two there will be still other points; so that, if I began to count, the operation would go on forever. Viewed from the inside, then, an absolute is a simple thing:

and then Maxwell: Matter and Motion (Dover reprint, N.Y. 1950, p. 18):

When the change of configuration of a system is considered with respect only to its state at the beginning and the end of the process of change, and without reference to the time during which it takes place, it is called the displacement of the system.

When we turn our attention to the process of change itself, as taking place during a certain time and in a continuous manner, the change of configuration is ascribed to the motion of the system.
and p. 22:

It is true that when we say that a body is at rest we use a form of words which appears to assert something about that body considered in itself, and we might imagine that the velocity of another body, if reckoned with respect to a body at rest would be its true and only absolute velocity. But the phrase "at rest" means in ordinary language "having no velocity with respect to that on which the body stands," as, for instance, the surface of the earth or the deck of a ship. It cannot be made to mean more than this.

It is therefore unscientific to distinguish between rest and motion, as between two different states of a body in itself, since it is impossible to speak of a body being at rest or in motion except with reference, expressed or implied, to some other body.

The two men would be unable to communicate with each other as regards motion: Bergson looks for the 'absolute', and believes that the ideal of cognition is the experience 'from inside'. Maxwell looks for relations to and connections with the surroundings, and his basis of cognition is a relativism. According to Bergson, Maxwell studies the substitute for motion, while according to Maxwell Bergson studies illusions without any substantial content. Bergson finds fault with Maxwell, that he does not study at all 'motion as such', but only its symptoms for the sake of the configuration of the system. Maxwell finds fault with Bergson, that to experience motion does not mean to get to know it scientifically, but, on the contrary, it often makes its cognition impossible.

It becomes evident that words have different meanings for Bergson and for Maxwell: the 'motion' of Bergson is something different from Maxwell's 'motion', just as the same words 'to get to know' have different meanings for both philosophers. At bottom, almost all words have different meanings for them: it is not as if the word of one of them meant a thing that the other would give a different name, but that a thing to which one of them gave a certain name does not exist at all for the other one. That is why it is impossible to translate exactly the utterances of one of them into the language of the other one. Bergson's motion, the motion in itself, absolute motion, does not exist at all for Maxwell; there exists no word to express it, nor is there a need for it, the same applies also to the 'perception from inside' of Bergson. In general this philosopher has a much richer language, while the physicist limits very markedly the store of his words, and he does it on the basis of the specific tradition of 'scienticality': a certain discipline of thought, produced by the history of science, makes him give up some words as useless. The philosopher is not bound by this discipline, but he is bound by the specific tradition of philosophers who fundamentally do not give up any notions which remain from any period of thought.
Still more different is the mental style of the mystics. For Swedenborg¹

all perceivable objects — animal, rock, river, air — even space and time, do not exist for
themselves or for any material purpose, but are merely hieroglyphics which narrate other
history about other beings and other tasks.

Every man ought to ask, when confronted by every object: what is your meaning?
Why does the horizon keep me imprisoned in this centre along with my joy and pain?
Why, out of the countless different voices do I always catch the same meaning, and in
the infinite series of hieroglyphics read the same fact, never formulated with sufficient
clarity?

According to Swedenborg a horse denotes a sensual comprehension, a tree —
the feeling, the moon — belief etc.; in his books we read such sentences:

In heaven nobody is supposed to stand behind somebody’s back and look at the back
of his head, as this impairs the influence which comes from God.

With this man, neither Bergson nor Maxwell would be able to establish com-
munication, and yet he had, and has, his own believers — an entire thought-
collective with a common style of thinking. Swedenborg’s moon is different
from Maxwell’s moon, his heaven and God have no corresponding notions
in Bergson or Maxwell. His cognition, i.e., guessing what a certain object
‘denotes’ is something different from the cognition of Bergson, i.e., the under-
standing of this object, or the cognition of Maxwell, i.e., its measurement.

These representatives of three thought-collectives thus cannot establish
understanding among themselves, although within the range of their collec-
tives they not only understand each other, but even extend their systems of
views in co-operation with other members of the group. Their thought-style
is not an individual peculiarity, but a group one: it is based on a certain
education and training and on a certain defined historical tradition. Thus,
one should speak about a different philosophical, scientific and mystical
thought-style. Each of these styles had passed through specific historical
evolution, and each occupies a specific place in the mental life of mankind.
We have quite a few such thought-collectives which act as carriers of more
or less distinct thought-styles. They are produced by various separate forms
of collective thinking, e.g., certain disciplines such as physics, philology,
economics, or the knowledge of some practical professions such as craft,
commerce, or next the knowledge of religious, ethnographic, political and
other communities, the philosophical systems of definite schools, the so-
called common-sense philosophy of life, etc. Some thought-styles approach
one another, e.g., the physical and biological styles, others are more distant
from one another, e.g., physical and philological, and finally some are as
distant from one another as, e.g., the physical and the mystical. One can therefore speak of separate styles and of varieties of styles, and, similarly, of kindred and distant thought-collectives.

As a rule communication is only possible within one collective while within kindred collectives it is feasible only with some complexity: the inter-group exchange of ideas is always connected with a more or less marked modification of the ideas. When passing from one group to another, words change their meaning, the ideas obtain a different style colouring, the sentences receive another meaning, the opinions a new value. If the groups are considerably distant from each other, the exchange of thoughts can be completely impossible, and transformation of a thought consists, in such a case, in its complete destruction.

III

Another fundamental phenomenon of epistemology is the fact that the circulation of thought is always related, in principle, to its transformation.

If, out of the collective of experiences which are up-to-date at present, I do formulate a certain thought, i.e., if I compose an utterance, in particular a series of words, I am in the first place bound by the structure of the language I am using, and by norms and customs which obtain in the circle to which I belong. Moreover, I must take into account the person or the circle for which I am composing this utterance, for I want to be understood for a certain definite purpose. Consequently, each formulated thought, which is destined for real use, bears the sign of the producing unit and the address of destination. Hence a formulated thought, an utterance, is a directional value, a vector (if one can use this definition here): an abstract sentence, without the sign of the producing unit and without the destination, and also without any regard to those social forces which bring about its direction and circulation, is incomplete and does not suit the purpose of the considerations of rational epistemology. Only a sentence in its natural relevance, i.e. in its social meaning within the society, possesses a definite meaning; an abstract sentence can be understood in different manner: it can be equivocal or meaningless, depending on the circle of the recipient.

If I formulate a certain idea for the members of another thought-collective, I transform it so as to render it approximate to the style of that collective. Thus I try to create a common collective, somewhat intermediary, poorer in substance, but wider. I try to change the style of the given idea. Such a formulation and transmission of an idea is called propaganda. A relevant
example can be the propaganda of Christianity by Jesuits in China in the 17th century (known from history), which applied a far-reaching transformation of Christian ideas for the purposes of the Chinese collective.

If I formulate a certain idea of epistemological content for the purpose of my own collective, this can aim at: (1) its popularization, as far as laymen of that collective are concerned, (2) information regarding it, as far as the equivalent specialists are concerned, and, finally (3) its legitimization within the framework of the style-system of ideas, i.e. its official formulation, valid for the collective as such.

Everybody will agree that both popularization and legitimization bring about a change of the social value of the utterance. Popularization uses the everyday language, i.e. inexact expressions; it leaves out criticism and reservations, and stresses some aspects of the problem by way of pictures and comparisons. A layman gives to the specialist his specific epistemological confidence, but he lacks the possibility of controlling it. Consequently, the apodictic nature of the specialist’s utterance undergoes a considerable enlargement in popular representation. For the sake of an example, let us compare any statement from the field of hygiene in its specialist and popular formulations.

The legitimization of the statement isolates it from its genetic psychological and historical relations, and adapts it to the schemas of the entire system. Thus it tears it, so to say, away from its own native soil and transplants it to the common artificial garden: it deprives the individual of its separate nature and clothes it into the uniform of the service of the collective. It enables it to take part in the credibility and dignity of the entire system which is recognized by the thought-collective as being the only good, even the only possible one. Thereby the statement acquires additional traits of objectivity and certainty (durability). As an example let us realize in what way the value of a statement changes when it passes from a newspaper article to a textbook: the same sentence possesses then another meaning, by losing the character of the assertion of the author Mr. X, and becoming a recognized element of the given branch of science.

The informing statements exchanged among specialists who are equivalent in a given field play the least role in the real, socially important exchange of ideas. One can talk about an informing statement only for the case when its recipient has specific confidence in the cognitive abilities of the sender (cognitive confidence) and simultaneously the full possibility of checking the contents. It is clear that the information statements in this understanding are merely a certain boundary possibility. The point is that experience teaches
us that, as far as any new information given by a specialist is concerned, another specialist's attitude is colored either with an excess of confidence and a lack of the possibility of checking (and thus appears as a secondary factor, i.e., to a certain extent as a layman), or else with an excessive desire of testing or legitimizing (in other words as a superior factor). An equilibrium of criticism and confidence between specialists occurs in the first place when the content of the exchange of ideas does not relate to new knowledge, but to the established and generally recognized fields. The more mature the given field, the more developed the given group of ideas (or better speaking: the less vivid the development of the given group at a certain moment) the greater the number of informing statements or, more strictly speaking, the more the statements between specialists approach the ideal informing statement, and at the same time the more do they acquire the nature of conventional mottos of the given thought-collective.

Thus the intended circulation of the thought, i.e., its circulation in the intended directions inside the collective or between thought-collectives is almost always connected with transformation. In the first place we have here a circulation between the creator, or, generally speaking, the mental elite and the crowd, during which occurs the transformation or transfer which we call the popularizing transformation. Next, now circulating within the crowd, the new thought interferes with the mental store and adjusts itself to the specific style of the thought-collective. Each new thought undergoes a process of legitimization according to the rules of this style, thus undergoing a legitimization transformation. Once the style does stabilize itself for a while, i.e., once a thought-collective group remains for some time in a stable state, circles of equivalent specialists are formed. In such a social organism, an idea can temporarily circulate with a minimal transformation, and even, in ideal cases, without any transformation as an exchange of conventional signals. However, every shock, every new creative focus, destroys these tracks of circulation without change and again initiates the transforming circulation.

The intended circulation of ideas, motivated by desire to communicate, is not the only possibility. Other social forces, *inter alia* the force of curiosity, result in the thought reaching a recipient for whom it had not been intended. Such unintentional circulation of ideas is very important from the standpoint of the sociology of thinking, and it is linked to the most outstanding alterations, sometimes with a complete change of the meaning. Words and sentences pass from one thought-collective to another, transferred by an individual for whom they had not been intended, and their meaning during this migration changes sometimes to such an extent that only a loose, distant similarity remains. We
can use as an example the corruption of medical thoughts transferred by untrained hospital auxiliary staff to popular classes (false popularization); or scientific ideas in the language of journalists; or an exotic religious idea as understood by colonial administrators.

When transferred to another collective, the idea undergoes various vicissitudes. It becomes a mystical, inconceivable motive around which a deep cult (apotheosis of the thought) is grouped. In other cases it becomes ridiculous and undergoes scoffing (caricaturing of the thought). It predominantly fertilizes and enriches the alien style, while being altered and assimilated: the content changes sometimes beyond recognition, even if the word has remained unchanged. May I give as an example the word and notion of 'race', which has been transferred from the natural-science or anthropological style to the political one. Or else the word and notion 'witch', which was transferred from the medieval to the modern style.

None of the aforementioned forms of the circulation of thoughts — which in no way exhaust all of the existing forms (e.g. pedagogy) — occurs completely separately in reality; always — or almost always — popularization is combined with propaganda and legitimization, the information accompanied by popularization or propaganda and legitimization, while the dispatched informational message which has been consciously addressed is accompanied by an unintended reception etc. This is due to the fact that an individual belongs to many thought-collectives and, while being a specialist of one of them (a member of the elite), he is at the same time a layman in other collectives. That is why virtually every circulation of thought is combined at the same time with stylization (intensification) and with over-stylization (alteration). Thus some elements of up-to-date thought content may be authorless. Out of the understandings and misunderstandings, out of repeated transformations and recastings, a creation arises, during social circulation, for which no original, primary components can be found, just as in the legendary knife for which, in the course of centuries, once the handle was changed, and another time the blade, and which had been all the time considered to be 'the same knife', even though it does not contain anything unchanged — except some symbolical value which it represents. Similarly thought products change their components during social circulation and acquire a new content which is not produced by the individual but originates a motu sociali.

This can be observed during every discussion if it lasts for a sufficiently long period of time: in the case of criss-cross sentences, polemics, corroborations, corrections and misunderstandings, a certain word becomes a slogan, though this had not been intended by anybody, and around it, under the
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influence of a common mood, some definite postulates condense, often even not distinctly stated, but contained implicitly in statements. Somebody will formulate them at a later time, and the collective will recognize him as the discoverer, although the real author is not he, but the collective mood.

IV

The third fundamental phenomenon of epistemology is the existence of a specific historical development of thinking, which cannot be reduced to the logical development of thought-contents nor to the simple increase of detailed information.

Everyone who reads the scientific literary monuments is struck by their far-reaching incomprehensibility. A book on alchemy appears to us a tangled muddle of fantastic pictures, empirical observations and distorted views of earlier centuries. We find symbols in it which are dealt with as things, and things possessing features of symbols, mystical correlations between phenomena which we consider to be distant from one another, a strange mood of bombastic mysteriousness, a clumsy manner of argumentation and expression of thoughts, and curious confrontations. No picture of our own reality can replace the descriptions found there, no problem corresponds to the present-day one, no solution can be reproduced exactly.

An old medical book brings impossible or (as we think) extraneous details into a single picture, exaggerated or indistinct — and one which does not correspond today with anything. We read a description of a plague and believe that we would be able to diagnose it, to identify it with a known infectious disease, say syphilis. Yet the archaic author says, e.g., that domestic animals and fishes in rivers also fell ill, or that the plague was accompanied by strange meteorological phenomena. For this reason the description cannot be translated into the today’s thought-language.

The descriptions of strange adventures and voyages, descriptions of such animals as the griffin or phoenix, descriptions of such events as rains of frogs, queer rules of treatment and housekeeping regulations (e.g. how to get rid of mice) etc., show a world which is completely strange for us but not without a specific style. These archaic views cannot, by any logical operations, be transferred into today’s, nor would they change basically by merely increasing the number of learned details. We usually dismiss with a laugh these ‘fairytales and prejudices’. A closer study, however, shows that they contain elements from which our present-day notions and views have developed, and it also permits us to suppose in what way this development occurs in principle.
In the *Tales about the composition of human members, chosen from Aristotle and other selected sages* published originally in 1535, written by Andrzej from Kobylin, we read, on p. 87:

Why do physicians forbid eating milk and fishes during the same dinner? Answer: Because both these dishes are very cold, and therefore, when united together they multiply a great phlegm which directs the man towards leprosy or towards wild scab, i.e. *morbus gallicus*.

This cold differs, as far as the content of the word is concerned, from our notion of the cold: it is, so to say, some constant chemical property or chemical element, but not a physical state. But it has also another meaning. P. 106:

Why do such bitter things (spices) burn less inside than in the mouth? Answer: Because such burning is due to their natural heat. But since the inner heat (as mentioned before) is much greater, it therefore restrains the lesser heat which is in such a spice, and even extinguishes, by reducing its strength, and therefore they do not cause damage and are not so easily perceived inside.

Yet this quasi-chemical property behaves as if it were a physical state. In another case the pair of notions 'cold-hot' means for our author that which we call at present a feature of temperament. Thus we read that “the lads possess more heat than women”, that boldness, irascibility, keenness etc. depend on heat, that old age is cold, and therefore during old age “melancholy, which is cold and dry” does increase, or, that “when the blood which is boiling from anger gets in contact with the heart, the latter is more heated”.

This heat remains in relationship with the “subtlety” and vitality or intelligence in the following examples: “Why does the heart-beat yield more certain signs on the right than on the left side? Answer: This is because of the cardiac heat which shows its greater power on the right side” (p. 51). Hence “right hands are more subtle and stronger than left ones” (p. 50). Or, we read elsewhere that the senses of fishes are dull “for they do not have any heat” (p. 92).

But these are not, as one would think, some transferable, metaphorical uses of the word “heat”, for we read that the cold of old age blanches nails and hair, “for every cold blanches while the heat, while burning, browns”, or that wine imparts to old people a natural heat which already ceased in them. Or, that the heat of hunger can cook raw dishes and make them digestible. *Thus this heat is identical in all its forms, because they can replace one another*: their essence is the same.
THE PROBLEM OF EPISTEMOLOGY

When is a body warmer: before dinner or after dinner? Answer: There is no doubt that there is more heat in the body when food has been added, just as there is more fire when firewood is added. One should know that natural heat is increased in three manners. First, in quantity, viz. by applying warm things, such as furs, quilts, young children or animals; these things add heat. Second, in quality: by means of medicines and spices. Third, in both ways, viz. by quantity and quality, viz. by means of hot food which we consume (p. 66).

Thus we have here a finite system of views: the heat of the fire, of temperament and affection, the 'hot' spiciness of dishes, the heat of hunger, the warmth of the quilt and of young children etc. are identical. On the other hand the cold of the frost, the cold temperament of phlegmatics, the cold of old age, the cold of fear, the cold of death are also identical in nature etc. Everything that excites, that increases vitality and all symptoms of vitality, is linked in some way with heat, or rather not separated from heat, from fire. There is in it an implicitly contained view that 'fire' and 'life' are conjugated in a certain way, by no means a figurative or symbolic one; they are in its essence, to a certain extent identical. It is a very old idea; apart from the vivid view of fire, the fact that many violent affections act stimulatingly, at the same time bringing about the congestion of the face just as physical heat does, and that the feeling of burning (heartburn?) caused by stimulating spicy dishes resembles the feeling of heat agree with it. Next is the fact that the body cools after death, and some striking effects such as that 'deadly fear' causes a turning pale. Thus there was an unclear, though systematic idea of the basic identity between 'fire' and 'life'. There also was an analogous pair of notions which denoted opposite properties: 'cold' — 'heat', which contained that idea of a relationship between fire and life. These notions underwent transformation: they were differentiated and divided so to say into several meanings; one meaning acquired the value of a 'physical meaning', the other the value of a 'figurative meaning', improper, poetical, i.e. based on an 'illusion'.

The special thought-style which developed in the meantime, the style of modern physics, gave up 'cold', while heat acquired a completely different meaning from any to that time: nothing unites the energy idea of heat with emotional heat — except history and a traditional linguistic custom based on history.

If today, with the available scientific thought-style, we start reading old scientific journals, we involuntarily substitute today's content for the words: 'heat' means to us today's 'physical heat', or else the modern 'heat in the poetic figurative meaning'. Yet this word used to mean both terms at the same time, because no such differentiation existed then. That is why this
word cannot be at all translated into today’s language. Therefore the views expressed by means of such notions are to us full of symbolism, fantasy, prejudice or — nonsense. However, such a discrimination between symbolism and naturalism, between fantasy and observation, which exists today, at that time simply did not exist. This is proved irrefutably by medieval drawings; it suffices to turn our attention to the fact that a ‘great lord’ is usually represented in larger dimensions than the members of his retinue, that the buildings shown are only slightly larger than human figures if the figures present the main object of the picture. Similarly the heads are disproportionately large. One does not discriminate between geometrical size and social size or importance for any reason.

Thus notions undergo important changes. These changes do not result from any analysis of sensations that would force us to transform them in a definite direction. They are not a logically or objectively indispensable development of the thought. Indeed, the medieval views yield a compact system which, basically, does not contain more logical errors than one finds in a system of today. Similarly, nothing entitles us to think that the only possible process would be a process to which, e.g., the medieval notion of “heat” would be subjected. It would be possible, for instance, to preserve the notion of heat as a chemical element: even during the life of Lavoisier this matter was still a live issue. In such a case we would not have the energetics of the present-day, but the problem of the transformation of elements would have been simpler and could have been solved earlier; perhaps using this transformation we could have reached the same energetics, perhaps the other way round. The development of ideas occurs using its own paths, it has its own historical conditioning, not a logical one; it is — so to say — passive, not active. Our cognition includes certain elements which are neither speculative nor empirical, but are derived ab evolutione historica.

Of course, the notions must not be considered to be separate bricks which exist only for themselves and out of which the given idea is composed. Obviously, we isolate them ex post artificially from the idea, from the entire collective of ideas, from the continuous process of thinking. But even the isolated notions show, as can be seen, a specific stylish coloring which is characteristic of the given thought-style.

The notion of heat and cold referred to contains implicitly the characteristic general idea of the analogy (or rather identity or relationship) between fire and life. This idea is also contained in other notions of many centuries; never being proved, for a long time not directly formulated, such as having its consequences stated — analogy between smoke and spirit: smoke, frost, spirit,
soul, 'to give up the ghost', or representation of the soul as a 'gaseous vertebrate', *Pneuma*, etc.). For its own thought-collective it appears to be quite obvious, natural, and it does not require any motivation, for it suggests itself as an indispensable necessity. Now it is important that such a proto-idea retains this quality of seeming obvious even after differentiation of the notions which comprise it, i.e. even after their disintegration and after a transformation such that the proto-idea is no longer directly contained in the notions themselves. It becomes then a subconscious guiding principle for the development of notions, which is shaped so as to enable it to be demonstrated.

Such were precisely the paths of the proto-idea regarding the identity between fire and life. The notions of heat and cold, which contained this proto-idea, underwent a fundamental change. Many similarities between fire and life lost their real meaning; a 'fiery temperament' or 'fiery drink' are today only poetical expressions, but the idea of life and the idea of heat underwent such a development, and such a limitation, and they acquired such a meaning that the specific connection between them was preserved: 'life' was understood in terms of energy during the various stages of the development of the thought; and parallel to the development of the idea of heat, the proof was found that the essence of life is combustion. The entire development of science in this field can be considered *ex post* to be the solution of this problem: how to define fire and how to define life so as to maintain between these two definitions the same connection which prehistory considered to be obvious.

But it would have been *theoretically* possible for the development of the notion of life to take place in the morphological direction, and not the functional one. In such a case, crystals, for instance, could be counted among living beings, and life thus understood (i.e. as the ability to produce specific shapes) would bear no relationship to combustion. Then, we would pay more attention to the morphogenesis of living forms, and it might not have taken until Pasteur's time for the thought to be established that every living being, even the simplest and smallest one, descended from living beings. Or, the other way round, it would have been possible for caloric to remain a chemical element, just as Lavoisier wanted it, and then the development of thermodynamics and energetics in general would have been inhibited. In such a case, life, i.e. the flow of matter (if we assume that the development of this idea nevertheless would take place in a functional direction) would, likewise, not be equivalent to combustion; it would have found a symbolic picture, e.g., in a stream or river, and it must be clear that an embryonic proto-idea of such an analogy did exist, though it was to be much less strong than the proto-idea
of an analogy with fire. I do not claim that such possibilities did exist in reality, i.e. that a concrete and detailed consideration of all factors which were operational in all epochs would enable us to adopt them; on the contrary, I am convinced that the more accurate our study of the development of the ideas involved, the more complete would be the historical determination actually found. It is precisely my wish to underline this specific historical determination of the development of epistemological thinking, as opposed to logical or real determination, which can be discussed only within the limits of a more or less established style.

Therefore, in what way does the original, primary development of thinking take place?

Thought-collectives give rise to convictions, views, mental connections and pictures in a manner which is very similar to the formation of words, expressions and linguistic habits. Words are not originally conventional names of things, but their real equivalents, they are "a transposition of experiences and things into a material which can be easily shaped and is always near at hand" (Hornbostel). The word is the living picture of the object; even more than that, its magical equivalent. Similarly pictures and notions arise, as well as conceptions, i.e. their connections. These are the spontaneous transpositions of experiences. The conviction of the analogy between fire and life did not arise as a logical deduction from a certain number of premises; rather it is an expression of the experience of this analogy, or even more: it is a direct experience itself of that analogy. It is a vision of the thought-collective, which shocks its mental life so profoundly that it cannot be rejected any more. Being itself without any stable sense, since it provides the mutual relation of notions whose content has not yet been established, it becomes a guiding principle for the assigning of this content. Before the content of the notion 'life' had been fixed, there was an unclear proto-idea of the identity between the essence of life and fire. This unclear thought became the guiding principle of the development of the notions of life and fire: we have even today just such an understanding of life and fire as to satisfy that proto-idea. If we read today the old views of fire and life ex post, we find the 'true' thought, i.e. a thought identical with ours, and, lacking proofs, we are inclined to assume some miraculous folk intuition. But this is only an illusion: that proto-idea had another meaning than that we suspect in it, for 'life' and 'fire' meant something else then than today. However, these ideas, once correlated by it — in spite of the far-reaching developmental changes within each of them — retained their mutual mental connection.

We find quite a few similar fixed proto-ideas in the history of the sciences
which are the guiding principles of the subsequent development of some fields. They result finally in scientific views whose original nucleus, a mental protoplasm which is unclear today to us, did exist prior to modern empiricism and prior to the modern method of understanding them, and nevertheless the development out of these connections is, at the same time, the development of empirical knowledge and of ideas. Every single stage is thus empirically and systematically legitimized, and each impresses the participants as the only possibility, indeed as the equivalent of the independent external being, i.e. of 'reality'.

Elsewhere we examined the problem of the proto-idea relating to alteratio sanguinis laetica which is much older than the present-day notions of blood and syphilis; this proto-idea became the guiding principle of the development of both these ideas, and is today carried into effect in the so-called Wasser- mann reaction. There is no logical connection between this reaction and that unclear proto-idea, but there is a strict genetical connection; and the development of science in that field can be considered to be the solution of a specific problem: how to specify the notion of the blood test and the notion of syphilis so as to have a certain prejudged connection between them.

Likewise there were the proto-ideas of the atom, the chemical element and chemical composition, of the conservation of matter, of the spherical shape of the earth, of the heliocentric system, etc. All these thoughts existed prior to present-day proofs (i.e., they were differently motivated). The notions which expressed them underwent a fundamental change, but nevertheless the connection between them remained, and even was one of the guiding principles of the development of these notions.

There also was a vague thought that the essence of illness was a worm or a poison which corroded the body. In one case a worm, in another an evil spirit or poison, sometimes numerous tiny worms, visible or invisible, large or small. This fantasy can be found in different epochs and with different peoples, in the same way as the bow or the sling is widespread. In M. T. Varro (Rerum rusticarum libri III) we read:

Animadvertendum etiam, siqua erunt loca palustria . . . quod crescent animalia quaedam minuta, quae non possunt oculis consequi et per aera intus in corpus per os ac nares perveniunt atque efficiunt difficiles morbos.

This sounds to us as a modern popular description of Flügge's droplet theory of infection through animalia quaedam minuta, i.e. bacteria. True, the malaria parasite which is found in moors, enters not through the nose or mouth, but via the sting of a mosquito. The idea of animalia minuta (Varro probably
borrowed them from the Greeks) developed into bacteriology, but it does not refer to such diseases as occur under conditions described by Varro, and not to such bestioles of which he wrote. From the modern viewpoint it cannot be called erroneous or right: objectively it is erroneous for paludal diseases, and contains an unclear idea, which is important as a nucleus. This was not an 'intuition' just because what has been achieved is different from what had been expected. Likewise it was not a supposition, the more so as its character was not that of a supposed possibility but of a ready judgment. It was a prejudice, and it became truth in the same way as the condition of the devil's contract from Mickiewicz's ballad proved correct: the inn had been called 'Rome' in order to bring Twardowski to Rome.

One has therefore to admit that the prehistory of thought presented to posterity some guiding principles for further development of thinking, in the form of a series of unclear complexes of supposed connections and analogies. During subsequent development, the notions developed basically according to those guiding principles, but other factors are also active.

As soon as the collective brings a specific style into being, and the work of legitimizing notions in accordance with the established principles of style begins, their meaning differentiates: the word becomes equivocal, and each meaning begins to lead a separate life. In a book published in 1755 (Odilon Schreger: Studiosus jovialis, Pedeponti 1755) we read, e.g.:


The notion of heaviness which we have here differs completely from the present-day physical one: it contains the non-differentiated set of the contents of the present-day notions of weight, ponderousness, heavy disposition and difficulty (awkwardness) of lifting. However, this archaic notion of heaviness presents a compact and stylized whole: its opposite is 'lightness', and the difference between the corresponding phenomena finds a consistent explanation in the presence or absence of those spirits of vitality which have a connection with air and fire. We cannot charge this notion of heaviness with anything, neither from the standpoint of logic nor from that of empirical knowledge. A balance is not at all an instrument for measuring this heaviness, and besides, if a balance is not used in a vacuum, it will confirm that a warm
body is lighter than a cold one, and, after air has been removed from the weighing space (if such an experiment could be made available to the adherents of the given old style), the balance gives the 'heaviness' wrongly because 'Lufit macht insgemein leichter': obviously there was more air (i.e. counter-heaviness) in the living and satiated men, and therefore after the air had been removed they acquired a weight which was equal to or greater than that of a dead or hungry one! One cannot devise an experiment which would convince our author, even leaving out of account that he would not agree to carry out a modern experiment without seeing a connection between its foundations and his own statement: our physical reality is completely foreign to him. Only after changing the thought-style would he be able to distinguish between several meanings, in a notion of heaviness, previously consistent for him, and to grant possibly to one of them the developmental trend identical with our classical physical notion of weight. Later, in the course of a further development of the style, he would face the need to further divide this notion of weight, etc. — just as physics had to distinguish between 'weight' and 'mass', and will probably in the future divide each of these notions in a manner which is today not to be predicted, and which is unnecessary.

Apart from the differentiation of notions, many other phenomena contribute to the development of a thought-style. Some notions and problems disappear, others appear for the first time. This occurs regardless of objective or logical arguments: e.g., the problem of the philosopher's stone, i.e. of an agent for getting eternal youth and making noble from base metals, — not because all studies had thus far been without any result (though many alchemists affirmed that they possessed it), but because this problem is at variance with our thought-style (we know on the basis of general principles that it is impossible). Unbreakable glass has also been sought for a long time and without success, but this problem is not given up, because it agrees with our thought-style.

There is specific readiness to perceive forms which agree with a style, and at the same time the ability to perceive the non-stylish phenomena disappears, and a suitable technology arises, etc. Unfortunately it is impossible to treat in more detail here all of these phenomena of the development of thinking, i.e. of historical changes in mental styles.

V

If epistemology is meant to be a science capable of development, useful and rich in a substantial way, it ought to broaden the range of its interests. It must
not be limited to the study of the domains and stages of science which are officially recognized at the given moment, but, taking into account the variety of thought-styles and the multiplicity of thought-collectives, it must become a comparative science. Likewise it ought to take into account the developmental moment and, while including the embryonic stages of cognition, it should aim at the research methods of unclear, waverling and indistinct cognition. It must take into account fundamentally and in detail the social nature of thinking and cognition.

Consequently it ought to include psychological, sociological and historical methods. Its subject will be the whole of cognitive life, its organization, fluctuations over time and developmental peculiarities, local features, properties of its various forms; it will study pedagogic methods from the epistemological viewpoint, it will find points touching economics, technology (apparatus!), art and even politics. Finally, it will take into account mythology and psychiatry.

Epistemology thus understood is a science of thought-styles. When comparing thought-styles, studying their historical genesis, their development, the social forces which produce and maintain them, the methods of introducing them into the thought-collective, epistemology will earn a positive opinion from contemporary 'official' scientific knowledge, for its possibilities and its philosophical value. It can form a specific outlook upon the problems of reality, truth, illusion, discovery and error. It can supply useful factors and indications to individual sciences.

If the perception of any definite form, if the discovery of a detail, if the formation of any view, depend on specific intellectual readiness, upon the intellectual mood of style, — as I have tried to show in my work 'O obserwacji naukowej' (On Scientific Observation) — and if that mood is a social phenomenon and finds its conditioning in the specific tradition of the thought-collective, then the first task is to study the sociology of thinking; Cognition is a collective activity, since it is only possible on the foundation of a certain store of science, acquired from other people and supplying only in that way that general background against which a certain observed or understood form can reflect. The sentence 'John got to know the phenomenon Z' is incomplete: one has to add to it 'in the thought-style S', or also 'from the epoch E'. It is the thought-collective that is the carrier and author of the thought-style.

There are momentary and there are stable thought-collectives. Every one has probably observed that, during a vivid, absorbing conversation of several persons, after some time a certain specific state arises which causes the participants to utter thoughts which they never express in another collective.
A common intellectual mood appears and, finally, from the mutual understanding and also the mutual misunderstandings, a specific mental creation arises whose authorship does not belong to any person, but only to that collective. The arrival of a new person destroys or modifies that mood, and the relevant common creation is changed. A very intense common mood can lead to so-called collective suggestions and hallucinations, just as a trained, uniform mood of a stable collective leads to an outlook on life and its application in practice.

In addition to such ever originating and disappearing momentary collectives, there are also stable collectives which are grouped round some fixed social compositions such as particular sciences, religions, practical professions, stable associations with definite aims, e.g. sports etc. Whenever forces collected a group of people and act for a longer time, such stable collectives are formed. Then, in the course of time, a fixed mood becomes consolidated composed of two closely and mutually connected sides: a readiness for directed perception and a readiness to act suitably in a definite manner. In this way a specific thought-style arises. This style is then transmitted from one generation to another, by ‘initiation’, training, education or other devices whose aim is introduction into the collective. The collective develops, grows, and creates its corresponding expression in the form of religion, science, art, folk customs, the state, etc.

The fundamental feature of all stable collectives is their more or less exact delimitation. The thought-collective is delimited formally by customs and statutes which subordinate the admission of members to some conditions and ceremonies (sacraments of admission), such as those for the members of specific religions, specific professions, etc. Also the special disciplines have conditions of admission which are by no means logically motivated, but merely traditional. Classical (Greco-Roman) training has, e.g., in Europe been the condition of admission among scholars, and such classical education, while lacking any logical motivation, is one of the components of the scholarly thought-style. Among the formal factors limiting thought-collectives, we can also number special words (‘technical definitions’) used in that group, sometimes special phrases, and even a separate language (Latin!).

Up till now there are no special studies of the epistemological meaning of technical terms, but I am unable to attempt to fill this gap, as this would require much space. It is a fundamental matter that the technical term expresses, within the relevant thought-collective, something more than what corresponds to its logical definition: it possesses a certain specific power, being not only a name but also a slogan or symbol; it possesses something I
would give the name of a specific thought-charm. One can easily convince oneself of this by substituting for the technical term, a description with the same meaning, but without that specific sacramental power, without that charm: 'king' is something more than 'person in authority'; 'master' is something more than 'man who has mastered a certain skill'; 'element' is something more than the 'component brick' etc. We speak of a 'royal pronouncement', 'masterly work', 'elementary phenomenon', and in each such definition there is a certain tinge of style which can be sensed only by somebody who is included within the given thought-style. If, in a legend, we substitute 'hereditary president' for 'king', we shall transform it into a parody and destroy its specific mood. This refers in particular to old terms that have been founded and inculcated into thinking by a lasting thought-style. In a macabre tale, the substitution for 'spirit' of the description 'a component of man which outlasts his death' will destroy the entire mood, and make it comic. Similarly, in a patriotic poem, the substitution of 'surface of the globe' for 'earth'. But modern scientific terms have this specific thought-charm, this specific sacramental strength, too.

This strength is contained in such terms as 'species' (zoology and botany), 'atom' (physics and chemistry), 'analysis' (chemistry), 'diagnosis' (medicine), 'germ layer' (embryology), 'organ' (anatomy), 'function' (mathematics), etc. None of these terms can be fully replaced by a logical explanation, for the tradition of the given discipline and its historical development have surrounded it by that specific sacramental power which speaks to the members of the collective with a greater strength than the logical content. Were it not for the power of the term 'species', the struggle for evolutionism and Darwinism would have been impossible; were it not for the similar power of the phrase 'squatting the circle', perhaps nobody would care two hoots about it.

Just as for technical terms, so also for special phrases, mental customs etc.: all of them are specific features of thought-styles, inaccessible to strangers, and in a certain sense sacred for the initiated. Their replacement by other expressions and phrases, even identical as far as logical content is concerned, but without the style features, produces a satire or a parody. Such was the origin of many of Voltaire's satires, cf. L'Ingénue or Candide.

Thus style is a limited unit, a closed organism, and there is no access to it by way of a universal, so-called 'logical' or 'rational' path. All educationists know that the initiation to any field of thought must always lead through an 'apprenticeship' period in which only authority and suggestion are active, but not any general, 'rational' interpretation. These introductions have, in all fields, the value of the sacrament of initiation, which is known from
ethnology. No discipline can be learned by studying its mature notional system; there must always be an ‘introduction’ which is partly historical, partly anecdotal and dogmatic. It is a training in subjecting oneself to the specific mood of the collective. One cannot understand any domain without knowing its historical development, but one also cannot understand this development without knowing its present-day notions: this alone makes a rationalist pedagogy impossible.

All stable thought-collectives, as carriers of organic thought-styles, possess an identical general inner structure, much as in its details this structure can assume various forms. The force which maintains the collective and unites its members is derived from the community of the collective mood. This mood produces the readiness for an identically directed perception, evaluation and use of what is perceived, i.e. a common thought-style. At the same time it is the source of that feeling of the intra-group mental solidarity mentioned before, of that specific comradeship which produces the ‘comrade’, ‘countryman’, ‘coreligionist’, ‘colleague’, etc. Something contrary is the feeling of hostility to the ‘stranger’, to a man who worships foreign gods, uses foreign words devoid of the charm felt within the collective. He is a ‘dumb’ one, and his sentences are either nonsense or illusions (cf. ‘Scheinprobleme’ of the modern natural sciences). His utterances, which destroy the intellectual mood of the collective, rouse hatred.

Not all of the members of the thought-collective have the same attitude towards its products: a smaller part play the role of sui generis intermediaries with respect to the — larger — remainder. In this way there arises, round every product of collective life, a smaller esoteric circle, composed of members having a more direct relationship to this product, and a larger exoteric circle composed of members who participate in it through the intermediation of the other ones. In the collectives of religious, artistic, scientific thought etc. we find the esoteric circle in the form of priests, artists, specialists etc., and the exoteric circle in the form of the faithful, public, laymen etc. Every scientific idea, every artistic or any other thought possesses its few ‘initiates’ and its more numerous ‘adherents’ who adopt this thought through trust in the initiated ones: without such a division of roles and work any coexistence within the collective would be impossible. It is precisely this division that results in the collective being not the simple sum of individuals, in the fact that the circulation of thought in it has creative capabilities whose result is not an individual work, but precisely a collective one. The collective consists of many such mutually intersecting esoteric and exoteric circles, an individual belongs to many exoteric circles and to few — or no —
esoteric circles. There are various degrees of initiation and numerous links between them, just as there exist paths and links which unite different circles.

The exoteric circle is connected with the esoteric by specific social forces. On the one hand it is the specific trust of laymen in the 'initiates' or specialists, and on the other it is the specific dependence of the latter on so-called public opinion and so-called common sense. The effect of both these forces is identical: they strengthen, intensify and carry into effect each thought circulating in the collective, impart to the products of this thought the specific mark of the extra-individual, 'real' being. Because of this trust, the layman has a tendency to overestimate the capabilities of the specialist and to underestimate his limitations. Thus every mental production of a specialist, e.g. an artistic or religious idea, will acquire, in its migration to the laymen, the features of a greater certainty, greater completeness, greater obviousness and weight. But the specialist himself is not independent of laymen: his dependence on 'public opinion' and 'common sense', conservative from the nature of things, induces the specialist to adapt each novelty to the adopted set of opinions, and equip it automatically with identical features of greater certainty and weight. This is furthered also by the general feeling of intellectual solidarity among all members of the thought-collective, as mentioned before, and by the fact that specialists are recruited from among the laymen, and therefore every specialist owes a tremendous proportion of this general education to this established conservative exoteric knowledge.

The result of this is that every movement of thought within the collective — ipso sociologico facto — intensifies and individualizes it, just as its circulation among the collectives changes and transforms it. These are the general rules, common to all collectives.

In addition to the trust of the layman and dependence of the specialist, there also exist other social forces acting within the collective. One ought to mention, e.g., the characteristic competition between the elite and the mass, the mutiny of the mass against the elite, as a result of which the layman derides the 'priest', and the dislike of the elite for the mass, as a result of which the specialist despises the layman.

The complexity of human life is expressed in the simultaneous coexistence of many different thought-collectives and in mutual effects between these collectives. A modern man never belongs — at least in Europe — exclusively and totally to one collective only. By profession, e.g., a scientist, he can be besides a religious man, can belong to a political party, can take part in a sport etc. Besides, everyone takes part in the collective of the practical thought of
'everyday life'. In this way the individual is the carrier of the influences of one collective upon another. Contradictory thought-styles intersect in it often carefully isolated from one another [cf. a physicist who is a religious man], they clash, are modified, assimilate. These external influences become one of the factors providing that creative chaos of flickering alternate possibilities from which later, by way of a stylizing migration within the collective, a new form, a new discovery arises.

Thus there are three sources of conditioning of the concrete contents of each thought-style: (1) the prehistoric ideogenesis from the period of the beginnings of that style, from the period when that style split off as a variant of another style. The proto-ideas belong here. (2) The changes brought about by the continuous migration of thought within the collective, dictated by social forces acting on the collective. Stylization, systematization, legitimization, revolt, mental revolution. (3) Constant effects of foreign styles.

The social structure of thought-collectives, described above in general outlines, assumes various forms in the particular collectives. The relation of the smaller esoteric circle to the larger exoteric circle, which is one of the forms of the élite/crowd relation known from sociology, can assume different forms. Collectives in which the position of the crowd is stronger than that of the élite, have certain democratic features: the élite strives for the trust and appreciation of the mass, stressing that it serves their common property and satisfies public opinion. The highest criterion is the 'appreciation of everyone'; everyone can and must discuss every truth. Such thought-collectives have open limits, and willingly accept new members; a tendency to development and progress follows from their foundations: their ideal lies in the future to which one should aim by way of work. An example of such a collective is the community of natural scientists.

Other features are found in those collectives in which the position of the crowd is weaker than that of the élite: the latter tends to maintain distance and becomes isolated. It stresses the supernatural origin of the ideas it represents, and its significance requires obedience and docility. The criterion of truth and acceptance is found in some single master, often a mythical one. In such collectives, ceremonialism and dogmatics develop. They are more or less exactly limited and conservative: their ideal lies in the past, in events, powers and revelations which took place in the past. Examples of such a collective are the majority of religious communities.

A more complex structure is found in some thought-collectives. For example, the thought-collective of developed fine arts has, beside the artists
and the so-called public, also the critics who mediate between them and, on their part, exert an influence both upon artists and upon the public. The critics constitute a specific knowledge of art in which both the artists and the public form the esoteric circle. Thus this thought-collective possesses two esoteric centers which are mutually conjugated and, in many respects, oppose each other: the center of artists and the center of critics; and two exoteric areas which partly intersect: in one case it is the public and the critics, in another the public and the artists. The fact that the collective has two centers makes it impossible to establish the position of the individual with respect to the specific products of the collective: it always oscillates between simple admiration and criticism. Owing to this oscillation the products of this collective are constantly in the stage of flickering, which, in the science collective, shows up only for the latest discoveries. We say that the art products are subjective and that they depend on position. But this is absent in folk art which has no critics: for country folk, the commands of its art are equally as objective, compulsory, the only possible, as the laws of nature or religious dogmas.

The set of people setting a new fashion in clothing displays a specific mental social structure. In this collective the esoteric center has a marked superiority with respect to the exoteric circumference, hence its nature is aristocratic. However, it differs from the religious collective in that the elite, to be sure, observes its distance from the crowd with precision, and it is ceremonious and dogmatic in its way, and at the same time it avoids that pathos which is not avoided by the members of the religious elite with regard to their person. This humble discretion of the elite of the fashion world finds its reflection in the nature of the fashion creations: fashion is considered to be an 'external' feature and, (despite the whole captivating power with respect to the members of the collective, which is the stronger the farther the individual is located in the exoteric circumference) is never devoid of a certain ironical after-taste.

The creativity of the thought-collective is influenced, apart from its structure, also by many other sociological factors. A large collective works in a different manner — for the continuity of its work is independent of the duration of life of the individual — than a small collective where work is broken down, or even snaps with the death of the individuals. A small collective will never lead to such a wide and organically connected structure as that found, e.g., in the mental community of naturalists. However, on the other side, the size of the collective may become the cause of its break-up, i.e. of its differentiation into several smaller collectives. We can also observe these relations
in the history of the development of the community of naturalists, in which so-called specialization leads to the formation of several separate thought-styles. The larger the collective, the more formal elements and the less other matters.

Moreover, a considerable effect is exerted by the degree of organization of the collective: there exists a difference between the creations of a collective with a firm organizational framework, fixed norms of intercourse within the collective etc., and the creations of organizationally immature collectives.

To the sociology of thinking one should also add the investigation of mutual effects between collectives. These effects take place either by the individual’s taking part in more than one mental community, as mentioned before, or by way of clashes between the members of various collectives.

There exists basically no contact between the completely separate collectives, i.e. those having nothing in common. If two more or less different collectives enter into any connection, there arises a common collective — possibly very short-lived and very poor in content. The more alien these two collectives are, the poorer in content is the common one: in some conditions it has only a very primitive content: hatred and physical struggle. If the two collectives are even more different from one another, any contact and influence cease.

VI

Now I should like to apply the principles of the sociology of thinking to considerations of present-day science, especially the natural sciences.

The modern scientific thought-collective ought to be called democratic: the criterion of truth is found — at least in principle — in the ‘general public’, i.e. in the mass ['general verifiability'] and not in the elite, which clearly stresses that it serves the ‘general public’. There are no secret powers, one cannot refer to a mission obtained from upper circles; every epistemological act should be derived from universal powers, i.e. powers to which everybody is entitled, and from generally adopted formulae.

The *esoteric circle* consists of specialists, and there exists a whole gamut of expertise and a wide specialization in its range. The *exoteric circumference* consists of laymen for whom we have specific ‘popular science’. The borders of this circumference are open to everybody, admission does not require any formal ceremonial. The democratic nature of the scientific collective manifests itself also in that every specialist in a certain scientific field is a layman in most of the other fields, unlike e.g. the collective of religious
thought in which priests form a total élite. Hence the specific relationship of élite to masses, which determines the thought-style of the entire scientific collective: the specialist in one field possesses general education, in popular form, as far as other fields are concerned, and is bound by this education in his specialist creation. This results in important properties of scientific progress, which we shall set forth below [the feedback system: the masses are subject to the élite, but the élite is also dependent on the masses].

The democratic system of the scientific thought-collective shows itself externally — as it is well known — in truly democratic devices such as congresses, scientific press, scientific discussion, and the democratic settlement of the opinions of `the majority of research workers', i.e. in the origination of public opinion. These are relatively recent features, since they developed for good only in the 19th century, but our present-day thought-style stands and falls together with them. Some features of the scientific collective remain as a heritage from previous epochs such as the hierarchical titles ['Master', 'Doctor', 'Professor'], some academic ceremonials, some old-fashioned exclusivity among specialists, etc.

The esoteric center of the scientific collective is divided nowadays into specialists *sensu stricto* [professionals] i.e. specialists dealing with a certain problem, such as a specialist in aniline compounds, and more general specialists e.g. chemists. The esoteric circumference also possesses its hierarchy: we have 'laymen with general education' and the 'general public' without this education. The specialists are almost always recruited from among the general specialists, their positions as such is often of a transitory nature: they frequently change their specialty and, having completed their research in a certain direction, they return to the ranks of general specialists with whom they always remain in contact. A general specialist is almost always recruited from among people with general education. On the other hand, there are in current social conditions no direct transitions between the circle of laymen with general education and the 'wide public': general education is acquired before maturity; whoever failed to acquire it in school remains mostly for good in the circle of the wide uneducated public. Since we have here the most impassable border of social strata, it is here, naturally, that one finds some very important (from the epistemological viewpoint) breaches of democratic principles.

The usual course of things is that an adolescent obtains a general education which, from the standpoint of comparative epistemology has the value of the medieval *minor orders* (the sacrament of initiation). Having passed this stage, the essence of which consists in an authoritative introduction into the principles of the scientific thought-style, this style now operates as a compulsion
for this thinking and not of any other one. It is from this moment onwards that scientific problems become understandable, the proofs binding, the scientific objects visible, the results verifiable and applicable.

‘Common sense’, the personification of the thought-style of everyday life, obtains a surface appearance in the direction of the principles of the specific scientific style. Examples of changes may be the exchange of a couple of notions: ‘top — bottom’ for the idea of the ‘distance from the center of the earth’, or ‘hot — cold’ for ‘temperature’ and ‘amount of heat’.

This border of social strata is naturally not absolute: scientific education percolates also, without the help of an official school, to the layer of the general public, and it does happen that an uneducated layman enters the circle of laymen with general education even in his period of maturity. Still the gap between the public at large and educated people is very clear. Its result is in the first place a certain discrepancy between the main postulate of science which wants to be universal and its significance limited to the borders of the collective. The aim of the scientific thought-style compels us to take into account ‘common sense’ which is the specific result of tradition and of the forces acting upon everyday life; but the proper development of the scientific style forces us frequently to deny common sense: hence the discrepancy between the perspicuity [Anschaulichkeit] and the accord with the system of scientific notions [Systemfähigkeit] which is visible today in physics.

The layers of the scientific collective correspond to the separate forms [of communication] of scientific thought. A specialist expresses himself in a scientific journal, the general specialists find their expression in a scientific textbook, while a popular book corresponds to laymen.

In the journal stage, science has clear personal and interim features: They are the opinions of the author X, not yet ‘adopted generally’. They do not provide as yet any general picture, they have many reservations, being like a tile which waits to be placed in a mosaic. The author is aware of their provisional character and wants to compensate for it: hence that pluralis modestiae, this invocation to the collective, which forces the specialist to hide behind the imaginary multitude. Here also belongs the characteristic caution of the journal: the disciplined author writes that “he tried to prove that . . .”, or that “it appears to be a fact that . . .”. It is only in a textbook that we read such sentences as: “It has been demonstrated that . . .”, “it is a fact that . . .”, because a judgment about the existence or non-existence of a phenomenon belongs, in a democratic collective, to a numerous council, not to an individual.
The textbook changes the subjective judgment of the author into a proven fact. It will be united with the entire system of science, it will henceforward be recognized and taught, it will become a foundation of further facts and the guiding principle of what will be seen and applied until a new developmental wave will wash it away. In the meantime it will pass into popular literature.

The popular book will visualize that fact, will change the proved fact which is in agreement with the system of science, into a directly perceptible form. Proof passes into shadow, authority becomes active, with the magic of the simple announcement that ‘the scientists have found that...’, and the corresponding apotheosis of the heroes of science. Social distance transforms the author from a creator to a discoverer. The developing scientific fact changes from mental composition to an object. It becomes impersonal, self-contained, it becomes a thing.

But, outside his speciality, the specialist is a layman. He brings popular notions into his field, and adjusts his mental compositions to them. He draws general epistemological material from popular science: it is from there that the idea of truth as representation of reality independent of the subject of cognition originates; this idea returns to specialist knowledge as the specialist matures. Thus the circle of the social migration of thought closes in a scientific collective. The factogenesis a motu sociali, which takes place during this migration, passes through 3 basic stages: (1) resistance against the alternate mental possibilities, cf. my ‘Scientific Observation’,* (2) the proved fact, (3) the perceivable form (thing).

This is of course a very summary and schematic picture. An exact study of the sociology of scientific thinking would probably require many volumes. Even the analysis of the transition from the journal to the textbook stage requires much more space than the present paper, and the analysis of popular knowledge would have to be even more spacious.

I should like to mention two measures which are at the disposal of the scientific thought-style for giving the character of things to its creations.

One of them is technical terms, whose general significance we touched on before. The specific power of scientific technical terms consists, to a large extent, in detaching their significance from the subject of cognition, hence in establishing the ‘objective’ meaning. In this way the object being defined becomes independent, as if possessing absolute existence. Even if the technical definition includes the name of the author, the factual character of the defined object is given: cf., e.g., ‘the Wassermann reaction’. It is the name of the person that becomes the definition of the thing [one says, e.g., “to perform a Wassermann” instead of “to carry out the reaction given by Wassermann”]
rather than the technical term loses its isolating meaning. Some problems do still exist only because they are linked to the style term: e.g. Besredka's antiviruses, Löwenstein's anticutins.

Scientific terms are often not arbitrary words but rather words built in a systematic way from radicals and suffixes whose meaning is accepted in advance [see examples just given]. Such terms do prejudice in advance that the specified object has a fixed place in the system of the given science; therefore their suggestion is particularly strong. It is only one step from them to scientific signs [e.g. chemical ones] and to the symbolic calculus [logistics, mathematics]. At this stage the objectivization of mental products is the strongest: they acquire the features of a complete independence from man.

Another measure is the scientific device. The analysis of the epistemological significance of a scientific device would also require a separate study. It can be mentioned briefly that a scientific appliance, which is a realization of some result of a definite thought-style, directs our thinking automatically on to the tracks of that style. Measuring instruments force one to apply the notion of unit for which they were constructed; even more so, they force one to apply the notions from which they originated; whoever uses a balance can no longer use the notion of weight as given in 1755 (quotations on p. 96 above); whoever uses a thermometer, must not use the archaic notions of heat and cold (given on p. 90). On the contrary, he ought to consider such notions impossible and erroneous. A telescope makes it impossible to see 'fantastic' forms in clouds, i.e. forms foreign to the scientific style, which means that the telescope directs us toward the scientific style, just as molten wax, a pack of cards or other similar devices direct the fortune-tellers towards their thought-style. Living as we do among devices and instruments from the current scientific thought-style, we always obtain 'objective' stimuli urging us to that way of thinking but no other. Hence the conviction that the meaning of that style is independent of man and 'factual', and that the products of that style have a 'factual' nature. A telescope shows Saturn's ring; a man reared in the scientific thought-style does not understand that to recognize a relation between the image seen in a telescope and the distant planet, one has to think in this style. What is more: even such notions as a 'planet', 'image seen in the telescope', 'distance' or 'relation' include this style. Looking through a telescope and seeing in it that image (and not, e.g., the reflection of one's eyelashes), and the disposition to infer from what one sees in that tube to what is 'in the sky' are already elements of the scientific thought-style. Whoever can look into the telescope and think of Saturn consequently uses a certain definite thought-style. There is no other possibility
for him: he must recognize the ring of Saturn as a reality independent of himself, and his own thought-style as the only 'good' one. It is in this that the role of thought-style consists: for its participants there do not exist two possibilities; and between the participants of two separate styles there is no communication.

Speaking of thought-style, we ought to remember that the actual social circulation of thought within the collective is itself not the only style-determining factor. The examples given before (heat - life, blood - syphilis) illustrate in what way the historical course of mutual influences of various thought-collectives determines the fate of given problems. Many elements of the present scientific thought-style are derived from the historic intersection of style influences (a congressu historico). A typical example is the notion of a chemical element, which originated from the synthesis of the ancient or medieval notion of an element and the modern notion of weight, authorless, accepted a motu sociali within the collective.

Each stage of science is a function of the preceding stages and of the effects of foreign thought-styles. "Das wissenschaftliche Denken setzt die Ganzheit der vorwissenschaftlichen Erfahrungen und Begriffsbildungen voraus und ergänzt, verschärft und korrigiert das vorwissenschaftliche Weltbild" (Jordan, 'Über den positiven Begriff der Wirklichkeit', Naturwissenschaften (1934), H. 20, 485). An ahistorical cognition, abstracted from history is impossible, and just as impossible is the asocial cognition, conducted by an isolated researcher. An 'empty mind' does not perceive, does not compare, does not supplement: does not think. Every research worker must pass through a training stage which consists in referring to the tradition, and which must admit the division of research work: both these moments automatically create the social problem and launch social and historic forces. A theory of cognition which does not take this into account is just simple play.

Natural sciences whose ideal (cognition of truth) is in the future — unlike the aristocratic collectives whose ideal (e.g. revealed truths) is in the past — are often compared with a marching column: the front consists of the vanguard composed of specialists who pave the way, followed by the main detachment whose nucleus is the general staff (general specialists), and basic strength is the totality of the collective members (laymen). One could even speak of the rearguard, the straggler. Now just as every single position of the marching column is determined according to the position of the main detachment, and not according to the always varying and labile position of the vanguard, one also determines the official position of 'science' from the position of the general specialists, and not from the 'individual' opinions of
THE PROBLEM OF EPISTEMOLOGY

the leading specialists. This at least is the practice of scientists. However, the classical theories of knowledge, which do not recognize the historical development of thinking and the sociology of thinking, have quite a lot of trouble with this.

They admit that the transition of a statement of a specialist from a journal to a textbook does, and ought to, depend solely upon the verification of that statement, and is, in principle (or should only be), the expression of that verification. The specialist discovers, but it is the general specialist who verifies. However, in the developing science (and, in principle, all sciences do develop), there occurs, simultaneously with the ‘verification’ of the earlier discovery, a new discovery of the specialist, which more or less modifies the former one. Thus the verification of the statement occurs simultaneously with its decline: as soon as a systematic textbook is published, it is always already obsolete. A question concerning some special matter from the field of developing science must always be answered in agreement with the textbook (‘official’) state of the science and with a series of other solutions which are the personal views of some leading specialists or some schools. Where is truth in the interpretation of classical epistemology? At which stage of scientific cognition? The judgment of the modern textbook consists in verification, hence ‘proved to be right’, but one of the personal judgments will also pass into a future textbook, hence also ‘will prove right’, and, to be sure, when this will happen, there will be new statements which will deny this. The notion of truth in its classical significance, as a value independent of the subject of cognition and of social forces, compels one to accept truth as an unattainable ideal, and the history of science teaches us besides that we do not approach that ideal, even asymptotically, for the development of science is not unidirectional and does not consist only in accumulating new pieces of information, but also in overthrowing the old ones.5 Thus classical theories of cognition ought to distinguish between: (1) the ideal, unattainable truth, (2) the official ‘truths’ which ‘should’ somehow approach it, (3) illusions and mistakes. At the same time they have to admit that there is no general criterion of truth. If we overlook the material difficulties of these views (especially when we examine the history of science), they remind us, even formally, of the clumsy theory of epicycles. The problem of cognition and of truth appears, from this viewpoint, as in point of fact, unexaminable, and a scientist realizes that to occupy oneself with them is an indecency sui generis.

The epistemology which is the science of thought-styles, of their historic and sociological development considers that truth is the up-to-date stage of
changes of thought-style. This does not simplify the problem, but renders it explorable. Once such a possibility has arisen, nothing and nobody will remove it for good.

Each scientific cognition is, in the first place, a disappointment as, while it satisfies us with a surprise, it also destroys admiration for what it displaces. But, in the next stage, each solution creates a number of new questions, and therefore the cognitive disappointment is followed by a new deeper admiration. One can expect that the disappointment which must accompany the decline in a certain sense of the naïve classical theory of cognition, will be followed by a period of a deeper fascination with perspectives which are opened up by the theory of thought-styles. If this theory will overthrow only that evil spell of doggedness with which the fanatics of their own style fight the people of a different style, its cultural role will be found to be of high value. If it only uncovers the mechanism of action of each bit of propaganda, it will already immunize us against an absolute submission to propaganda: it will teach that man stands above the idea, because he is the idea's creator.

Yet in the first place the theory of thought-styles throws a specific light on the relation between 'reality' and 'cognition': the chasm between 'nature' and 'culture' fades away because the cognitive activity (note: the collective one, which creates a separate thought-style) is not a unilateral action as, e.g., the figurative rendering of a certain object, but consists in a bilateral interaction: The thought-style creates reality, not in a different way from other products of culture and, at the same time, itself undergoes certain harmonious changes.

NOTES AND REFERENCES

2 Józef Rostafinski (ed.), Cracow, 1893.
4 Przegląd Filozoficzny (1935).
5 These difficulties cannot be unravelled even by many-valued logic alone (Łukasiewicz, Post, Zawirski) or by probability logic (Reichenbach), as logical calculation is only possible in relation to uniformly clear (understandable) sentences, and not when, inter alia, sentences with an unclear content and non-comparable notions are involved, i.e., sentences expressed in a foreign style or in a style which is precisely that of the period of faster development.
PROBLEMS OF THE SCIENCE OF SCIENCE [1946]*

LUDWIK FLECK

It is highly interesting to establish to what extent scientists who devote the whole of their life to the problem of distinguishing illusions from reality are unable to distinguish their own dreams about science from the true form of science.

In the first place there does not exist, beyond dreams, only one kind of science; there are at present only some specific sciences which, in many instances, lack any connection among themselves, and which are sometimes divergent in their basic features. We can discuss science only in the same way in which we use the word 'art' to document the common nature of trends in music, painting, poetry, etc. Similarly all sciences possess a common trend towards an ideal end-state which is known as true knowledge. But just as art is not the sum total of music, painting, poetry, etc, so also sciences do not add up to form a consistent homogeneous whole.

For instance, the connection between linguistics and chemistry is indeed very slender. Let us assume that this should be not so, let us even admit that this will be not so at some time in the future — but, before this happens, both chemistry and linguistics will undergo changes. Today's chemistry is far distant from today's linguistics.

Moreover, no science comprises an objective image of the world, even in the sense of its one-to-one semantical representation. It does not even contain any part of such a representation. Were it so, we would have in science a constant, unchanging part, and scientific knowledge would grow due to simple increase of information; now experience does teach us that this knowledge changes continuously as a whole. Even the most certain, basic elements undergo changes. Every specialist will distinguish an old textbook of his science from the new one: the former is completely anachronistic. If it is a textbook of physics, chemistry or bacteriology of 1910 or 1920, we [in

* Życie Nauki 1 (Warsaw 1946).

1946] shall immediately find that it is obsolete, not only because of the lack of more recent discoveries, but from the trend of basic reasoning.

Sciences do not grow as crystals, by apposition, but rather as living organisms, by developing every, or almost every, detail in harmony with the whole.

I may ignore the constant, final results in my field, but I do know that every result becomes, sooner or later, a source of new problems, and if these problems are solved, then the old results have a sense which differs from that assumed by the author himself. I know that scientific workers often try to reason themselves or other people into believing that it is they themselves who had foreseen this new sense by way of some miraculous intuition; however, documents show that this is not the case. One sees sometimes bricks of one's own work built into the building constructed by other workers. And the author is surprised as a matter of fact that this particular surface has been used for the front face after polishing, while other surfaces were hidden. Sometimes a certain edge of this brick, enhanced by hewing, becomes a part of an ornament which had not been included in the plan. Sometimes one would like to retract, as wrong and unable to exist, one's own thought which had been published at an earlier date — only to find with stupefaction that this very idea had developed and grown up into the scientific surroundings. Scientific results have their own life and pass through their own vicissitudes — changing faster with the faster development of science. It is only the prejudices and superstitions that last, changeless, over centuries, being similar in this respect to the tautological theorems of mathematics or logic.

One would assume that continuous changeability is a transitional state which proves the imperfection of today's science and its tendency towards improvement; that a final state which does not undergo any changes is possible and that we are nearing this state. True, no science at present contains an assured portion of the objective picture of the world, but all sciences are getting nearer and nearer to it.

Since every major discovery has its repercussions upon the whole of science, such a final state, were it only for one only major problem, would be attainable only after all of the problems had been solved. But what does 'all of the problems' mean, when new problems can arise again and again? One would have to stop the movement of the planets, the vibration of pollen grains in air, the evolution of living beings and — what is most important — the movement of human thought; otherwise there will arise new, unexpected problems whose solution will force us to revise the entire system.

Simplicius. You are wrong; the number of truly separate problems is
limited. As sciences make progress, entire groups of problems will be reduced to only one fundamental problem, and, in the first place, the apparent problems will be eliminated.

_Sympatius_. Well, the final state will look as follows: A finished *Codex Pansophiae* and, as an indispensable addition, the *Commentary* to that *Codex*, which would contain, in the first place, all principles of transformation, reduction and elimination of problems. Thus, e.g., the Codex would include no mention about the problem of the philosopher’s stone, but the Commentary would contain a comprehensive article about the development of chemistry from alchemy, and references to the Codex, to the chapter of physics dealing with the transformation of elements, to the biology chapter dealing with hormones, old age and death (*elixir vitae*), to the pathology chapter dealing with illnesses which this stone is likely to cure. On the other hand, the entire mass of practical questions derived from chemistry, viz. as regards the solubility of a certain substance, its melting point, its optical properties etc. — the commentary would refer to the one only formula in the Codex, from which all this could be deduced. The Commentary would likewise contain prescriptions for use of this formula for practical purposes. Every specialist from a foreign field, looking for an answer to the problem would have in the first place to identify it and to transform in the Commentary. A stubborn philosopher would not find any answer in the Codex to the problems of the Absolute, the primary cause, the Idea of well-being, the Essence of all things, but all this would be in the Commentary along with the explanation of the elimination of these questions. A pupil who would like to know what does the wind do when it does not blow, or why did it appear to sophists that Achilles would not be able to overtake the tortoise, and many similar things, will find these problems not in the Codex, but in the Commentary, where it was clearly explained to the questioner why such questions must not be put.

_Simplicius_. Rightly so.

_Sympatius_. I doubt whether logic (along with the logistics beloved by logic) would be found in the Codex or in the Commentary. And what about mathematics? And theoretical physics? In other words, I fear that the Commentary will be much more interesting than the Codex. It will contain the entire history of the sciences, all of the directly practical problems, 99% of philosophy, probably the formal sciences, and popular science for the half-educated ones, as people will probably not be born with a complete education. The Commentaries will also include practical regulations for experimenters, for, even when the experiments of discovery will be unnecessary, they still will remain indispensable for practical purposes.
On the other hand, I believe that the Codex Pansophiae will be, once for all, composed by an international commission in the form of a collection of formulae and graphs, arranged in a theoretically grounded order. Instead of the table of contents we shall have on its first page the formula of this order. The graphs of the text will be \( n \)-dimensional coloured stereo-graphs, to be seen through spectacles with glasses which would change their colour \( 20(n - 2) \) times per second, in order that the details of the graph, when watched successively, would yield the impression of \( n \) dimensions, just as the illusion of movement arises in the cinema. What a magnificent experience it will be to watch such a graph! It would look for all the world like Faust above the sign of the Cosmos!

Since a daredevil among the commentators could desire to change the system of the codex (under the pretext of an improvement), and this could give rise to unpleasant shocks, it will be necessary to give the codex the protection of the law. On the contrary, the commentaries will eagerly change, improve, develop . . .

Do not you think, Simplicius, that, in such a case, your Pansophia would remain unchanged only thanks to the police force and that it would become similar to those dead regulations, such as rituals of religious cults, and that it is these commentaries that would become science proper? That new, unofficial codices would arise again and again, which would acquire more and more adherents? That, indeed, there would be basically no difference between that final state and the present one? That, therefore, this 'final state' — to use your own language — has no meaning at all! If you want, you can assume that we have already reached that state: \( panta rei \) — such is the codex of all things or, if you prefer it; \( A = A \). Everything else — are commentaries to that codex. On the contrary, if you admit that this knowledge is too vague and you want to have a completely detailed and precise knowledge, you have to consider that the universe itself is a system of total-knowledge, while our science is a commentary to it.

Simplicius. I think you are exaggerating again. You will not gainsay that today's science is closer to the objective picture of the world than the science of 100 years ago? And besides, this 'codex', as you call it, must not be separated from the commentaries. On the contrary, beside the positive, exact and certain side of our scientific cognition, we must mention historical data, surmounted errors, pedagogic notes, practical indications, everything in a freer, less exact, tone, in other words artistically.

Sympatius. Thank you for the hope of saving artistry. I am sorry not to be able to reciprocate your compliance. I do not think that today's science is
closer to the objective picture of the world than the science of 100 years ago. On the other hand, I am sure that today’s science is closer to our world of today, while the science of 100 years ago was closer to what was then the world of the creators of science. You yourself do affirm that *consensus omnium* is the ultimate touchstone of science. Do the unborn have a say in this parliament? Do the grandsons vote instead of the grandfathers? In such a case I can assure you that, to our grandsons, the science of 1940 will not seem much better than that of 1840. I am convinced that the progress of science will, in the future, proceed very rapidly, and that ten years will mean more than 100 years before. We, the science workers, are more numerous today than 100 years ago, we have a longer history behind us, our world contains more details, it is more complex — that is why our science is vaster, richer in details and more profound, because of a larger number of intrascientific relations, but that is all. If the ‘final state of science’ does not mean anything, is one entitled to discuss the process of getting nearer to it?

*Simplicius*: I am afraid, Sympatius, that your ultra-criticism and your exaggerated relativism will lead to a barren scepticism. There must be a certain solid and stable foundation of science, otherwise the entire building would be top-heavy. The contemporary splendid technology and its farther possibilities sufficiently justify our science. Our cognitive, technological and mental apparatus becomes better and better — and science is ever progressing!

*Sympatius*: Science is not a terrestrial building which stands on a foundation, with an attic at the top. Science is rather like a round fruit, with a juicy pulp, and a thick, indigestible skin. It may be turned at will, the base can be the top, or the top the base, depending on our desire, but they are both equally tough and indigestible. Only the center of science is useful, while the foundations of mathematics, physics, chemistry, biology are equally tough, doubtful, probably useless, and the top is also the same. In order for this miraculous fruit to grow, it must be taken between two fires: the hot, though dark, fire of romanticism and the cold, but bright, fire of scepticism. For, the romantic daydreaming of the creator is equally as necessary as the envious scepticism of competitors. I would even say that it is precisely this envy that creates the social value of cognition, by deprivatizing the results. The aim of my inferences is not to belittle the value of science, but, on the contrary, to raise it.

There are people who think that one can construct a science of cognition without fundamental observations, experiments and studies in this field. They even believe such observations and studies to be superfluous as they know everything beforehand, by embracing idealism or materialism, intuitionism or
conventionalism, positivism or realism. It is on the basis of a few anecdotes from the history of the sciences, a few of their own life experiences and a large number of suggestions from other people, that they assume a 'world outlook' which explains everything to them.

One cannot look upon the sciences as being only a set of sentences or a system of thoughts. They are complex cultural phenomena, at one time perhaps individual, at present collective ones, made up of separate institutions, separate actions, separate events. Written sentences, unwritten customs, one's own aims, methods, traditions, development. Preparation of the mind, cleverness of hands. A special organizational structure, with its hierarchy, ways of communication and co-operation, an organizational court, public opinion, press and congresses. A distinct relation to other aspects of cultural life, to society, to the state, etc.

I had a very rare opportunity of watching, for nearly two years, the scientific work of a collective composed of laymen only. The results of this observation explain some problems of the science of science much better than speculative discussions. The collective worked on complex problems from the field of typhus; they had at their disposal fully equipped laboratories, plenty of experimental animals and an extensive specialist literature. This was in the Buchenwald concentration camp (Thuringia), so there was a tragic responsibility as far as the results were concerned; and the workers were thrown upon their own resources for, though the German Leiter did hold, true, a wartime doctor's diploma, yet his specialist education was non-existent. His role consisted in supplying materials and urging his subordinates to work.

The collective consisted of: (1) a young Polish physician, without any specialist preparation; he was the head of the collective; (2) a doctor of laws and philosophy — an eminent Austrian political figure; (3) a worker from a factory making rubber articles — a German Communist activist; (4) a young Czech physician, with rudiments of bacteriological preparation; (5) a practising Czech veterinarian, without bacteriological preparation; (6) a Dutch student of biology, with his assistant, a student of the 3rd or 4th year of medical studies; and (7) a Vienna confectioner. I did not belong to this collective, nor did I take part in its work, but I was able to observe it from a direct vicinity. One of the tasks given to the collective was: to examine whether the germ of typhus (Rickettsia prowazekii) is found in the lungs of mice and rabbits which had been infected by a certain method. True, the workers had never seen Rickettsia, nor did they know the standard bacterial flora of lungs and bronchi. Likewise they did not know the cellular elements
of these organs. Consequently, they had to learn to see elementary things on the basis of descriptions and illustrations, i.e. to pass, so to say, backwards along the path normally chosen for knowledge.

There were two descriptions of *Rickettsias*: the older by the German lady-author Sikora, and the more modern one by M. Giroud, a Frenchman. Both contain descriptions and illustrations of the complex developmental cycle of these microbes, though not an unquestionable one. Now the members of the collective found in their microscopic preparations, which had been conducted with an extreme meticulousness according to the instructions contained in books, all stages of the developmental cycle of *Rickettsias* and the required sequence — and yet, at that time, the germ was completely absent from their material. From the dyestuff precipitates, fat globules, various bacteria and cellular remnants they managed to arrange the entire developmental cycle. This did not happen at once. This construction grew slowly, in the atmosphere of a mutual stimulation and strengthening of opinions. The collective mood, which became the motor of this fantastic synthesis was composed of a tense expectation of the effect, of the desire to be the first to establish something, not to be too late with the confirmation that something had been established, and to satisfy the boss who had been urging them along all the time. Thus the components of the atmosphere were, as a rule, identical with those usually met. I have observed such a situation — the birth of discovery.

The *Boss* (reproaches the biologist with having failed, so far, to learn how to dye *Rickettsias*). Had they been properly dyed, one would have been able to see them in the preparations from the lungs of infected animals, for, according to literature, they must be there.

*Biologist* (to his assistant, in order to turn away the attention of the boss). Today's preparations look rather different from the usual ones.

*Assistant*. I kept them just a bit longer in xylene.

*Biologist*. What can be these shining, uniformly pink bodies? We have not seen them thus far. Is it possible . . . .

*Assistant*. I have noticed them, too; their presence struck me at once. Perhaps they are those *corps homogènes rouges* according to Giroud?

*Biologist*. This is what I was thinking.

*Boss* (looking into the microscope). Yes, they might be that.

*Assistant*. Of course, what else could they be?

*Biologist*. At last, we've got them.

*Boss*. And it is high time, too. At last something positive.

In truth, they were the eosinophilic grains from the rabbit's leukocytes,
as I found later. However, among the collective which was thirsting for a positive result, the news was spread: at long last, Rickettsia has been found in the preparations obtained from rabbits' lungs. When the joyful tidings spread among the collective, the certainty of the result became doubtless: the collective placed its trust in the boss, the boss relied on the opinion of his 'specialists' which he had corroborated in order to bear out his own authority, and these 'specialists' might have, at the outset, felt that this might have been something rather involuntary, but the consensus omnium dispelled all doubts. The confectioner and the rubber-factory worker, who represented 'common sense', popularized the discovery seriously and with appreciation. In other words, the social forces acting in the collective were identical with those usually encountered.

Next, from one link to another, the entire cycle grew. What did not fully tally with the findings was explained away by the permissible divergence of statements in this field. Why, even Giroud and Sikora fail to agree between themselves! And then, we know that biology is not mathematics. Die unvermeidliche biologische Unexaktheit — such was the slogan proclaimed by the I.D. and Ph.D. mentioned before in the list of members; he was the last critical resort of the collective.

The development of this 'science' did not proceed at all rapidly — on the contrary, one indulged long discussions and repetitions of tests. Sometimes even some findings were cancelled; in other words, admission of errors was in order.

Just as the developmental cycle of Rickettsias, so also the complex building of other observations and experiments grew: the guinea-pigs had temperature when inoculated with the n-th lung passage (in which no germs were present, and the fever came from the boils near the anus of the guinea-pig, due to unskilful insertion of the thermometer). The virulence tests, in the skin of the rabbits, yielded the expected results according to Giroud, as skin tests, in incompetent hands, always do confirm whatever we wish to expect. The immunity tests of guinea-pigs which had allegedly passed through typhus infection, were positive, for, even if the second infection produced a temperature rise, one found the cover for it in the non-existent pneumonia which was constructed by the collective daydreaming just as were those Rickettsiae.

This collective illusion functioned for one-and-a-half years; it was formulated in a system which did not have more logical lacunae than an average scientific output. The epoch of 'discoveries' was followed by the epoch of 'routine', with established methods, with a specific acquired fund of experience and skill. And everything was in agreement among the members
of the collective, no less and no more than in true science. The records of experiments, the summaries of results, the suggested modifications of methods were sent to the world outside the camp to genuine German specialists, men well-known in the world of science, and returned with words of praise. The German boss did get a high decoration. So great is the persuasive power of a harmonious system, and so limited is the verifying value of testing the inner harmony of the system.

An interesting shock occurred only when rabbits’ lungs with typhus germs arrived from a genuine scientific institute. The preparations from these lungs showed what no description and no drawing can fully replace: the real material. But he would err who believed that a single direct contact with scientific reality would bring about the downfall of the entire edifice. Only some most extreme deviations from what was seen in the material that was received were abandoned. The collective failed even to admit in private that its entire construction was faulty; quite on the contrary, it created a synthesis of the old theory with the new facts. The members of the collective only became more careful and less naive. One can admit that a number of such shocks originating from a collective of real science would have ultimately put them on the right track to official science.

Most convincing was not the ‘truthfulness’ of the material obtained, but, in the first place, the respect enjoyed by the ‘true institute’. I am positive that if submitted anonymously the same material would have made no impression at all. It might have been considered completely pointless, nay, it might have been utterly disregarded. I saw symptoms which entitled me to such a conclusion.

Thus we faced the following situation. A closed collective of intelligent people, left completely to their own devices, have found, when working using the standard scientific apparatus, between the scientific view of a certain field of knowledge and the observed phenomena which undoubtedly did not belong to this field, a relation which (according to the members of the collective) entitled them to state that that view was the image of those phenomena.

Simplicitus. You do not describe anything out of the ordinary. We all know that it is always possible to err, to lose one’s way, and to stray. Lord knows how far. We know quite many such examples.

Sympatius. This is not a simple error, but a complex system of errors. What was at stake was not to establish one isolated fact (if such facts do exist at all) but the relations of numerous facts, i.e. that which we call the structure of a certain field, and which some people consider to be something
that is doubtless ascertainable intersubjectively, i.e. something with respect
to which one can always find a complete mutual understanding.

Simplicius. It is so, indeed. Two different structures can be similar in one
narrow field, but in every case the divergence will be discovered sooner or
later. In the case just examined, the researchers would undoubtedly find
sooner or later that the practical consequences of their erroneous views
differ from those expected on the basis of true views.

Sympatius. Your 'sooner or later' comprises the regressus ad infinitum.
How many structural details are to be established in order to ascertain the
consistency of an image with reality, or rather: how many details in two
structures ought to be compared with one another, in order to ascertain the
identity of both structures? Five, or five thousand? Each later detail which
had not been taken into account could be always the crucial one. We cannot
compare 'all' details, because the expression 'all details' has no sense with
regard to real problems.

Simplicius. In principle the more the better. But, in practice, a small
number usually suffices.

Sympatius. I am glad you give practical conditions, i.e. conditions which
are adhered to in scientific life. We pass thus from speculation to observation.
Now, in practice — as we learn from observation — there exists, for every
scientific worker or, better still, for every collective body of workers —
as these are collective matters — a characteristic moment at which the
worker or the collective body assume that no further verification is required.

The opinion becomes rounded, systematized, limited, in short it becomes
mature and obtains its form which is consistent with the thought-style
of the given collective. The collective body considers that any further ques-
tions are superfluous, simply indecent. Some things must not be asked
of the members of religious, political or scientific collectives. Did not you
yourself speak of the elimination of certain problems from science, because
they are meaningless? Yet they are meaningless only when we apply to
them the style of scientific thinking. The question of the Absolute, which
is meaningless for both of us, did, and will, have a deep meaning for many
people who live and die for it — just as we live for the sake of Progress.
Thus the problem of admitting that the relevant problems do not require
any further study is a problem of thought-style. At the given moment,
it is routine that enters the ring of the collective body, instead of turbulent
creative thought. "One does not need to look for anything new, verifica-
tion has been completed; any attempts at falsification would be against
good manners. Let us quietly enjoy the fruits of our work." All of this
can be observed in the progress of events in the camp collective mentioned above.

An error of this nature, or rather such a closed, harmonious system of errors, cannot occur at all in the case of an individual work, just as a developed discovery which yields a closed system of ideas is always the result of collective work. There was no individual author of the error; the error grew out of the collective atmosphere, out of the integration of individual actions and omissions, thoughts and reticences, of misunderstandings arising because the individual A had formulated the misunderstood thought of the individual B — a thought that was not entertained by anybody, but which was often of a crucial nature, as it followed the line of the collective mood. A mood that had produced a specific observational readiness which determined what is to be the object of the attention, and what can be disregarded. Finally, the view had filled the bowl of social interest, had fallen into a systematic framework, had created for itself an axiomatic foundation, was petrified in routine, and would have lasted for centuries — had the collective lasted for centuries and had no external influences made themselves felt. No automatic process whatever would be able to give rise to corrections, to an increased experience, to reflection.

Most important in our story is the fact that — as this became evident — the social mechanism of the origination of an error is the same as that of the origination of true knowledge, as examined in the source material and the history of science.¹ The history of the fundamental chemical discoveries, the history of the transformation of the phlogiston theory into the oxygen theory and therefore the history of the composition of water, serve as a good illustration. Similarly more recent discoveries in the field of pathology or biology demonstrate the collective character of the work of discovery and the style-character of a closed view which appears as an organic whole. Both in faulty and in true science it is the same collective forces that play the role of a motor, while the individual is the representative of certain social functions rather than a conscious source of action. In both false and true knowledge a view does not arise by a logical calculation of some elements, but by way of a complex process of stylization. There exists no observation that would not be forestalled by a directing and limiting readiness of thought.²

Simplicius. Do you want, following the example of the sophists, to convince me that there exists no difference between truth and illusion?

Sympathius. No, my dear friend, I am not as naive as that. What I want to do is to say that scientific results and views are basically determined exclusively as single historical events at successive development stages of the
scientific thought-style which is the outcome of the specific structure of the scientific thinking collective. Neither a Robinson Crusoe, nor a group of Robinsons, even if equipped with technical means, will glide automatically onto the tracks of science, if they are isolated from the scientific community. Even a partial isolation, brought about e.g. by political limitations, causes a partial difference between the results — and here lies the secret of the effect of the environment on science.

Let us, however, revert to our camp collective. Its thought-style was characterized, in the first place, by replacing the fundamental specialist knowledge (which was not available) and the experiments (in which one had no confidence) with speculative considerations, and by replacing the practical specialist experience (which was also not available) with so-called common sense.

When paraphrasing the well-known sentence of Gauss', one can say that the lack of specialist education in the empirical field can best be recognized by the limitless accuracy of logical inference. I have listened to consultations and discussions lasting for weeks on end, in which people tried to solve specialist problems by a speculative method, starting from a few textbook theorems which play the role of axioms, and from some data obtained from one's own experience — which were not linked together so as to form knowledge, but which were taught and commented upon — just as one expounds dreams or comments on the declarations of a diplomat.

Let us take the following example: in the experimental zoo an epizootic broke out among rabbits, caused, as I was able to establish, by the group D paratyphoid bacteria (according to Kauffmann). During the first day some rabbits died which had been inoculated the previous day with a vaccine from a broth suspension of heat-killed paratyphoid B bacteria (B. paratyphi Schottmüller). Vaccinations were conducted in order to weaken the rabbits which were to be used for the passaging of the typhus virus. During the following days the non-vaccinated rabbits also started dying.

This is the theory as developed by the collective: since, during the same period, cases of meat poisoning took place, caused by Bac. Gärtner (another group D paratyphoid germ), one has to admit that in the meat from which the vaccine broth had been prepared there was also the endotoxin Gärtner which, being resistant to heating (according to the book data) survived in spite of heating. The paratyphoid B group bacteria grown in this broth underwent transmutation.

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\text{endotoxin D + Bac. paratyphi B} = \text{Bac. paratyphi D.}
\]
PROBLEMS OF THE SCIENCE OF SCIENCE

It is clearly written in the textbook that the proper difference between *paratyphus* B and *paratyphus* D lies in the difference of the endotoxins (antigen O). Moreover, it is written elsewhere that the group I pneumococci can be converted to group II pneumococci by cultivating the former in a solution of an endotoxin characteristic of the group II. Hence such transmutations are feasible.

The group D bacteria obtained by transmutation are pathogenic for rabbits; apparently, being more immune to temperature, they had not been killed during the heating of the broth culture from which the vaccine was prepared. Hence the vaccinated rabbits were infected and, from them, the epizootic was transferred to other rabbits which began to die during the following days.

These are the elements of this theory:

(1) Axiom I: the difference between *paratyphus* B and *paratyphus* D lies exclusively in the endotoxin. (However, a specialist knows that, much as one uses the difference of endotoxins for diagnostic purposes, this difference is many-sided.) Axiom II: that transmutation modelled on pneumococci is a rule. (A specialist knows that it is an exception and that it cannot be compared with the transmutation of paratyphoid bacteria, since, in the case of pneumococci, it is the difference in the structure of the cell membrane that is important, while in the case of the paratyphoid bacteria the question concerns the difference of the cell-interior structure).

(2) ‘Common sense’ had dictated to the members of the collective body that, if, during the first day, only those rabbits died which had been vaccinated on the preceding day, then the relation between vaccination and infection is obvious. (The specialist knows from experience that, if only a few rabbits from the many dozens of the vaccinated ones had died, then it can be due to chance that only the vaccinated rabbits had died, but it can also be the result of the impairing effect of the vaccination itself, that the incubation period for the vaccinated animals was shorter than for the non-vaccinated ones which fell ill 24 hours later. At any rate a direct relationship between vaccination and infection is improbable under these conditions.) ‘Common sense’ dictated to the members of the collective body that simultaneous meat poisoning of humans and a death of rabbits injected with a broth made from this meat ought to be related to one another, the more so as the humans did suffer from group D enteritis Gärtner while rabbits died as a result of an infection with bacteria of the same group. (A specialist knows from experience that meat from carcasses of animals which had succumbed to paratyphoid is often used for making broth for bacterial cultures and that
this does not entail any consequences of this kind. On the other hand, there
exists a probability of bacteria D from the remnants of the infected meat
being spread by rats to the rabbits’ zoo, if we admit that the illness of humans
and the death of rabbits were caused by the same germ — which had not been
proved.)

(3) Speculative elements: the collective body applied a speculation which
established a causal relationship of a few hypothetical possibilities, each of
which was highly problematical, and obtained a complex theory (of course
even more problematical) to explain the commonplace phenomenon: an
epizootic in the rabbit zoo.

A specialist would not use events to explain such a frequent problem,
which — he knows from experience — can easily be inessential i.e. of a chance
nature, or others which take place only under entirely specific conditions. In
any case he would try to back up such a bold hypothesis with experiments:
to establish whether the broth from the meat of sick animals did contain a
sufficient amount of endotoxin, whether the aforementioned transmutation
can indeed take place (quite an improbable occurrence), etc. The technical
terms such as endotoxin, transmutation, resistance to heating, pathogenicity
of the germ, are to the specialist words which denote the results of certain
definite experiments and observations, or else which follow from certain
theories. To the layman they are concepts whose entire content is comprised
by the verbal definition of a textbook, since only the specialist knows that no
such definition tallies fully with the substance of these words. The layman
knows the rule, the specialist knows besides also the exceptions and the
possibility of further exceptions. The layman considers that the rule had been
dictated by God or some demigods, while the specialist knows that it was
drawn up by his colleagues. The word is therefore for a layman a full-valued
equivalent of the real subject, and its manipulation — provided it is done
according to the rules of logic — is for him the equivalent of an experiment.
Hence the use of verbal speculation and the characteristic precision.

I did often hear endless discussions on the subject of definitions; finally
one began to differentiate between filtering and filtration, cooling and
chilling etc., while such terms as ‘brain symptoms’, ‘infection’, ‘incubation
period’ did play the role of absolute beings in the discussion which, in the
given case do or do not exist, and not the role of the names of phenomena
which can appear more or less distinctly.

_Simplicius._ What are the final conclusions you wish to draw from your
deliberations?

_Sympatius._ The logical nature of the structure is not the touchstone of
science, since systematic error frequently yields views which are more logical. An inference from the fundamental elements or elementary sentences is not the touchstone of science, since there exist no such elements. What we will consider to be the basic element depends solely on our standpoint; similarly it depends on our standpoint which of two structures we have to consider to be identical. The consensus omnium is not the touchstone of science, for there is never a consensus omnium, but only the agreement of 'our collective', and this also depends on false cognition. Every thought-collective considers that people who do not belong to it are incompetent. Practical applicability is not a touchstone, for due to the harmony of illusions even a false view is applicable. The alchemists' gold allegedly did enrich many people, and even the cost of wars was paid for by alchemists' gold.

The only touchstone of science is in the specific features of scientific cognition: the historic singularity of their development, the structure of the relevant thought-collectives, the characteristic of the scientific thought-style. It is only by way of a comparative method, in the framework of general sociology of thinking that we can get acquainted with the features of scientific thinking.

The science of sciences is a separate science based on observation and experiment, on historical and sociological investigations. It forms a part of the science of thought-styles.

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NOTES AND REFERENCES


3 "Der Mangel an mathematischer Bildung gibt sich durch nichts so auffallend zu erkennen wie durch masslose Schärfe im Zahlenrechnen" (Gauss).
TO LOOK, TO SEE, TO KNOW [1947] *

LUDWIK FLECK

Many erroneous opinions are removed by the psychology of perception and the sociology of thinking.

I. IN ORDER TO SEE ONE HAS FIRST TO KNOW

Let us look from a short distance at Figure 1. What do we see? From the black background the picture of a gray, wrinkled surface stands out. Some places look like rough folds, others like densely arranged warts, one place

Fig. 1.

* Problemy, 1947.

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reminds us of the waves of a muddy liquid, others of clouds of smoke (perhaps because the picture in this border place is out of focus). We find a place which looks like a frizzly fur, yet this is no fur, for there are no hairs to be seen. Now what is it? Is it the skin of a toad under a magnifying glass or perhaps a part of the culture of the celebrated fungus to which we are indebted for penicillin? Or perhaps a close-up of the neck of an old mountaineer?

No, this is a perfect photograph of a cloud of the type known to meteorologists as cirro-cumulus. Let us now look again at this figure, but from afar. Once we know what it is and in what way one should look at it, we see immediately the enormous depth of the sky, and a large fluffy cloud whose variable structure, while unimportant in the details of limited places, in its entirety reminds us of a sheep’s fur.

In order to see one has to know what is essential and what is inessential; one must be able to distinguish the background from the image; one must know to what category does the object belong. Otherwise we look but do not see, we look intently at too many details without grasping the observed form as a definite entirety.

This happens not only under the artificial conditions of the experiment we have just carried out, but also in any, even the simplest and the most complex, perception. A passer-by who watches an event in the street; a man who looks at a work of art in a museum; a scientist who examines a natural phenomenon; a sociologist who studies, who follows the aspects of social life; a physician who observes a patient; a farmer in the field, a craftsman at the bench — we have, all of us, to learn how to see the more or less complex forms of our world. Very important is the circumstance that, as the readiness of perceiving some forms awakens, we lose the ability to perceive other forms. In the same museum, an artist sees something completely different from what is seen by a detective there on duty. It is impossible to see both these worlds simultaneously since the observations of an artist require a special atmosphere which disappears when we tune up so as to be ready to carry out a policeman’s observations; and vice versa. In a crowd, the observations of a physician are completely different from those of a woman of fashion. Thus, within the same set of elements one can perceive different forms.

Psychology teaches us that every perception is, in the first place, a seeing of some wholes, while the elements are only seen later. Sometimes these elements may remain unknown. We recognize at first sight a man of our acquaintance or a known flower, but often we are completely unable to give the distinguishing features accurately. We see all at once that somebody has a sad look, though we do not know which detail of his facial features changed. We see that the general appearance of a room has changed, but we do not know which items of furniture have been moved. Moreover, in spite
of many different details we can observe an identical form within a specific whole, a specific ‘entirety’; thus all Chinese people may seem to be identical to the eye of a European, although undoubtedly they have individual differences. The word ‘father’ when pronounced by the squeaky voice of a child and by the drunkard’s bass of a sailor may have not a single sound in common, but it is still the same word.

It is precisely such entireties, which thrust themselves upon sensory perception, and which are to a large extent independent of their constituent elements, that psychology calls ‘forms’, regardless of the sense which supplies them. Thus we can have visual forms, e.g. cross, letter, figure; or auditory, e.g. a certain tune, a word; or olfactory ones, e.g. the smell of grocers’ shops, or of railway stations.

The problem of seeing forms can be best analysed in the seeing of letters. The Roman letter A can have a highly varied form, i.e. despite the change of many details it still remains the letter A. We says that the form can be transposed, cf. Figures 2–8. Similarly, a melody can be recorded in various keys, even in such a way as to change all the tones, and yet the melody remains the same. Letter A consists in principle of two arms converging at the top and of one cross-piece. These are the fundamental features. The arms can be of equal or unequal length, they can be inclined more or less, can be straight or curved (side features) — but they must meet, or almost meet, at the top. Otherwise A turns into H (the competing form).

If the converging tendency of the arms is marked by their mutual inclination, a small gap (Figure 2) does not spoil the form: we complete it

![Fig. 2.](image)

automatically. Similarly unnecessary additions do not within certain limits spoil the form: we make no mention of them, and complete the form negatively. The cross-piece can be placed at different heights of the arms, but not at any of them: if it reaches the lowermost end of the arms, A becomes a triangle. If it reaches the top end, the cross-piece ceases to be a cross-piece,

![Fig. 3.](image)
since it does not pass across the arms; the form disappears, although no other known form arises. When the arms are curved lines, then the cross-piece for an A which widens upwards should lie high up near the top. When drawn near the bottom, it spoils the form, even as it does not produce any known form. In the case of an A which narrows upwards the cross-piece can be placed at a low level without any damage to the form.

![Fig. 4.](image)

Each form possesses not only positive forms but also negative ones, viz. a lack of certain fundamental features of the competing form: A must not have a cedilla at the bottom end of the right arm, as otherwise our eyes will see it as the (Polish) letter Ą. Even slight thickening in the letter l changes it to t or l. 0 is converted to 6 by a slight bend at the top.

![Fig. 5.](image)

The absence of this bend is a fundamental feature to which we lend our careful, though unconscious, attention. The form 'sleigh' may have different forms and quite many supplementary details, but it must not have wheels, for otherwise it becomes a form of wagon. A desert may have different kinds of appearance and many different details, but it must not have trees. Thus,
in order to be able to discern a form, one also has to know the competing forms. However, the limits of possible transpositions are determined not only by the features of competing forms; there exist likewise certain limits dictated by the characteristic style of the form itself, such as that convex A with a cross-piece at the bottom, or an A with the cross-piece at the top. In the letter E the middle line must not be longer than the top or bottom one (cf. Figure 7), in the letter B the bottom loop may be larger than the top one, but

\[ \text{EEE-BBB 333} \]

Fig. 7.

must not be smaller, etc. Numerous changes of the secondary features, when carried out together, likewise destroy the form, though each change by itself is harmless. Thus the transposition of the form has its specific laws, and one ought to have at least an intuition of these laws, if not clear knowledge of them.

Acquaintance with the form gives rise to disposition of its perception (perception readiness), the intensity of which varies from one human to another, depending, \textit{inter alia}, on the degree of education in the relevant field. If the form is surrounded by competing forms (context), the disposition is enhanced, and one recognizes the form more easily; the range of possible transpositions is greater, and we complete the gaps more easily (cf. Figure 8).

\[ \text{POSTAC GESTALT} \]

Fig. 8.

The context becomes a superior form which increases our readiness to see inferior forms; however it can result in such an excessive readiness that we complete non-existent details of the inferior forms, or even entire missing inferior forms. In such a case the superior form competes with the inferior one: we see words without seeing the letters. This is known to proof-readers who concentrate on watching the letters and only some features of words,
which enhance their readiness to see the letters. However, these are more complex problems as, e.g. it is more difficult to read a proof of texts in a foreign language, in which words do not form known forms, much as a proof-reader in principle does not attempt to fully recognize the words of the text, even in his mother tongue.

At any rate, in order to see, one has first to know a lot about the fundamental features of the form; everything else is inessential. Thus, what are the competing forms, what are the negative features of the form watched, what are the possibilities of transposition? In order to see a rare form, one has to know to which context it belongs.

Yet it is rather an odd thing that, once we have learned to see a form, we can — moreover, ‘we ought to’ — forget a large part of this knowledge. One has to know how, but there is no longer a need to know. The child, when learning the alphabet, strenuously acquires knowledge a large part of which the adult has to forget. We forget that the upper loop of the letter B must not be larger than the bottom one, but that the vice-versa rule is not obligatory; that the middle line of letter E must not be longer than the bottom line, etc. We develop our handwriting in various situations, by comparing with the handwriting of other people, and by avoiding misunderstandings and conflicts. We write, having forgotten the principles of calligraphy, we immediately recognize the letter in the entire scale of transposition possibilities without analysing the details, without up-to-date knowledge of the details. In the course of frequent use, from vigorous knowledge comes the know-how and the direct readiness to perceive the letter, which appears as soon as the situation produces a suitable stimulus. As a matter of fact, we see the form as a whole which can become the element of further superior forms, only when we forget, at least to a large extent, its elements and structure. If not, we do not see the forest for the trees, and syllables make it impossible for us to recognize words and sentences.

*To see, one has first to know,* and then to know how, and to forget part of the knowledge. One has to acquire a directed readiness to see.

II. WE LOOK WITH OUR OWN EYES, WE SEE WITH THE EYES OF A COLLECTIVE BODY

We walk around without seeing any points, lines, angles, light or shadows, from which we would have to arrange ‘what is this’ by synthesis or reasoning, but we see at once a house, a memorial in a square, a detachment of soldiers, a bookshop window, a group of children, a lady with a dog. All of them ready forms.

There exist more distinct forms, e.g. a house, a detachment of soldiers,
and the less distinct ones, e.g. 'lady with a dog'. Is this an entirety, a distinct
form? A certain connection between both elements of this pair already exists
in the existence of a leash itself or in the moving of the dog round its mistress,
but this connection is weak and, much as we had immediately spotted 'a
lady with the dog', and not 'lady' and 'dog' — this whole is yet so faintly
distinct as to cover its component elements considerably less than e.g. ‘the
detachment of soldiers’ in which we simply do not distinguish among the
separate soldiers. A huntsman with a dog would, to the initiated observer,
present a whole that is more clear-cut, since, as a 'set', it represents a certain
activity which is carried out in a body. We know a lot about this activity,
hence our readiness to see this form as a whole. A horseman on horseback,
especially a cavalry man in uniform or a jockey in a characteristic attire, for
this reason gives us a form which is even more distinct. If we see a horseless jockey, we are likely to feel that this is only a part of something, that something is missing. A horseman — this is a well-known form, which we have seen many times, and about which we have heard and read quite a lot, so that the readiness to see this form is very strong. It appears to be clear that the distinctness of a form, much as this form is seen by the eyes of a single person, arises in these cases from sources beyond the individual person; from the opinion of the general public, from the prevailing habit of thinking. A form is constructed not from 'objective physical elements' but from cultural and historical themes.

A pencil and a note, if they simply lie beside one another, do not form a whole: we see two elements separately (cf. Figure 11). However, a note in

![Fig. 11.](image)

a pocket format, with a pencil in a suitably mounted receptacle — this is a very distinct whole: a note-book. It has its name, traditions, sense — it is a form *populi gratia*.

Let us ponder something for a while: letters, figures, words — these are undoubtedly forms created by the collective body of men. But, take e.g., a house? 'House' is a very distinct form, with a long scale of possible transpositions, competing in our society with such forms as cottage, castle, villa, church, shed, etc. The main features of the form 'house' are: a plan of suitable size, with a front wall, windows and a gateway; the roof, previously indispensable, can be imperceptible today. The negative features: lack of towers, since we have seen a castle, lack of features characterizing a church or a villa, etc. It is obvious that only a man belonging to our society sees a 'house' i.e. recognizes this form within the entire scale of its possible
transpositions. A situation is possible today, in which the inhabitant of Warsaw will see a house, and the New Yorker will see ruins, a heap of rubble. There are situations in which the inhabitant of Warsaw sees avenues, and the country bumpkin will see a row of small, sharply cut small gardens, various paths and roads, a series of houses, some ruins, some kiosks and quite a traffic of carts, motor-cars and people — but where is that avenue?

A predominant part of our forms (though probably not all of them) were created by the environment, linguistic customs, opinion of the general public, tradition.

It is these factors that train us to produce a certain form: the collective body of men sanctions the isolation of certain entireties from the collection of certain elements. It creates the idea of a certain content and a certain range, and this concept is carried into effect, becomes flesh, a form having certain features and a certain range of transposition. Whoever is a member of the collective body, will see it. There used to be times when witches were seen, when they were recognized allegedly at once, possibly by the satanic expression of the eyes, by a devilish smile, provided they stopped dissembling for a while. This form was created by the then collective body. We, people of today, directly see a railroad station, a form that a primitive man would be unable to see. He would look at the mass of ironwork in tangled 'laths' fixed to the ground, at houses on wheels, at a hard-breathing monster exhaling fire and smoke, and he would probably see his own forms: the dragon, the devil, perhaps many other things, but not our good old railway.

We look with our own eyes, but we see with the eyes of the collective body, we see the forms whose sense and range of permissible transpositions is created by the collective body. We are inclined to complete them, both in the positive and negative sense, i.e. we do not see that some elements are missing, and we are blind to needless additions. Next we see superior forms, and we cease to see from what component forms they arise. We learn mutually to see forms of a certain kind (e.g., various meteorological phenomena, such as storm or calm); a directed readiness with specific features does arise; a common thinking style is developed, for example the seaman's style of thinking. Imitation, propaganda, mutual completion in collective actions (hence the need for communication), veneration of common ideals — these reinforce and specify this style. If, due to a historic coincidence, two collective bodies come together after having been isolated from each other for a long time, their members seem to the other set to be madmen or liars: 'how is it possible to ignore races?' — 'how can one not see classes?' — 'How is it possible not to see the bad and the good spirits who show themselves at every
Fig. 12. The primitive man would see a hard-breathing dragon.
step?—‘How can one disregard the laws of nature which manifest themselves in every phenomenon?’

What is our behaviour when for the first time we face an object which is unknown to us? Just as a child does when watching a smeared ink-blot. He sees in it a wing of a bird, leaves of a tree, a flower, two horses grown together, an angel, in other words forms known from other sources. These forms mutually displace themselves, they disappear, make room for one another, vary, oscillate.

This is a very interesting problem, and it is possible to follow exactly the formation of specific forms, e.g. by checking the drawings and descriptions of the first anatomists or the first bacteriologists. Figure 13 presents an anatomical figure from the 15th century. Its author was unable to see the characteristic form produced by the intestinal system in the abdominal cavity, which is today known to everyone with a secondary education. He saw not these characteristic twists, but only the ‘twisting in general’, and into the oscillating blob he inserted the forms which came most easily to his head: 5 helices. He without any doubt saw them immediately in the abdomen. The ribs, the thorax he did not see as 12 characteristically bent lines, but as ‘ribbing in general’, and he drew 17 parallel lines because he saw this ‘ribbing’
form, and not the 12 ribs. In the earliest descriptions of organs, e.g. bones, we find a wealth of comparisons with different known forms: bird's bill, plough, sword, stirrup, letter S etc. These comparisons were preserved in the names. coracoid (raven-like) process, Siebbein [sieve bone] (ethmoidal bone), vomer (Pflugscharbein = ploughshare) etc. Often more than one comparison was stressed, and the controversy lasted quite a time as to which form best corresponds to the shape of the given organ. During this controversy, in a really collective work, a new form was formed, i.e. a discovery was made. The anatomists learn to see the characteristic organs exactly in the same way as children learn to see letters.

In the 19th century an entire new world was created under the microscope. As far as the form of the single cells, the protozoa, is concerned, the comparison is easy, since they recall simple geometrical forms: rods, spheres, spirals. However, if we have to describe group forms, a specific system evolving from the propagation of bacteria, the problem becomes much more difficult, since one had to learn how to see forms very different from those met with in everyday life. We can keep track from the outset, as the pictures oscillated, as observers saw various fantastic forms of everyday life mutually displacing one another, as the picture established itself, as the number of comparisons decreased almost yearly, from one author to another, and as, during discussions and mutual correctives, there arose a new form, so distinct that it itself became a pattern sanctioned by the collective body, a pattern that was later made use of in seeing the successively appearing new forms. The role of the group life, i.e. of the collective body, is clear: from the store of traditional, generally admitted forms, one derives in the first place those mutually displacing resemblances; and then the collective life produces among these oscillating possibilities a novel prescribed form, which is then fixed and pressed upon the individual person. The collective experience and custom determine which feature is fundamental and what can be variable, and how far this variability can extend. The sociological forces give rise to that above-mentioned readiness of perception. Within every member of the society a slightly different form is instilled, and the range of these dissimilarities determines the entire extent of possible transpositions.

If our seeing had not been of the form-perceiving type, who knows whether abstract concepts would have arisen, or whether generalization and, in general, knowledge would have been possible.
Yet not everybody admits that virtually every seeing is of the form-perceiving type and that virtually every form is conditioned by collective life and by the collective thought-style. Many representatives of sciences still using the thought-style of classical physics, affirm that it is possible to conduct a so-called ‘objective observation’ of an isolated elementary fact, independent of the psychologically or sociologically conditioned readiness to see the less or more ‘subjective forms’. That, with the aid of suitable apparatus, it is possible to measure the phenomena of the ‘external world’ completely regardless of our thinking style. That it is possible to describe an observed figure without comparing it with forms chosen from the store-house of knowledge, simply by examining the coordinates of the successive points in its outline in an optional conventional system. An American sees a heap of rubble, a Varsovian – a house. Now a physicist will determine by means of apparatus the position and size of every brick, and will reproduce a whole which will be neither a ‘house form’ nor ‘a form of a rubble heap’, but an objective description of observations, a map of objects which are independent of ourselves.

In such opinions of some physicists one finds a wealth of illusions and misunderstandings characteristics of their style of thinking.

In the first place it is impossible to isolate the object of observation from the thought-style. Our physicist (Figure 15) stands with his apparatus in front of a house – a heap of rubble. In what place does he begin? Not in any defined place. Single bricks lie around, their fragments, their waste, dust from the mortar, sand. All this extends even beyond the tree which grows in the pavement in front. There is no sharp limit; the physicist is to establish it artificially; he has also to decide whether this tree and the crow which sits on the rubble, do or do not belong to what he has to measure objectively.

It is impossible to isolate the object of observation without assuming in advance that it possesses certain features. This is admitted by some modern physicists, e.g. Bohr: “Even the observation concept itself contains an option since it depends in principle on what objects are reckoned among the observed system”. However, they do not see that this apparent arbitrariness is a necessity imposed by a specific thought-style: the physicist stands in front of this heap, like a child in front of an ink-blot, and sees in advance in it the angles and flowers of his scientific style. It is impossible to isolate any element, regardless of the traditional thought-style created by the society to which he belongs. The point is that the process of analysing and isolating
the elements does not differ in anyway from the process of producing new forms from the decomposed old parts. In substance it means that the number of negative features increases, while that of the positive features decreases as one passes from earlier forms to the new ones.

Next the physicist takes the stones, bricks, fragments, and weighs one after another, in order to classify them objectively and to write corresponding figures in the corresponding places of the map. This appears to be a simple action and is clear almost to all the members of European culture. However, one has to realize that, in its current popular-physical meaning, weight is a relatively young concept. True, it was known to a certain extent in antiquity (Aristotle distinguished between heavy and light bodies), but even in the 18th and in the 19th century many educated people did affirm that, e.g., a corpse is considerably heavier than the man when he was still alive. Why, it is because it is heavier to lift it, because it 'drops from the hands'. Similarly, a hungry man is heavier than a satiated one (provided not excessively so!), because it is 'heavy' to move. That sorrow makes a man heavier, and gaiety — lighter in weight. In the figurative uses of the word 'heavy' as used nowadays
(a heavy fate, a heavy task, a heavy road) one still finds this 'appropriation' of weight. When a physicist uses a balance, this means that the collective body of physicists had isolated, in the course of history, certain elements from among the set of phenomena, impressions, concepts and old opinions, and has developed them up to a consistent whole, while rejecting the remainder.

The use of an apparatus is always the expression of applying a certain developed thought-style. It would have been difficult to convince a man who does not use, at least partly, a physical style of thinking that weighing on our balances is an operation which has a certain connection with the 'weight' or that it does not influence the 'weight'. The scientific apparatus directs thinking towards the path of the scientific style of thinking: it produces a readiness to see certain forms, while removing at the same time the possibility of seeing others. There exists no analogy between carrying a heavy stone and carrying a heavy sorrow, and it cannot exist for a physicist or, strictly speaking, for a physical style of thinking. Similarly there exists, for a physicist, no analogy between high tones and yellow color and between low tones and blue color, much as psychology ascertains that almost everybody feels this directly. The point is that physics does not consist in analysing all sensory statements which are binding for normal people, and in constructing the world from the experienced basic general elements; rather it is a system given by the historic development of the thought-style of the complete collective body which maintains contact between its members over centuries. Since physicists also participate in other aspects of the collective life of their epochs, no wonder that their 'objective' opinions contain, in each epoch, features which are characteristic of the intellectual life of that epoch, as stressed with surprise by Schrödinger. Today, statistics and the calculus of probability dominate physics, biology, immunology, economy, sociology etc., such being the intellectual fashion. Not long ago, all sciences and technology were under the sign of [strict determinist] mechanics.

Let us revert to our physicist and our heap of rubble. He would have to carry out an infinite number of measurements, as he would have to measure separately each dust particle and each point. Of course this is impossible. On his map the physicist will denote only the 'main' points, and he will fill in, making use of some general principle. Now how is one to know which points are the main ones and what principle is to be used? Again solely on the basis of a definite thought-style and on the basis of the entire store of knowledge at our disposal at the present moment. For every phenomenon we would need, strictly speaking, an infinite number of measurements, provided we did not assume in advance the rule of interpolating the values
between the measurements: the general opinion handed down by tradition takes part in every new observation. In this way old discoveries determine the current result of observations and also condition future discoveries. The sequence of discoveries and errors clearly influences the contents of science.

"Physics refers rather to the relation between the read-off positions of pointers than to the positions themselves", writes Eddington; however, even he does not see that these relations are dictated by the mental compulsion imposed by the collective body and that they are examinable, (which might be a cause of religious mystics, i.e. of non-examinable factors).

Finally, to carry out an ideal measurement by means of instruments the instrument must be completely isolated from outside influence, this task being also completely infeasible. Of necessity we must decide what degree of isolation is to be considered sufficient. The remaining divergences of multiple measurements are to be levelled out by means of the calculus of probability. How many single measurements are to be carried out? A theorist would say: as many as possible. Now this is unreal, as no instrument will endure an infinite number of measurements, and one has to take into account that, when the number of measurements exceeds a certain maximum, the instrument will be damaged, i.e. it will yield false values. And then no phenomenon stands still, as at a photographer’s, waiting for a long time without any change. Thus the number of measurements must be limited, and we do this again on the basis of custom, the entire store of individual and collective knowledge, etc. Which measurements should be considered as successful, i.e. what scatter is admissible? Shall we require that the difference between two measured values exceed the probable error by the factor of one, two or three? In order to answer these questions we have to know whether the instrument is equal to the task, and what is the nature of the problem itself and the aim of the measurement. Thus, during each single observation, we have the joint action of the entire store of the knowledge of the collective body, and its customs. The constructor of the apparatus, the supplier of materials from which the apparatus will be made, are present at each measurement, just as the creators of concepts are at every idea of the measurement.

In any case an observation with the aid of a scientific apparatus is not tantamount to the co-ordination of some number which is independent of us, with a certain constant independent element. It is rather the construction of a sentence of the following type: “under the conditions of the measurement the probability that the weight of the body C does not exceed the limits 5.32587–5.32589 g amounts to about 95%”. What have we found? A complex construction, an entire theory expressing the relation between a
Fig. 16. The tower of Babel according to a 16th-century drawing. Characteristically false proportions: the tower height amounts to only 3–4 heights of a man, i.e. about 7 m, but it already touches the sun. The drawing has two scales: the smaller scale of the tower measurements, with which are roughly co-ordinated the sizes of windows, stairs and mouldings, and a greater scale of the dimension of humans, with which are co-ordinated the tools, cable and bucket with clay. The author saw and represented by means of forms: the ‘tower’ form with its components separately, and separately the form ‘man’ with what belongs to it. The link is the gateway whose dimension is drawn in a medium scale, too little for the man, too large for the tower.

series of numbers, a set of conditions partly depending on ourselves, a state of our knowledge at the given moment and a certain element isolated by us.  
*From this construction one cannot deduce anything about something which*
is independent of us. The objectivity of scientific observations consists merely in relating them to the entire store of knowledge, experience and the traditional mental customs of the scientific collective: the outcome is independent of the passing moods of the individual and of his readiness which is given by the collective of everyday life, but, instead of forms conditioned by the style of everyday thinking, science creates at best constructions conditioned by the differing scientific thought-style. Before making such constructions, it creates specific forms of scientific perception, such as a certain species in zoology, some disease in pathology, a certain force of yesterday's physics, etc.

Now since it develops and deepens its constructions, by building more and more general superior ones, it thus increases dependence on the thought-style of the scientific collective. Finally one will reach the most general features of the physical style: mathematics. Hence the idealism of many physicists and of Jeans: God – a mathematician.

It is not only in atomic physics that the limits between the so-called subject and the so-called object fade, according to Bohr. The Procrustean bed upon which the physicist strains facts so that we are unable to decide whether a scientific fact has been ‘developed’ or ‘produced’ by science, is a universal institution. Everywhere, when we have pushed the analysis sufficiently far, we shall reach the elements of knowledge which, to stubborn metaphysicians, will appear to be the forms of a priori thinking with regard to observation, or intuition, and which actually result from the combined nature of cognition and may be investigated by methods of the sociology of thinking.

Thus also scientific observation — of the form-perceiving or construction type — depends on the collective thought-style.

In everyday life and in science, between the subject and the object of the old science of cognition, the collective as a third factor does intrude itself.

IV. THE COLLECTIVE BODY AS THE ‘TERTIUM’

Along with the imposed comprehensive forms, with widespread general opinion relating to a certain field, the generally adopted analysis of elements, with technology, art and science, with the everyday custom, legend, religion, even the language used — the collective body intrudes into the process of looking and seeing, thinking and cognition. If every observation, the ordinary everyday one, or the most accurate scientific one, is a modelling, the pattern is supplied by the collective body. And there is no other possibility.
This is no scepticism. Otherwise any communication and community life would be probably impossible.

The process of cognition is not a two-term one, as individualist opinion proclaims: it does not occur solely between an abstract ‘subject’ and an equally absolute ‘object’. The collective is incorporated into this process as the third member, and there exists no way to exclude any of these three members from the process of cognition: every cognition is a process between an individual, his thought-style which results from affiliation to a social group, and the object. There is no use in discussing the subject of cognition, without regard to the thought-style, or the object independent of both, just as one cannot discuss a collective body which exists without individuals. The sentence ‘John recognizes the object C’ is incomplete, just as the sentence ‘this book is larger’. They have to be completed: ‘John, as a participant of culture K, or John, on the basis of the style S, recognizes the object C’, ‘this book is larger than that one’.

One can easily see that, if a live exchange of thoughts is conducted among a group of men, there soon arises a special collective mood, as a result of which people utter sentences they would not have uttered in other groups.

If such a collective body lasts a relatively long time, a distinct social structure arises: some individuals begin to lead, while other become subordinated. Emulation arises, a desire to imitate, admiration, disdain, likes and dislikes. Parties are formed, some sentences are stressed, because pronounced by Mr. N, others are disregarded, because uttered by Mr. M. Principles of the basis of the exchange of thoughts and behaviour are formed, ideology is produced. Unfinished sentences are completed, and unclear sentences are stated accordingly. Finally we face a system of opinions whose authorship does not lie in any individual: it is the collective body that is its author. A highly intense common mood can result even in collective suggestions, which are quite often known and observed.

If the collective is quite sizeable and lasts for many years in a uniform mood, it will raise its young participants (even when they are not collected in one place) in the collective discipline; it will produce solidarity and the feeling of mutual confidence of its members. They will see the same characteristic overall forms, they will believe in the dogmas of the collective philosophy of life, they will think using solely the categories of a certain style. For, “what thinks in a man, it is by no means himself, but his social community” (Gumplowicz). The behaviour of the members, their deeds and their entire life attitude will result from the collective compulsion: the style will be carried into effect externally in the common language and in common institutions, in similar attire, houses, tools, etc.
Fig. 17. A lady shacklesthe hands of her lover (a 14th-century drawing). The horse has the size of a dog, the castle is like a sentry box. The artist has shown a series of separate forms: horse, lady, castle, a head in the window — just as if he had seen this scene.

Levy-Bruhl, starting from his investigations on the thinking of primitive nations, affirms that the research on the "collective conceptions of these peoples and the relations between them cast light on the generation of our categories and logical principles. Using this path one can reach a positive theory of cognition, based on a comparative method." Unfortunately, this author believes at the same time in the objective features of objects, upon which the attention of the observer is automatically directed if the mystical elements of thinking weaken — and thus he departs from his own theory.

There exist stable thought-collectives with a centuries-long tradition,
e.g. collective bodies of people occupied with a certain science or a certain philosophy, professing a certain religion, working in certain professions. Some of them act on their members more, others less strongly, and that is why sometimes a rich and consistent style is formed, and at other times merely slender rudiments.

If such a rich and developed thought-style is found, communication between members of the collective with people outside it becomes difficult, sometimes impossible, at least as far as some problems are concerned. A naturalist would try in vain to communicate with a theosophist, mystic or cabalyst: even by using the same words they speak about something different, for their words have a different meaning, their concepts have a different style-hue, their inferences make use of other relations, the starting-point and the aim of their thinking differ from one another. Each sentence which is heard by the member of another collective body will be more or less changed into the new style, hence the speaker says one thing, and the auditor understands something different: in the international peregrination, thought is subjected to deformation, and that is why direct communication between members of different thinking collectives is impossible.

Something different is found in the intra-collective peregrination. Each thought-collective comprises two distinct classes of members: the elite and the mass. This might be priests and the faithful, the initiated adepts and ordinary members, experts and laymen, master workers and journeymen — this feature of social structure is always noted. The mass looks up to the élite with a specific trust, the élite depends on the mass which is the carrier of the overpowering ‘opinion of the general public’. Each intra-collective migration of the thought strengthens it: the layman accepts the opinion of experts as a revelation which must not be questioned, thus the apodictic nature of the utterance increases. When the expert hears that the layman uses his, the expert’s, thought, he accepts it as a confirmation, a vox Dei. Similarly the laymen among themselves or the experts among themselves are confirmed in the thought-style if they find that ‘my colleague is of the same opinion’. Thus, in the intra-collective migration each thought becomes strengthened ipso sociologico facto. Hence its rule over the members of the collective, who believe in the suprahuman origin of the given way of thinking, the only good one. Hence the contempt for men who think differently, i.e. wrongly.

Sociology of thinking is a young science unappreciated by scientists. It is much better known and misused by politicians, and the whole of mankind loses the more by it.
The scientists, most frequently individualists, do not want to see the collective nature of thinking. What would remain of their renowned genius? Hence the eternal senseless controversy about 'materialism' and 'idealism', *a priorism* and empiricism, the untranslatability of opinions from distant cultural media, and the impossibility of understanding ancient epochs. Hence the legends of mysterious intuition and flight to metaphysics or mysticism.

Only the sociology of thinking can explain to us the problem of communication, and lack of it, among humans. Perhaps it would be fruitful to examine such important phenomena as propaganda, actions of authority, the role of imitation, intellectual co-operation and competition, ways and means of the diffusion of opinions. Sociology examines the problem of introducing into a certain style of thinking the problems of the collectives as they are delimited and internally organized. The problem of the characteristic structure of various collectives, e.g. that of scientific thought, of everyday life, of the psychology of professions, classes or other groups. The psychology of development stages of society, e.g. the psychology of primitive peoples, psychology of revolution, of the period of stagnation and reactionism. The psychology and philosophy of life in past epochs.

The advantages of the sociology of thinking thus understood are clear: it will enable us to rationally direct the intellectual life of societies. It will find a way to immunize masses against absolute propaganda. As a comparative science it will counteract fanaticism, this enemy No. 1 of mankind.

Instead of the philosophy of life which undergoes constant changes and is dependent on moment and place, it will provide the idea of the mechanism of the formation of views. Instead of what separates, it will point to that which will be common to all, and which brings them closer together.
CRISIS IN SCIENCE

Discussion on 'Science and Human Welfare', Science, July 8th, 1960

LUDWIK FLECK*

There is no doubt that science is becoming a servant of politics and industry, to the great detriment of its cultural mission. In almost all countries throughout the world politicians and industrialists dispose of scientists, often decide on their work and sometimes even on their beliefs and convictions. This happens not only because some modern scientific activities require large resources. A more dangerous factor is the growing opportunism of many, mainly young, scientists to whom Science is only a modern way to a good career.

It is my conviction that the reason for this deplorable attitude of many scientists lies in the gap between the obsolete, but still generally held, official opinion on the nature of Science, and the practical, though limited, insight into Science possessed by the ordinary scientific worker. 'Truth', 'objectivity' — are hallowed ideals. But they are for the ordinary modern scientist in their classical meaning too naïve, and in the practical sense of these words as encountered in everyday scientific life, too complicated and devoid of any greatness.

The ordinary scientist of the day finds that 'scientific truth' is a complex mental construction, inseparably connected with the investigative techniques, statistical interpretations and manifold conventions. He knows that it may often be expressed only in a specific jargon, and be intelligible only after a prolonged professional training. In his opinion, 'scientific truth' depends on conjuncture, i.e., on the scientific opportunity, on the environment and on the personal influence of the author. It ought to be suitable for incorporation into the existing system of science, and finally of course, it ought to find approval. For the only proof of the value is in the success . . . .

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How can such a guide-made, and indeed temporal truth (found or made by X. Y., but actually by his assistants, for X. Y. himself is always at congresses) be united with the eternal Laws of Nature (as preached to good children)? However, the best policy is not to ask too many questions and to keep on good terms with those in power . . . .

Such is the genesis of the opportunism mentioned above.

I agree, of course, with Harold K. Schilling, the author of the paper ‘A Human Enterprise’ (Science, June 6th, 1958)² that there is “a need for more adequate models” of science than the customary “stereotype that bears but little resemblance to science as it is known intimately by those who live it from day to day”. “If, in planning for the future, we are to project for science a truly significant function in public affairs, we must base our thinking about how it should operate in the future upon a model that depicts as accurately and inclusively as possible how it does in fact operate now”.

Now, such a new model is available under the present circumstances almost spontaneously, and Schilling is very near to it, at the present time, in the era of team cooperation, of papers published by several co-authors, of vast numbers of Journals, Reviews, Conferences, Symposia, Committees, Councils, Societies and Congresses — the communal nature of scientific cognition becomes obvious. No more can cognition be comprehended as a function of two components only, as a relation between the individual subject and the object. Every cognition is a social act, not only when it actually requires cooperation, because it is always based on knowledge and skill handed down from many others. It is social, for during every lasting exchange of thoughts there appear and grow ideas and standards which are not associated with any individual author. A communal mode of thinking develops which binds all participants, and certainly determines every act of cognition. Therefore, cognition must be considered as a function of three components: it is a relation between the individual subject, the certain object and the given community of thinking (Denkkollektiv) within which the subject acts; it works only when a certain style of thinking (Denkstil), originating in the given community is used.

In the excellent book by R. J. Dubos Louis Pasteur (p. 120 et seq.)³ we read how the “Zeitgeist, i.e., the scientific and philosophical temper of time” obliged such scientists as Berzelius, Wöhler, Liebig, Helmholtz and Claude Bernard to reject Pasteur’s theory of the role of yeast in fermentation: “to appeal to a living agent as the cause of a chemical reaction appeared to be a backward step”. Experimental proofs against this theory have been demonstrated (Louis Thénard). Poignant jokes against Pasteur circulated. Liebig,
blinded by his preconceived ideas, was unable to perceive some easily demonstrable and quite distinct phenomena supporting Pasteur's claims.

It would, however, be a mistake to assume that the style of thinking, and the leading general ideas or images (Gestalten) derived from this style, are always rather a hindrance in the search for truth and a source of error. The whole of modern knowledge of infection and infectious diseases originates in very ancient beliefs in an analogy between putrefaction and disease, and in small 'animalcules' as a cause of both. The idea may be found in Greece and Rome (Hippocrates, M. T. Varro), during the Renaissance (Fracastorius, 1546), in the 17th century (Kircher, 1658; Leeuwenhoek, ab. 1680), in Pasteur's first papers (1866, Études sur le vin, ses maladies, causes qui les provoquent). All these 'ingenious intuitions' which existed before any empirical proof, and stemmed from an old pre-scientific Denkstil, acted throughout the ages as a propelling force for a host of discoveries. It is doubtful whether our knowledge of infectious diseases would have made such progress without these 'intuitions'.

These ancient ideas gave birth to modern microbiology, although the analogy between putrefaction and disease died in the childhood.

Many other so-called visions and intuitions originated in similar ancient 'leading pre-ideas', e.g., the Copernican theory, the atom theory, the theory of elements. They were first neither true nor false, because they were unclear in the original, ancient shape. But they have represented the third component in the process of cognition and have developed into important scientific theories.

In the book, Entstehung und Entwicklung einer wissenschaftlichen Tat- sache, Einführung in die Lehre von Denkstil und Denkkollektiv I wrote:

Every epistemological theory is trivial that does not take this sociological dependence of all cognition into account in a fundamental and detailed manner. But those who consider social dependence a necessary evil and an unfortunate human inadequacy which ought to be overcome fail to realize that without social conditioning no cognition is even possible. (loc. cit. p. 50)²

A single, really isolated human being would be condemned to mental sterility.

I have tried to demonstrate how Denkstil determines not only the development of complex ideas, problems and standards, but also the very contents and the limitations of an observation. The history of anatomy supplied the material. The style of thinking has been defined as the communal tendency to a selective perception and to corresponding mental and practical utilization of the perceived. I have tried to analyze the general structure and composition
of the Denkkollektiv (Center, Periphery; Elite, Public; Authority, Believers) and its action on the development of the communal mood leading to a communal style of thinking. Every mental intercourse within the community (the intra-communal exchange) strengthens its ideas and endows them with the features of objective reality. Every communication beyond the community (the inter-communal exchange) alters the sense of notions, gives them a more or less new meaning, and can thus be a source of new ideas.

In this way the three components of the act of cognition are inseparably connected. Between the subject and the object there exists a third thing, the community. It is creative like the subject, refractory like the object, and dangerous like an elemental power.

Many similar statements about science and society may be found in M. Polanyi’s works. However, his ultimate inferences are quite different.

“The experience of D. C. Miller (ether drift) demonstrates quite plainly the hollowness of the assertion that science is simply based on experiments which anybody can repeat at will”.6

“Science will appear as a vast system of beliefs, deeply rooted in our history and cultivated to-day by a specially organized part of our society”.6

“Human thought grows only within language and since language can exist only in a society, all thought is rooted in society”.7

“The members (of a school) are separated for the time being by a logical gap from those outside it. They think differently, speak a different language, live in a different world”.6 No doubt, in this quotation Polanyi describes the effect of differences in style.

Thus scientists become more and more aware that “science is communal” (to use Schilling’s phrase). The same is true of all kinds of systems of cognition, be they non-scientific superstitions, political convictions or other points of view. This is expressed in a general way by the statement of the three-component nature of cognition.

What is the advantage of the three-component model?

First, the demoralizing gap between theory and practice in scientific life vanishes. There will be less opportunity for hypocrisy. New ideals arise: the relation between science and the humanities becomes closer and science becomes more human. Therefore, loyalty to mankind and loyalty to the scientific community will increase among scientists. Scientific conventions, social customs of the scientific community become explainable. The scientists will become more modest, recognizing the limited role of the individual.

Sociology of thinking should be developed as a fundamental science equal in its value to mathematics. The problem of organization, planning, teaching,
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popularization should be based on this new discipline. Comparative studies on styles will make the students more tolerant towards strange styles, and prepare them for co-existence; proponents of different styles can appreciate each other, even work together to some degree without mutual understanding, if they know that a different mode of thinking, and not ill will, is the cause of differences.

The most profound problem of the present time, the relation of the individual to the community, appears in a new light. The communal mood of the Denkkollektiv, standing for the third component in each cognition, may have either of two effects: it may blind or it may make clear-sighted. Working evenly for a long time, it creates science, art, technology. When it suddenly bursts out — it causes riots and revolutions. Every politician and businessman knows that propaganda, i.e., rousing of the desired communal mood, is fundamental for every communal action. The scientists ignore — at least officially — this matter and become victims of it. An open-eyed attitude to propaganda will make the subject resistant to its abuse: when every school child learns that any folly, no matter how big, may be made credible by proper propaganda — a critical resistance to propaganda will rise.

The history of science, and of thought generally, considered as an evolution of communal styles of thinking, and based on structural changes in the respective communities, ceases to be a collection of ridiculous anecdotes and sentimental apotheoses. The genesis of ideas becomes explicable. The historical evolution of some fundamental concepts like 'compound', 'element', 'reality', 'cause', 'existence', 'individual', indicates that their present stage needs by no means be the final one. The realization of this will help scientists to be bolder in their creative conceptions. The sterile problem of idealism and materialism will vanish.

Scientific truth will turn from something stiff and stationary into dynamic, developing, creative human truth.

NOTES AND REFERENCES

1 "A scientist must not only have the right ideas, do the right experiments, and give birth to a paper. He also must build a coherent doctrinal corpus and force it into reviews and textbooks." (A. Lwof, 'Factors Influencing the Evolution of Viral Diseases', Bact. Reviews 23 (1959), 114.)


3 R. J. Dubos: Louis Pasteur, Boston, 1950, p. 120 et seq.

4 "We find as early as 1683 that the ingenious Fred. Slave, M.D., F.R.S. commenting
upon a murren in Switzerland which carried off many cattle, says: "I wish Mr. Leeuwenhoek had been present at the dissections of these infected Animals, I am persuaded he would have discovered some strange Insect or other in them". (C. Dobell, *Antony van Leeuwenhoek*, London, 1932, p. 230). Real proofs of microorganisms as causes of infectious diseases were presented only two hundred years later.


PART III

ON LUDWIK FLECK'S THEORY OF KNOWLEDGE AND SCIENCE
NATHAN ROTENSTREICH

THE PROTO-IDEAS AND THEIR AFTERMATH

A. ELEMENTS

1. Milieu

The purpose of the present exploration is not to deal with the details of Fleck's analysis of the scientific fact and its conceptual components. The purpose is rather to point to some trend in modern philosophy — as a matter of fact formulated at the time of Fleck's writing of this major book — without assuming that Fleck was aware of those trends and their affinity with his own 'style of thinking'. Hence we can say that we are concerned with the whole notion of 'pre-ideas' (Prä-ideen) as formulated in several major philosophical presentations, or to take advantage of a historical expression — we are interested in the 'climate of opinion' in which or against which Fleck's theory was formulated. To be sure, when Whitehead uses the term 'climate of opinion' he points to the understanding of the antecedents of a certain world-view. We are more concerned with the contemporary points of view than with that which preceded Fleck. One could say that we are interested in the contemporary milieu, in the philosophical sense, of Fleck's position and its major issues.

Having used here the notion of milieu we are reminded of a lecture given in 1932 by Schrödinger entitled 'Is the Science of Nature Conditioned by Milieu?'. Yet the concept of milieu as used in that lecture points rather to certain methods prevailing in a given span of time, as for instance the statistical method applied in one field of science and transferred from that field to another one, with the emphasis that the essence of statistics is "a wise giving up of the knowledge of details". Schrödinger points also to the prevailing trend of modern physics represented by the theory of relativity and the theory of quantum, which he describes as the trend towards turning down accepted views. Since our concern is more with the philosophical background than with the trends made explicit in science, we shall pursue a different line in our exploration of the milieu of Fleck's position.

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2. Style of Thinking

The issues dealt with will be mainly the notion of the style of thinking (Denkstil) as a pre-supposition of the formulation of the scientific theory, the notion of the fact and the scientific fact and eventually the reference to reality as it appears in Fleck's work. The last point we shall be making is more by way of anticipation than by way of identifying the climate of opinion — in the contemporary sense of that term.

Speaking in the first place about the style of thinking, let us recall that Fleck does not refer to a personal attitude of a scientist or of a thinker, an attitude which he describes as related to a moment and guided by a personal bias. He wants to detach the style of thinking from the personal aspects which as a matter of fact are endowed with feelings; hence he refers to what he describes as a collective attitude. When we are concerned with exploration of pre-suppositions, we can succeed only through the exploration of the style of thinking. This points to the historical dimension of the style of thinking which, because of its roots in the diachronic dimension, is even described as a compulsion (Zwang) of thinking in accordance with the style. The style of thinking is a guided perception; as every style this too is composed of a certain attitude on the one hand and a certain performance or realisation of that attitude on the other. It is a guided perceiving accompanied by an elaboration which is imbued with an approach grounded in thought and carrying a thematic character applied to that which is perceived. Though there exists the grounding of a style of thinking in the historical development, there are indeed also mutations of the style of thinking. The comprehension within a broad style of thinking makes it impossible to draw a clear line of distinction between that which is concrete and that which is abstract. What we do find is that a certain principle of thinking has to be preferred, since it comprises more particulars and perceives more conjunctions which are conforming to certain compulsion, more to than a different principle of thinking. Hence, the breadth of application of a principle to details becomes one of the criteria for our selection of that principle or of the style. Probably that breadth finds its corresponding supplement in the breadth of the human being, involved in the principle of thinking, though the first applies to that which is perceived while the second has a sociological meaning describe as "intracollective thinking communication". At this point, Fleck alludes to the affinity between the general structure of the collective of thinking with what is called the world of fashion — which is again a kind of paradigmatic approach independent of this or that content of that which is "modern".
The fact that particular expressions of thinking are grounded in a style leads to the conclusion that there is no *generatio spontanea* of concepts and positively that concepts are, as it were, determined by their ancestors. A theory of knowledge lacking the historical and comparative explorations is an empty play of words. One relativistic conclusion is drawn from that involvement, namely that probably there do not exist total mistakes (as there do not exist total truths). Since we are enclosed, as it were, within styles, each formulation carries with itself already half of the solution of the problems dealt with. As a matter of fact the enclosed systems exhibit mutual relations between that which is known, that which is to be known and the knower. There is a harmony within the system which is at the same time also the harmony of mistakes. The consequence of that statement is that within a certain style of thinking that harmony, including the identification of mistakes, cannot be resolved. The aspect of compulsion, that is to say the aspect that "we can't help it" in reaching certain conclusions, is to be discerned within certain pre-suppositions; within the circle of those we can reach some consequences which are of a compulsory character. There is no point in referring to "consciousness in general" which an individual is, as it were, its concrete carrier. We have to place the individual within the collective of thinking and thus the individual is not to be looked at in its isolation. There is a necessary correlation between the existence of a style of thinking and the construction of the concept of collective of thinking. In this sense knowing is an activity of man socially determined. It is here that Fleck himself points to the affinity between his view and the views of those who apply the sociological approach to the investigation of spiritual activities. Fleck himself refers to several sociologists and philosophers who are mentioned specifically by him like Comte, Durkheim, Jerusalem and Levy-Bruhl, and Gumplowicz.

3. Facts

An additional issue which we have to mention in this context is the concept of a fact. Fleck starts with the distinction between facts of our everyday life and those open to epistemological investigation, stressing that the facts of everyday life do not lend themselves to a transformation into objects of an epistemological investigation. To be sure, this is only a consequence of the basic presupposition of the whole view, namely that scientific facts have to be seen within the context of a style of thinking. To put it in different terms, the line drawn between that which is thought of and that which exists is
indeed too sharp. We have to assign to thinking a certain creative capacity vis-à-vis certain objects, and the objects have to be seen in their origin from thinking.\textsuperscript{16} We cannot rely on what is called commonsense since it is only a personification of a day-to-day collective of thinking.\textsuperscript{17} Generally speaking, every empirical discovery has to be seen as a supplement in the style of thinking, as its development or transformation.\textsuperscript{18} Hence, what is called a scientific fact has to be separated from what we in a usual way describe as a fact; here again a scientific fact is a certain relation of concepts, conforming to a style of thinking.\textsuperscript{19}

4. Resistance

Yet the trend towards the integration of ‘facts’ into the structure of configuration of a style of thinking cannot make us oblivious of one additional aspect present in Fleck’s view, namely that of compulsion. We have seen already the presence of that aspect within the context of the style of thinking. But it is not enough to identify resistance within the sphere of thinking only. To be sure, within that sphere Fleck points to the general line of the activity of knowing, namely the greatest resistance of thinking versus the smallest arbitrariness of thinking. But he uses also the notion or the metaphor of ‘viso’, as a viso of an obstacle or resistance to be expected or warned against. The fact emerges first in terms of being warned by the resistance and that situation is encountered in the chaotic beginning thinking. Then certain resistance of thinking comes in and eventually there is the configuration which is to be immediately perceived.\textsuperscript{20} There exists the element of resistance, obstacle or being warned by them, apparently both within the sphere of the style of thinking and the collective of thinking but also a kind of a primary resistance when we first encounter something to be scientifically and thus deliberately explored. What is called a fact in the sphere of knowledge is called a ‘viso’ of resistance which as such is pushed against the free arbitrariness of thinking. The fact has to be expressed in the style of the collective of thinking.\textsuperscript{21} To sum up this last point, we could say that the aspect of resistance is part and parcel of the collective of thinking in which we are involved, since we do not think as mere individuals. The lack of arbitrariness amounts to that involvement or that determination. There is apparently an additional aspect that the style of thinking enclosed as it is, namely that at a certain point it is open to that which it encounters and which to a certain extent compels it, or at least serves as its point of departure. This is reinforced by a statement in the paper on Medical Knowledge, that is to say that to
make an object observable, it must first be defined, that is isolated and contrasted against a background or a support. But the isolation against a background is a cognitive act. It does not create the background as such and here — and this is a conjecture only — the encounter takes place, the possibility of identifying the aspect of resistance or rejection vis-à-vis the background and not within the style of thinking. This has to be said in spite of the statement that there are no observations that are true to nature except those that are also true to culture, since we do not refer to observations but to that which can be made observable. That distinction between the observable and the observation is not raised by the various notions presented and explored by Fleck.

**B. AFFINITIES**

Since our concern here is with the trends of thought (Denkformen) present in the philosophical milieu of Fleck's own formulations, we shall follow the line of the previous presentation and deal with several philosophical expressions which are akin to the view presented by Fleck.

1. *Forms of Thinking*

We start with the notion of the forms of thinking as explored by Hans Leisegang; the first edition of his book appeared in 1928. We start with that book and not with Spranger's book on the forms of life — to which we shall turn presently — since the notion of forms of thinking is closer to the notion of styles of thinking — which is a central notion of Fleck's. It has to be mentioned at the very beginning that Leisegang too refers to Levy-Brühl and to the comments made by Jerusalem — that is to say that here too we find an attempt to draw certain general conclusions from the attempts to describe changing modes of thinking and the exploration of the thinking of what is called 'peoples of nature', that is what we colloquially describe as primitive people serve here as an empirical or semi-empirical anchor for the presentation of 'styles of thinking'. Moreover, a distinction is made between the psychology of world-views (Weltanschauungen) and an exploration of different types of logical thinking; that approach has a logical or a phenomenological character and goes beyond the psychological approach. This leads to a certain interaction between the logical and phenomenological exploration and the historical one, which is an analysis of the consciousness of reality which as such is characterised by strata and stratifications which are not unisimilar to the
geological strata. It is because of that that the concept of the law of thinking is replaced with the concept of form of thinking, though every thinking is but an elaboration of a represented reality. To be sure, we read the notion of the form of thinking as a totality of the lawfulness of thinking coherent in itself while looking into the expressed thoughts of an individual. But that form is not confined to one individual but has its expression also in the thinking of another one.

There exists a logical structure of a sphere of objects, the relation pertaining between them and the formation of concepts expressed in propositions and consequences. A graphical presentation of a context of thoughts which makes the logical structure visible can be called a model of thinking. Leisegang is aware of the fact that he takes over the formula forms of thinking from Hegel but also that for Kant categories were forms of thought but he tries to give that notion a broader meaning. He mentions also Cassirer's work which tried to find the form of thought of mythical thinking and we could enlarge this by saying that eventually Cassirer presented different modes of thinking as concurrent manifestations of symbolic forms.

The concept of forms of thinking, in spite of its broad description, is as a matter of fact a concept applied to philosophical systems or trends. Thus, the model of a circle of circles is applicable to different philosophical systems which have been formulated in the course of philosophical development or history of philosophy; Hegel's structure serves here as a model. The same applies to what Leisegang calls the pyramid of concepts which is juxtaposed to the circle of circles, because it presupposes or refers to a top-level notion or concept like for instance Kant's ego which serves as an anchor — the top of the pyramid — for the formulation of the system. In addition to that there are other forms like what is called the Euclidian-mathematical form, the structure of antimonies or circles on the one hand and linear progress on the other. To be sure, there are some coalescing aspects between the various forms, if not in terms of the forms as such in terms of the thinkers who are involved concurrently in different forms, though we can discern the major bias of an individual system in terms of the respective forms at stake.

We have to emphasise here that the subject-matter is a philosophical system and not scientific findings or facts — and this indeed is one of the major differences between the typological approach presented by Leisegang and the historical approach based on the blurring of the distinction between 'nature' and 'culture' which is a characteristic of Fleck's theme and attitude. We shall notice later that this difference prevails vis-à-vis other aspects of the philosophy of the twenties of the present century, which have been described as a
mouli of Fleck's own effort. The concept of 'logic' which Leisegang applies to the various forms of thinking does not connote logic in the formal sense but what we would call structures; there is a plurality of logics, since there is a plurality of systematic models explored under the title of forms of thinking.

When we ask about the origin of forms of thinking we point on the one hand to the origin in the intuition (Anschauung) and on the other to metaphysical interpretation. Metaphysics is built up from logical thinking but it does not emerge out of it. It goes without saying that the last statement which points to two poles as it were — intuition and metaphysics — can be seen as a broad or vague statement and does not call for a more detailed or let us say historical interpretation of the origins of the form of thinking. Perhaps a historical interpretation is precluded because types recur; the reference is not to the spectrum of data which gives birth to the form of thinking on the one hand and in which that form is rooted on the other. The way the self-enclosed form of thinking is central for the whole view is emphasised in the statement that the progress of a science can take place as a continuous development only within the boundaries of a particular form of thinking. This would be perhaps a hint in the direction of the historical dimension. But here too we notice, to say the least, the interaction between the historical component and the form of thinking which is typological and thus a-historical; in the sense the historical dimension cannot be viewed as being central.

When we make the next step to forms of life we notice that the typological aspect prevails too.

2. Forms of Life

It has to be observed here — and we pointed to that already before — that Spranger's book on forms of life preceded chronologically Leisegang's book on forms of thinking. In our exploration we did not follow the chronological line because we take the notion of forms of life to be a broader notion than that of forms of thinking. It has to be observed also that the sub-heading of the book of Spranger presents the theme as psychology of humanistic character (Geisteswissenschaftlich) but also as an ethics of personality.

As we have seen, Leisegang uses the term 'logic' whereas Spranger uses the term 'structure' or 'structural', describing the intention of the book as an exploration of spiritual phenomena to be seen correctly in a structured manner. Since the book is concerned with types or structures, and these are
forms of life, it does not analyse only systems of thought but — perhaps — mainly basic types of individuals, for instance the theoretical person or man, the economic man, the aesthetic one, the social person, the person concerned with power and life and ultimately the religious person — and these types are the central issue of the second part of the book. Probably the emphasis on the 'ideal types' of the individuals enabled Spranger to look at the purpose of his book as being of a psychological character on the one hand and supporting an ethical investigation on the other. He is indeed aware of the danger of any kind of typology because a typology disregards the fluid line of the history — that is to say that history contains in itself different components which ideally or typologically only can be presented as forms or as rigid configurations present one next to another. Moreover the exploration or identification of the types are not the ultimate end; they are put forward only for a conception of history as auxiliary means — for knowledge and understanding. This has to be emphasised because Spranger follows in his view the conception that what the human being is, is told only by history. Thus the historical complexity cannot be taken away. In this sense the identification of types is instrumental for the understanding of history. This can be put differently, — and the indebtedness to Max Weber is obvious — namely that the attempt to understand the whole human being in a period is accomplished by identifying the typical structure, that is to say whether the period is manifest, for instance, in art or in the constitution of a religious community. In this sense, the typology is meant not only to describe the spectrum of human orientations but, by applying it to a certain historical period, to present the configuration of the period at stake. Since history is essentially the ultimate concern and the ultimate locus of realisation, Spranger refers to what he calls 'style of a community', as for instance would be the case in the economic community related to the common sphere in the utilitarian connection or the scientific community whose cohesion is grounded in adherence to equal points of view etc. This enables the identification of forms of life to be not only a clue for the understanding of individual psychological directions or trends, but also to identify what is called collective consciousness which as such is immanent in the consciousness of the individual. But in its meaning it connotes a supra-individual attitude by virtue of which the individual person knows itself as a part and as a representative of a group and is active accordingly. At this point the collective aspect is identical or at least touches on what is called the will of norm. The reference to the collective aspect enabled Spranger to introduce the concept of universal validity which in turn has two meanings: (a) validity for all, that is to say the
universality of the subject-matter and (b) validity in terms of all entities which are comprised under the universal concept or law related to the entities conceived within the scope of that concept or that law. Thus, for instance, for the person engaged in research in as much as he wants to know — his only goal is universal objective lawfulness and a parallel aspect of that goal is the overcoming of passions.\textsuperscript{41}

To be sure, these are ideal descriptions — and Spranger is aware of the fact that such a procedure contains simultaneously isolation on the one hand, that is to say isolation of a certain attitude from the full scope from historical reality as well as idealisation on the other.\textsuperscript{42}

Once idealisation is brought in, an individual cannot be identified as such. The isolation referred to before is an isolation in terms of other forms of life but not in terms of the structure prevailing in a certain specific form. In this sense, the individual is both a part of a collective as well as an exemplar of it.\textsuperscript{43} We could perhaps interpret this statement by saying that to be a part has more a day-to-day connotation, while to be an exemplar has more a connotation in terms of the adherence to the ideal type and the motivation by the structure of that type. In terms of the day-to-day situation — or what is called everyday form of society — we cannot point only to one component, since human beings are bound together both through acts of might and power as well as through acts of sympathy — both through subordination and coordination.\textsuperscript{44} Still, there is a basic difference between individual acts of the spirit and societal acts: in the individual sphere there is a need to act and to refer to the presence of an \textit{alter ego}; this is obviously not the case in the sphere of societal acts of spirit, since these acts have explicitly as their presupposition or their object the other \textit{thou}. There is probably an interaction within the spiritual act of the individual and the trend of knowledge, since that trend is characterised by the reference to objectivity.\textsuperscript{45} To put it differently, every individual phenomenon is accessible in the scientific sense of that term by way of the universe or the universal lawfulness.\textsuperscript{46}

We were concerned here only with several aspects of Spranger's exploration in order to shed some light on the difference between a typological approach and a diachronic-historical one characteristic of Fleck. This has to be emphasised for two reasons. In the first place, as we have seen already, Spranger takes as his point of departure the overriding presence of history; the typology is but a deliberate isolation and idealisation. Still, a typological approach is justified because it identifies certain trends in the spectrum of human activities and instrumentally enables us to deal with certain concrete human modes of behaviour. In terms of the person engaged in cognition or knowledge,
Spranger does not present the style of thinking prevailing in a certain epoch. Apparently, he accepts the traditional identification, namely that objectivity, that is to say the identification of universally valid laws, is a built-in feature of the scientific or cognitive activity. Hence he grounds the concept of the collective within the concept of universal validity. Thus the difference between the collective of thinking present in Fleck and the collectivity appearing in Spranger becomes apparent.

3. Facts

As mentioned before, Fleck refers to the work of Wilhelm Jerusalem. That work appeared in a volume whose editor was Max Scheler. We cannot say that Fleck was aware of Scheler’s philosophical work in its varieties and development and we do not suggest the existence of that awareness. But, just the same, it might be proper to point at least to one analysis present in Scheler, that of facts or what we call facts.

Scheler distinguishes between facts with whom the science is concerned and they are situations; the objects of science are meant to be only symbolic as against those facts called ‘natural facts’ understood as exhibiting things and occurrences. The natural fact compared with the scientific exhibits the feature that is part in a totality. Thus physics starts its activity precisely by eliminating our feelings from the facts and it progresses only in so far as that elimination succeeds. This is clearer in the scope of physics than for instance in the scope of geometry where the space of the natural world-view as the space of geometry are not that much separated. Scheler makes it specific that he does not suggest a total break with the natural world-view but he tries to show that the facts of a natural world-view, its factors, things, relations cannot be taken as ‘given’ for the science. The same applies to the aspect of conformity, that is to say that the facts within the scientific scope cannot be measured in terms of their truth through the measure of the facts of the natural view. As we have seen, Fleck is inclined to look at the scientific fact in terms of its involvement in a certain style of thinking and thus in terms of its isolation from our everyday experience, though eventually the everyday experience cannot escape the impact of the scientific interpretation. There might be a philosophical difference between a view which emphasises a total isolation of the scientific facts and that which emphasises a more mitigated isolation. What is significant for the whole notion of forms in the view explored before and in the view hinted here is that an approach has a certain direction and structure and cannot be seen simply as a continuation of
a spectrum of life or existence in spite, or perhaps because, of the difference between the width of life and the limitations of form.

To see the common feature of all these views including that of Fleck we would say that we find here a kind of a transformation of a certain motive of Kant, namely that there are presuppositions for interpretations of data. To be sure, Kant took the presuppositions as being of a universal validity; he was concerned mainly with the presuppositions of mathematics, science and to some extent also of history. He was not concerned with the presuppositions of philosophical thinking which is the major concern e.g., of Leisegang, neither was he concerned with the juxtaposition of facts in the quotidian sense of that term and the scientific one. The concept of the principle of the universal validity precludes the possibility of looking at the presuppositions as having a tentative or historical validity confined to a certain style of thinking. Probably, since mathematics served not only as a paradigmatic science but also as containing the forms of any approach to data, the universal validity has been looked as safeguarded. This is not the case with forms of thinking where philosophy is at stake, let alone the forms of life where the cognitive attitude is one of several typical attitudes of human beings. This is also not the case with the genesis of the scientific fact in Fleck’s view, since the scientific fact explored is broadly speaking of an empirical character; it is a disease and cannot be placed within the mathematical framework even when eventually some mathematical formulae would be employed. Thus we could sum up by saying that what we find in the development in the notion of forms is a pluralization of forms on the one hand and a historicisation of them on the other.

4. Resistance

We mentioned already, while presenting a summary of Fleck’s major points, that he refers to the aspect of resistance (Widerstand) using the metaphor ‘resistance aviso’. He says for instance that this is the way the ‘fact’ emerges: first resistance aviso in the chaotic beginning thinking, then certain compulsion of thinking and finally a configuration which is to be perceived immediately. He uses the term ‘the resistance aviso’ which relates to the thinking collective. He says that each fact is bound to be placed on the line of the spiritual interest and its thinking collective since resistance is possible only there where striving (Streben) is present. Each resistance is bound to have its impact within the thinking collective and be presented to every participant as a compulsion of thinking and further as a configuration to be experienced
immediately. If we read the text correctly then essentially there are several levels of resistance — one related to the inner logic or momentum of the thinking collective and the other related to that which is immediately perceived. As such, we may approach it with certain preconceptions grounded in the style of thinking — but there is still a dimension of immediacy. To put it in different words we may say there is a component of resistance inherent in what is stated as a fact, since even the style of thinking is not a total construction but contains an aspect encountered. That aspect is juxtaposed to striving and thus has, at least, an innuendo of the outside world.

It is not our task to take a critical view of Fleck’s presentation or conception but to point to the fact that the relationship between resistance and the outside world — in Fleck’s language ‘fact’ — is present in the development of modern philosophy — perhaps since Friedrich Heinrich Jacobi’s argumentation. In any case, it appears in several philosophical presentations which will be briefly mentioned.

We start with Dilthey’s analysis of the solution of the question of the origin of our belief in the reality of the external world published in 1890. There is good reason to start with Dilthey since he originated, to some extent, the idea of types of world-views (Weltanschauungen) which we analysed before in their explorations; the second reason is that Dilthey tried to show that the aspect of resistance comes to prominence as being in contradiction to what he calls impulse — and what Fleck calls striving. In the juxtaposition between the impulse and the resistance the first experience of the distinction of the self and the other is made. Here we find the first seed of the distinction between the ego and the world. The reality of the external world cannot be established by way of reasoning only or by way of what Dilthey calls “mere occurrences of thinking”. We have to introduce into the context the experience of will and the obstacles placed on intention. In this sense there is a difference between an arbitrary motion and the experience of resistance. This view is grounded in a certain broader anthropological conception namely that the human being is in the first place what Dilthey calls a system of urges (Trieb) and only out of such a system the complex nature of the experience of resistance can be resolved. We do not claim to say that Fleck would subscribe to that view: the emphasis placed on urges obviously diminishes the position of thinking. But disregarding this significant difference we find in Fleck a sort of a coexistence between the aspect of thinking and the aspect of resistance which has an immanent character in terms of the logic of the style of thinking. But we find also an encounter with which that is given — and here, as we have said before, striving becomes
central. It has to be mentioned in this context, precisely because of the aspect of the sociology of knowledge which we mentioned before referring to Scheler, that Scheler too refers to the aspect of resistance. He says, though taking exception to some of Dilthey’s presentation, that resistance itself gives rise to the act of reflection. Only resistance, obstacle and a passive attitude gives birth to man’s vision of the inner world. But even the concept of striving appears explicitly in Scheler’s work when he says that resistance is a phenomenon which is given immediately only in striving: and this is in turn related to willing. In willing and only in it the consciousness of practical reality is given.

As a matter of fact, Nicolai Hartmann in his analysis of the problem of the givenness of reality mentions Scheler’s view coining it a ‘voluntative realism’. What is described as the hardness of the real is a corollary to the receptive experience and what he calls ‘Erleiden’. The aspect of reality, or the awareness of it, is related to receptivity where the human being or the subject is affected; yet that receptivity is not one stemming from objects but is experienced in rejections or resistances. We encounter here a cluster of experiences, that of being affected — that of encountering resistance and being compelled to a certain mode of action; hence the certainty of reality cannot be brought back to acts of cognition but to emotional acts. In this sense any mode of thinking is overcome. Summing up the acts at stake, they are described as emotional transcendent acts; they are not modes of one’s finding himself where finding oneself would connote the inner configuration of the subject while the emotional transcendent acts refer to the situation of one’s finding oneself within the world.

The purpose of this exploration has been to show that Fleck, again consciously or not, relates here to certain formulations within his contemporary schools of thought. This is so in spite of the ambiguity we referred to before, that is to say that on the one hand Fleck is so anxious to point to the rounded structure of the style of thinking; still he considers, on the other hand, as essential to stress some kind of openness of the style to facts encountered as resistance or foci of existence and, by the same token, to refer to striving that goes beyond the style of thinking in spite of the emphasis on thinking. Perhaps we encounter here one of the limitations of a conception which programmatically tried to remain within the immanent boundaries of thinking.

5. The Archaeology of Knowledge

We were concerned until now with some contemporary trends and Fleck’s own reference to sociology of knowledge served as a kind of a link between
his own position and the philosophical trends of his era. Before summing up we want to present Fleck's position as a kind of an anticipation of that which followed his time — and the case of Thomas Kuhn is well known — and we refer here to some points in Foucault's position. Foucault uses the term 'space of knowledge' as well as 'epistemological space' specific to a particular period. He uses also the description of a positive unconscious of knowledge which eludes the consciousness of the scientist and yet is part of scientific discourse. This emphasis on the epistemological space of a period somehow mitigates the disputing of the validity of the findings of knowledge placing it in a historical consciousness of a spirit of time. Foucault refers to Kant who marks the threshold of our modernity. The hard core of modernity would amount to the withdrawal of knowledge and thought outside the space of representation, whereby representation amounts to a definite reference to something given. Thus the emphasis shifts from a referential aspect of knowledge to its self-enclosed character with the proviso that that character is in turn placed in history. History of human knowledge could both be given to empirical knowledge and prescribe its force. Hence knowledge has historical, social or economic conditions. It was formed within the relations that are woven between man and it was not independent of the particular form they might take here and there. This consideration leads Foucault to the assertion of what he calls historical a priori and in this case there is 'transcendental mobility'. We can perhaps describe that view by saying that since history is the background against which knowledge is formulated history being a process is exposed to what we shall call now historicity of history. In Foucault's own words

history constitutes ... for the human sciences, a favourable environment which is both privileged and dangerous. To each of the sciences of man it offers a background, which establishes it and provides it with a fixed ground and, as it were, a homeland; it determines the cultural area — the chronological and geographical boundaries — in which that branch of knowledge can be recognised as having validity; but it also surrounds the sciences of man with a frontier that limits them and destroys, from the outset, their claim to validity within the element of universality.

The emphasis on the a priori but of a mobile or historical character is clearly cognate to Fleck's position and in this sense Fleck anticipated Foucault's view.

It has to be observed here that Foucault uses also the term "a certain style, a certain constant manner of statement". What we seek is not an external a priori authority of knowledge but the rules of its formation in discourse
itself. The historical or historistic aspect is emphasised by the saying that the a priori is itself a transformable group of statements. In this sense, when we go to the archaeology of knowledge, that is to say come back to the beginning of it, we are brought to the conclusion that the archaeological aspect does not have a unifying but a diversifying effect.

C. RÉSUMÉ

Trying to look at the climate of opinion in which Fleck formulated his view we can say that the underlying presupposition is that knowledge is not just a picture of reality or of facts. Knowledge is of a constructive character — and here obviously we notice an echo of Kant’s position. Yet like the various attempts exhibited in the philosophical presentation of Fleck’s contemporaries, he is not concerned with coexistent types of knowledge let alone of philosophy but with types grounded in the historical process and thus exhibiting a Zeitgeist. This makes Fleck’s view a historical interpretation of the a priori and because of that we brought Foucault into the context.

It is not our task, as hinted before, to go over to a critical analysis of Fleck’s position. It is clear that Fleck took seriously the view that man approaching reality of facts is not a tabula rasa and the negation of the tabula rasa is very broad. This negation led Fleck to the view that man approaches reality and out of his approach facts emerge being formulated from a position of a certain style grounded in history. Because of that he emphasised for instance in his article on the ‘Crisis of Reality’ that man knows that what he received from the historical accumulation of knowledge. In this sense, speaking again from the point of view of 20th century philosophy, the distinction between natural sciences and sciences of humanities (Geisteswissenschaften) gets blurred or even abolished, though the task to distinguish between historical facts and scientific facts is still a task of the philosophical approach.

Yet when we speak about the historical dimension and about our learning from history let us wind up with what Kant says about historical knowledge:

Historical knowledge is cognitio ex datis: rational knowledge is cognitio ex principiis; however a mode of knowledge may originally be given, it is still, in relation to the individual who possesses it, simply historical, if he knows only so much of it as has been given to him from outside ... whether through immediate experience or narration, or ... through instruction. Anyone, therefore, who has learned (in the strict sense of that term) a system of philosophy, such as that of Wolff, although he may have all its principles, explanations, and proofs, together with the formal divisions of the whole body of doctrine, in his head, and, so to speak, at his fingers' ends has no more than a
complete historical knowledge of the Wolffian philosophy. He knows and judges only what has been given him . . . his knowledge has not in him arisen out of reason, and although, objectively considered, it is indeed knowledge due to reason, it is yet, in its subjective character, merely historical.\textsuperscript{74}

Perhaps the experimental character of knowledge, to which Fleck refers and which is focus of his exploration, carries with itself a kind of mitigating factor of that characterisation of historical knowledge.

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\textbf{NOTES}

\textsuperscript{1} "Prä-Ideen" – Fleck's own expression – called also \textit{Archidea}. My friend Professor Y. Elkan called my attention to the fact that the expression is Joseph Glanvill's. I thank Professor Elkan for all the comments he made with regard to the present paper.


\textsuperscript{3} Fleck: 'On Foundations of Medical Knowledge' is contained in: Thaddeus J. Trenn (tr.), Ludwik Fleck's 'On The Question of The Foundations of Medical Knowledge', republished in \textit{The Journal of Medicine and Philosophy} 6 (1981), 237–256. The references contain among other items E. Schrödinger's \textit{Ist die Naturwissenschaft Milieubedingt?}, Barth, Leipzig, 1932. It is not clear to the present reader whether the article of Fleck in its original publication referred to that lecture of Schrödinger or it is an addendum of the editor Dr. Trenn.


\textsuperscript{5} \textit{Ibid.}, p. 130–131.

\textsuperscript{6} \textit{Ibid.}, p. 38.

\textsuperscript{7} \textit{Ibid.}, p. 35.

\textsuperscript{8} \textit{Ibid.}, p. 140.

\textsuperscript{9} \textit{Ibid.}, p. 141.

\textsuperscript{10} \textit{Ibid.}, p. 31.

\textsuperscript{11} \textit{Ibid.}, p. 53.

\textsuperscript{12} \textit{Ibid.}, p. 50.

\textsuperscript{13} \textit{Ibid.}, p. 56–57.

\textsuperscript{14} \textit{Ibid.}, p. 58.

\textsuperscript{15} \textit{Ibid.}, p. 62–63. As a matter of fact Max Weber uses the term 'Lebensstil', and 'Lebensreglementierung' implying a form of behaviour and representations of values of a certain group of people. \textit{Gesam. Aufsätze zur Religionssoziologie} (Siebeck) Tübingen, pp. 43, 327–368.
This is a comment which probably is not unrelated to the various versions of the distinction between Naturwissenschaft and Kulturwissenschaft.


Eduard Spranger: Lebensformen, Geisteswissenschaftliche Psychologie und Ethik der Persönlichkeit (Fünfte vielfach verbesserte Auflage), Verlag von Max Niemeyer, Halle (Saale), 1925. Wittgenstein’s relation to the concept of ‘forms of Life’ has to be explored.

Vorwort p. IX.

Ibid., p. 407.

Ibid., p. 445.

Ibid., p. 445.

Ibid., p. 405.

Ibid., p. 405.

Ibid., p. 360.

Ibid., p. 369 – text and footnote.

Ibid., p. 376.

Ibid., p. 124.

Ibid., p. 358.

Ibid., p. 112.

Ibid., p. 63.

Ibid., p. 122.

Ibid., p. 391.


Entstehung etc., p. 129.

Ibid., p. 129.

Ibid., p. 132.

‘Beiträge zur Lösung der Frage vom Ursprung unseres Glaubens an die Realität der Aussenwelt und seinem Recht’, Ges. Schriften, V. Bd. B. T. Teubner Stuttgart, and
Vandenhouck & Ruprecht, Göttingen, p. 105. It is obvious why Dilthey used the term ‘Glaube’ and not ‘Argument’ or ‘Beweis’.

74 Kr. d. v. *Vernunft B*, p. 867, Kemp-Smith, transl.
JERZY GIEDYMIN

POLISH PHILOSOPHY IN THE INTER-WAR PERIOD AND
LUDWIK FLECK'S THEORY OF THOUGHT-STYLE AND
THOUGHT-COLLECTIVES

The aim of this article is to contribute to an understanding of the genesis,
significance and reception of Ludwik Fleck's theory of thought-styles and
thought-collectives (Fleck, 1935, 1935a, 1936) by considering its epistemological
content in relation to the philosophy in Poland in the nineteen
thirties.

I. THE PROBLEMS OF THE ORIGIN OF FLECK'S
EPISTEMOLOGICAL IDEAS AND OF THE IDENTIFICATION
OF THOUGHT-COLLECTIVES

The following features and claims seem to form the core of the epistemological
content of L. Fleck's theory of thought-styles and thought-collectives
and of his epistemological programme:

(a) The rejection of the traditional empiricist and positivist view of scientific facts as 'given' in contradistinction to theory,

(b) The rejection of any a priori or speculative theory of knowledge including abstract models of science, e.g., in the form of logical reconstruction, as 'epistemologia imaginabilis';

(c) The requirement that epistemology should be empirical, based on the psychology, history and sociology of science, as well as on the assumption that all knowledge is socially determined,

(d) The claim that human knowledge is developed by groups of interacting individuals — thought-collectives (some momentary, others more stable) — which produce from earlier stocks of ideas (proto-ideas) mature, closed, belief-systems, possessing their own internal criteria of validity, with concepts at least partly 'incongruent' with the previous systems, and permeating — as thought-styles — the thinking of the individual members of the collectives. Such closed belief-systems are characterised by strong tenacity, supported whenever necessary by the 'harmony of illusions'; an individual may belong to more than one thought-collective, but it is the latter that is the originator and carrier of knowledge, since knowledge is always social in its origin and

character. There are no absolute criteria of truth (truths are "the only style-compatible solutions of problems") and validity; epistemology should be a comparative study of thought-styles.

(e) In the content of a cognition one may distinguish active from passive connections or linkages (Koppelungen); the former are psychologically and historically explainable in terms of changeable psychological and historical factors, the latter are not and are in this sense more 'real' or objective; cognition, therefore, amounts to ascertaining those results which must follow, given certain preconditions (19: 56, 85); the preconditions are active linkages contributed by the collective; the results enforced by the preconditions are experienced as objective reality; the act of ascertaining is the contribution of the individual. Every proposition in science, especially every scientific law, can be analysed into its passive and active components. Experience is a complex process based on the interaction between the knower, that which he already knows and that which he is about to learn.

(f) Of the three factors of cognition — the individual, the collective and objective reality — one may eliminate at least one or even two; the collective is composed of individuals, objective reality can be resolved into historical sequences of ideas belonging to the collective; however, the individual’s thinking cannot be at variance with the thought-style of the collective, in fact the individual is hardly ever aware of the prevailing thought-style; hence, one can dispense with the concept of a thought-collective in epistemology only by introducing either value judgements or dogmatic faith, which would not be desirable.

Apart from its epistemological content, briefly summarised here, Fleck's (1935, 1979; 1980) book contains also a sociological analysis of the structure of a scientific group, its communication system etc.

Most readers who have recently become acquainted with Fleck’s theory through the American (1979) translation or through the German (1980) reissue have probably asked themselves the following questions:

First, how — at the time when all European philosophy of science was dominated by logical empiricism (positivism) — the appearance of a philosophical book was possible which pictured science in a manner at variance with the dominant philosophy and advocated the replacement of traditional epistemology and of its logical-empiricist variant with comparative epistemology based on the history, psychology and sociology of science?

Second, why did the book with the theory and the epistemological programme presented in it remain practically unknown, not only before the last war and in the two decades immediately following the war, but also in the
sixties and seventies when the thought-style of the philosophy of science became in fact so close to Fleck's?

An answer to the first of these two questions which immediately suggests itself is this:

Fleck's view of science and his epistemological programme was the result of a critical reaction of an outsider, viz., a practising scientist with a biological-medical background, to the prevailing logical-empiricist (or positivist) approach represented by the professional philosophers of science in the universities in the nineteen-thirties; this critical reaction was further reinforced by his interest in and familiarity with the sociology of knowledge, especially the works of Durkheim, Scheler, Simmel, Gumplowicz and Jerusalem. In other words, Fleck's epistemological views were the product of his participation in several different thought-collectives (medical, philosophical, sociological) and hence thought-styles, a fact which according to his own theory explains, partly at least, most discoveries and innovations (Fleck, 1980: 61).

The above answer, prima facie very plausible, gives rise to various problems if one attempts to examine and assess it in the light of Fleck's own collectivist epistemology. Some of these problems concern the concept of the thought-collective in general, others the determination of Fleck's thought-collective in particular.

It seems clear that the questions of the genesis and reception of a theory, whether scientific or philosophical, require — from the point of view of Fleck's epistemology — an examination of the relevant thought-collective or thought-collectives. For, according to this epistemology, thought-collectives are the originators and carriers of progress in a intellectual discipline, in an element of science or in culture in general, hence in thought-styles. But how does one determine — generally speaking — the identity of a thought-collective relevant to a theory and — in particular — which thought-collectives were relevant to Fleck's philosophical doctrine? Is the fact that Fleck was not a professional philosopher (i.e., not an academic philosopher) sufficient to classify him as an outsider with respect to the Polish philosophical community of the interwar period or with respect to any philosophical community? Surely not if this fact is taken on its own and especially if it is generalised. For most philosophers in the past were not professionals and — more recently — there have been non-professional philosophers strongly interacting with professional ones (e.g., Wittgenstein in Vienna). According to Fleck's definition, a thought-collective is "a community of persons mutually exchanging ideas or maintaining intellectual interaction ..." (Fleck, 1979: 39; 1980: 54). But how essential is the requirement that intellectual exchanges be
mutual? Within an intellectual group influences may be one-sided (as was the case with Wittgenstein and the Vienna Circle); this is often reflected in non-reciprocal references due either to priority of discoveries or to precedence in intellectual and professional standing. Non-reciprocal references will, of course, also occur to the writings of authors who founded an intellectual discipline or a tradition, especially if those authors are no longer alive (for example, those on the list of intellectual ancestors of the Vienna Circle in *Wissenschaftliche Weltauffassung: der Wiener Kreis* (1929)). This asymmetric, ancestral relationship constitutive of an intellectual tradition as well as of a hierarchical group structure appears not to be allowed by Fleck's definition of a thought-collective. And yet, it is exactly this relationship which linked him to Durkheim, Lévy-Bruhl, Gumplowicz, Scheler and Jerusalem to whose writings he makes explicit references. Is, however, absence of explicit references to an author or a group of persons sufficient evidence of the non-existence of a common thought-collective? Surely not, if non-reciprocal references do not constitute such evidence. Furthermore, failure to make explicit references may be accidental and selective, due to temporal failure of memory, accidental and total, due perhaps to an individual's unorthodox style (e.g. Einstein's 1905 paper on Special Relativity) or else it may be systematic and selective, as is often the case when priorities are in dispute or when genuine independent discoveries are made (i.e., independent as regards the individuals in question but not as regards their intellectual background or tradition). Since Fleck used the existence of simultaneous discoveries as evidence in support of the social-collective nature of discoveries and of knowledge in general, it would certainly be inappropriate to use the presence or absence of explicit references as the criterion of membership in the same thought-collective and hence of the participation in the same thought-style. But presence of explicit references in itself is, in any case, not sufficient for the membership in the same thought-collective, for otherwise Fleck would share the logical empiricist thought-collective and, therefore, thought-style. It will obviously not do to avoid this consequence to require that intellectual exchanges (expressed in references) should be accompanied by explicit or, at least, implied approval, if they are to qualify as evidence of the membership in the same thought-collective; for one rarely disapproves of all the views of one's intellectual opponents and, moreover, Fleck did not intend to characterise a thought-collective in terms of the absence of any insider criticism. The case of intellectual opponents is a difficult one from the point of view of Fleck's definition of a thought-collective, for opponents often interact through exchange of ideas (e.g., logical empiricists inherited
the Kantian analytic-synthetic distinction though in a somewhat modified form) except perhaps when they completely misunderstand one another. They often share problems without sharing solutions of those problems. Although some opponents might be included in one's thought-collective, the latter idea might become too remote from Fleck's intended sense if all intellectual opponents were automatically given membership in the same thought-collective, hence share the same thought-style.

Bearing in mind all these arguments one must conclude, I think, that cross-references or their absence are not reliable clues for reconstructing boundaries of thought-collectives and making hypotheses about the origins of ideas. They are even less reliable if thought-collectives are seen as blurred rather than well-defined entities, if the internal fine-structure of a collective is regarded as relevant, or — finally — if the individual in question has some reasons for not making the usual acknowledgements of his relationship with the collective. In such cases one has to rely on other indicators, in particular, on content analysis. This I propose to do in the rest of this article with respect to certain selected doctrines advocated in the interwar period by some members of the Lwów and Warsaw schools in philosophy. When this is done then not only will the answer — given already — to the question of the origin of Fleck's ideas be somewhat modified but also the presuppositions of the question itself. Moreover, some premisses for an answer to the question concerning the reception of Fleck's theory will emerge in the process.

It will be pointed out, in the first place, that the Lwów school of philosophy, so important historically for the development of Polish logic and philosophy of science in the interwar period and for the intellectual life of Lwów where Fleck lived and worked, was certainly not dominated by logical empiricism, contrary to popular belief. Moreover, either prior to the publication of Fleck's book or simultaneously with it, several philosophers in Poland advanced views of science which defied positivist or traditional empiricist stereotypes and with which Fleck's philosophy shared important epistemological elements, although the method used by him — viz., the sociological method — and the general epistemological programme he subscribed to were different from theirs. Peculiar to the writings of all those philosophers was the fact that they either belonged to the conventionalist tradition (e.g., Ajdukiewicz) or were affected by certain ideas in conventionalist philosophy without accepting all of it or even without identifying those ideas as associated with conventionalism. The conventionalist philosophy in question was not that of Ernst Mach — to whom Fleck refers critically in his book — but rather the French critique of science which
between 1905 and 1935 found a receptive and rejuvenating ground in Poland.

It is most noteworthy that there are striking analogies between some of the most fundamental ideas of the conventionalist philosophy on the one hand and the sociology of knowledge on the other, analogies which have been largely overlooked in relevant literature. In both cases there is a criticism of the traditional empiricist view of knowledge as a passive mirroring of reality through an individual's senses and there is an emphasis on the role of non-empirical, creative, or conventional, subjective, cultural components of our knowledge. (These non-empirical elements may be seen as a priori although the a priori is understood here as either due to biological evolution, to social and cultural structures whose life-span is considerably longer than an individual's or due to much more rapidly changing pragmatic considerations of an individual or group.) In both cases theories or belief-systems (in a wider sense) are regarded as closed structures governed to a large extent by internal coherence and social consensus as two effective (constructive) 'truth' criteria and as adapting to the external pressure of experience through the changes in the creative or conventional components in such a way that the main features of the system remain conserved as long as it serves well the aims of the community. The closed nature of these systems, moreover, is believed — in both cases — to affect the language, in particular the meaning of expressions. In this way, either all absolute criteria of validity, truth and meaning are rejected and epistemological and linguistic relativism proclaimed or else the search for some invariants becomes paramount in order to avoid extreme relativism. The issue of epistemological and linguistic relativism is considered not only with respect to the scientist or the lay knowing subject but also with respect to the epistemologist himself. The unmasking of some components of our knowledge as non-empirical but performing some biological, social or pragmatic function, is thus coupled with the denial of the impartial and uncommitted role of the epistemologist.

The mentioned analogies between the conventionalist philosophy and the sociology of knowledge are the more remarkable since the former originated exclusively from the analysis of mathematics and physics whereas the latter predominantly from anthropology, comparative religion and economics. On the other hand, both French conventionalist philosophy and Durkheim's sociology were products of the interaction in France between the rationalist tradition of Descartes and Kant and of the empiricist tradition of d'Alembert, Maupertuis, Condorcet, Laplace, Monge and Comte. Among Durkheim's intellectual masters were Fustel de Coulanges and Emile Bourdieu. The search for the constant or conservative elements in the variable expressions of the
religious life of the ancient Greeks and Romans in de Coulanges (1864) was methodologically analogous to the search for the invariants under theoretical change within the “new critique of science” and Emile Boutroux (Poincaré’s brother-in-law) was one of the leading representatives of the latter. More importantly, perhaps, the conventionalist philosophy of science owed its origin to the impact of rapid changes in mathematics and physics which accelerated during the 19th century and which at the turn of the century led to the crisis in both, a crisis perceived by many as “the bankruptcy of science”. This is similar to the social context in which the sociology of knowledge emerged and evolved (cf. Merton 1945, 1974: 8–11).

Having mentioned the analogies between the conventionalist philosophy and the sociology of knowledge, one should point out at least one important disanalogy: the most characteristic feature of the sociology of knowledge, at least in its early stages of development, is the search for the dependence of the creative (conventional) elements of knowledge from the “existential basis”. There is no analogous to this in the philosophy of the conventionalists, if the ‘existential basis’ is understood in economic terms; there is, however, if the latter is taken in the sense of biological evolution (Poincaré) or in the sense of Bergsonian life (LeRoy). In any case, within the sociology of knowledge itself there evolved a trend, away from the original almost exclusive emphasis on the socio-economic determination of culture and knowledge, towards a sociology of scientific groups, their internal structure, communication systems, social role of scientists, etc. Also the existential basis came to be seen later in broader terms and its effect as more indirect. This tendency was clear, for example, in The Social Role of The Man of Knowledge (1940) by Florian Znaniecki, the leading Polish sociologist of the interwar period. Fleck himself saw the social determination of knowledge in terms of the concept of the thought-collective (1935, 1980: 135) and he emphatically claimed that a thought-collective was not identical with a social class (1935, 1980: 85–6). In Polish philosophy at the time, in particular within the Lwów school (Twardowski and his followers), the links between knowledge and the existential basis were studied exclusively through the examination of the language as a communal product and as a vehicle of communal psyche and cultural life, including changes in both.

Fleck’s only references to the conventionalist philosophy are contained in his criticism of Mach’s allegedly “formal point of view” (i.e., “independent of historico-cultural interrelationships”) with respect to our freedom to define any concept in more than one way, in other words with respect to the conventionality of the conceptual framework (Fleck 1935, 1980: 14–15; 1979:...
8–9). These references are misleading in at least two ways. In the first place, closer analysis of Mach’s works, especially of his book on the history of mechanics, will surely show that he did not see the development of the conceptual apparatus of science in this “formal” way, as detached from and independent of the historico-cultural background. Fleck seems to have mistaken here — as have done many other readers — the claim that our fundamental concepts are not uniquely imposed upon us by experience or by a priori intuition and in this sense are conventional (and arbitrary, free) with another claim — never made by conventionalists — that conventions depend only on the free choice or on the whims of the individual scientist and are unaffected by experiential, socio-historical or biological circumstances. One wonders, therefore, whether Fleck had in fact had much first-hand familiarity with Mach’s writings to which he refers. More important, however, is the fact that Fleck’s apparently critical attitude towards Mach’s formulation of conventionalism may easily conceal a fundamental element in his own epistemology which it shares with conventionalism. It is the conception of knowledge as formed of ‘active’ and ‘passive’ relations or ‘couplings’ (Koppelungen), as Fleck calls them, presumably following the terminology used in Heinrich Hertz’s mechanics.3 The former are our own creations and are, therefore, often seen as subjective; the latter can only be discovered once the former have been laid down; they are, therefore, seen as objective or imposed from outside (Fleck, 1935, 1980: 14–16, 68, 109–111). Every scientific statement is analysable into these two types of components (1980: 110). The distinction is essential for the conventionalist view of how progress is possible in objective knowledge in spite of frequent changes in conventional or subjective elements. It is also essential for their claim that a theoretical system can always be saved from falsification (through the use of what K. Popper called “conventionalist stratagem”) and that, therefore, changes in theoretical systems are never brought about by “pure experience” but only by experience permeated by decisional or conventional elements.

Retrospectively one can see that Fleck’s perception of the conventionalist philosophy was largely mistaken, as was in general the perception of that philosophy in his day, and for quite a long time to come.4 One must not, therefore, take his renunciations of ‘conventionalism’ too seriously but rather examine the possibility that the direct or indirect effect of the conventionalists (and conventionalist-related) tradition on his thought-style was greater than he cared to admit or was aware of. This would be in accordance with Fleck’s own claim that an individual is rarely if ever aware of his own thought-style. Moreover, if a thought-collective is to be understood in “functional rather
than substantial" terms, and is to be seen in a way comparable to the physicist's concept of a field of force (Fleck, 1935, 1980: 135), then it would be quite proper to suppose that although Fleck's interactions with a wider thought-collective (not recorded and not appreciated by him) were momentary yet had lasting effects on his philosophy. It is also quite understandable that he was unaware of having had some intellectual ancestors in common with his contemporaries in Polish philosophy: though he recorded his indebtedness to Durkheim, he seemed unaware of Durkheim's affinity with the rest of French philosophy at the turn of the century, the philosophy whose many ideas were reincarnated and further evolved within several philosophical doctrines in Poland between 1910 and 1935. Four such style-related doctrines will be briefly reported in the next chapter in the context of Polish philosophy in the interwar period.

2. PHILOSOPHY IN POLAND IN THE INTER-WAR PERIOD:

GENERAL SURVEY

After World War I there were six universities in Poland, viz., in Warsaw, Krakow, Lwów, Poznan, Wilno and Lublin. Several varieties of philosophy were represented in them by groups or individuals.

At the extreme right, philosophically speaking, there were representatives of so-called Polish messianic philosophy, the followers of the 19th century rationalist philosopher Jozef Maria Hoene-Wronski (1778–1853). Wronski's system of metaphysics and philosophy of history had been designed to serve Polish nationalist interests and to explain political misfortunes which oppressed Poland during the last quarter of the 18th and throughout the 19th century. In the interwar period the followers of Wronski did not hold any academic posts but were organised in the Messianic Association and had their own journal. On account of their metaphysics, their nationalism and preoccupation with the interpretation of Wronski's obscure writings, within the Polish philosophical community Wronskists were seen as irrationalists and obscurantists.

Catholic theologians and philosophers formed a large but by no means completely homogeneous group. One of their centres was the theological faculty of the Jagellonian University in Krakow headed by a renowned historian of scholastic philosophy, Konstanty Michalski. Another center was in the new catholic university in Lublin, founded in 1918. Neo-Thomism was strongly represented in both, but there were other trends of thought as well. For example, Jan Salamucha, a very competent and promising logician (killed
during the last war) attempted to modernise scholastic philosophy with the help of modern logic. A similar attitude characterised I. M. Bochenski, a dominican, who subsequently left for Rome and settled down in Fribourg. Catholic philosophers who produced one of the earliest criticisms of logical positivism, had many contacts with other philosophers and shared with them many interests.

Phenomenology was represented chiefly by Roman Ingarden (1893–1970) a man with a strong personality and considerable authority. Originally a student of Edmund Husserl, he later rejected Husserl’s idealism and developed a realist version of phenomenology in his Dispute over the Existence of the World, the first two volumes of which did not appear until 1947 and 1949 respectively. Of his earlier works the best known was Das Literarische Kunstwerk (1930), an original contribution to the aesthetics of the literary works of art which moreover formed an important step in the evolution of Ingarden’s philosophy towards a realist version of phenomenology. The latter work was influenced by K. Twardowski’s Brentanoesque distinction between actions (operations) and their products and by Twardowski’s conception of psychophysical objects. Ingarden lectured in Lwów University during the interwar period. He was in opposition to and was regularly criticised by the leading Polish nominalists, T. Kotarbinski (Warsaw) and L. Chwistek (Krakow before 1930, Lwów after).

The credit for the revival and organisation of Polish philosophy in the 20th century is by general consensus given to Kazimierz Twardowski (1866–1938) who was appointed to a chair of philosophy in Lwów University in 1895. Between 1895 and 1930, when he retired, Twardowski created in Lwów a lively centre of research and teaching in philosophy, psychology and logic. A great number of Polish academics in these areas and many philosophically oriented specialists in other areas, came from his school. Four of Twardowski’s former students, viz., J. Łukasiewicz (1878–1956), S. Lesniewski (1886–1939), T. Kotarbinski (born 1886) and K. Ajdukiewicz (1890–1963) together with some younger scholars with mathematical background, in particular A. Tarski (born 1901), formed in the twenties and thirties what later came to be known as the Warsaw school of logic and philosophy. The Lwów and the Warsaw schools were the most influential and creative philosophical groups in the inter-war Poland. Polish philosophy of science, in particular, was dominated by these two groups.\textsuperscript{5}

Twardowski, who with Husserl and Meinong had studied philosophy in Vienna under Franz Brentano, adopted Brentano’s idea of philosophy — of the theory of knowledge in particular — as based on so-called descriptive
psychology. The latter was supposed to be a detailed and purely descriptive analysis of psychological or mental phenomena which arise, according to Brentano, in the mental operations of representing, judging, willing (Vorstellen, Urteilen, Wollen) etc. Within descriptive psychology a classification was given of mental phenomena. It was claimed that a mental phenomenon differs from a physical one by the existence in the former of an immanent object (the intentional object) towards which the relevant mental act is directed (to which it points). So, for example, all objects of perception are intentional objects. Knowledge was seen as consisting of mental phenomena, hence Brentano’s philosophy implied a psychologicistic view of all the branches of knowledge, including logic, mathematics, the humanities, etc. Logic, for example, was seen as a branch of psychology concerned with the conditions on which the truth or falsity of judgements depended. In 1900 the latter view was subjected to an incisive criticism by Husserl in his Logische Untersuchungen. Husserl’s anti-psychologism had a wide appeal, the more so that it reinforced Frege’s earlier criticism of the psychologicistic view of arithmetic. Stimulated by Husserl’s criticism, Twardowski wrote an article entitled ‘Actions and Their Products’ (1912). He distinguished in it between physical and psychological (mental) actions (operations) and — accordingly — between physical and psychological products of these actions; among physical actions he further distinguished psycho-physical ones and — accordingly — also psycho-physical products (objects). On this basis he was able to give a non-psychologicistic account of logic, mathematics and the humanities. Psychology, like natural science, is — on this view — an empirical science, distinct however from natural sciences since unlike the latter it is concerned with mental phenomena and since its method consists of both introspection and extra-spection. Psychology examines mental phenomena which are complexes made up of actions (operations) and their products; it studies also dispositions, like memory, imagination, sensitivity, etc., on which mental phenomena depend. On the other hand, logic, mathematics and the humanities — unlike psychology — are concerned with the products of mental and psycho-physical activities as if these products existed independently of mental acts which create them. This is possible because some mental products are expressed in psycho-physical objects and in this way acquire not only a relatively longer existence but also a sui generis autonomy. Works of art are, par excellence, psycho-physical objects and can be studied independently of and along with the psychology of those who have created them and of those who enjoy them; aesthetics is not part of psychology, the psychology and sociology of art are disciplines distinct from aesthetics. Linguistic expressions form
another important category of psycho-physical objects and linguistics but also logic and mathematics are independent of psychology. Moreover, psycho-physical objects, especially language, scientific theories, works of art, etc., in turn affect our mental life, often — like the sorcerer’s apprentice — in an uncontrollable and unintended way (books shape our minds, works of art shape our tastes, create styles, provoke imitations and caricatures; scientific theories direct our attention, are used for practical purposes and provoke criticisms and new discoveries which eventually lead to the overthrow of the original theory). Most importantly, in his evolution man has become so dependent on language that any progress in abstract thinking is impossible without it; language has become an active instrument or an “organ” of thinking and is no longer merely a means of expressing thoughts and of communication.

The Brentano-Twardowski philosophy did not appeal much to most of Twardowski’s students. He influenced them not so much through any specific philosophical doctrine as through his teaching activities and the ideals of clarity, precision and rationality which he preached. Brentano’s conception of philosophy as based on descriptive psychology, which Twardowski was keen to cultivate in Lwów, did not find many enthusiasts in either the Lwów or the Warsaw school. The idea of intentional objects, crucial to all Brentano-inspired philosophy (Husserl, Meinong, Twardowski, Bolzano) was consistently and severely criticised and rejected as meaningless by Kotarbinski and by other nominalists, e.g., Chwistek. On the other hand, it was used by Ajdukiewicz, especially in his early writings, and, naturally, by the phenomenologist R. Ingarden. Twardowski’s distinction between actions and their products and the concept of psycho-physical objects as signs of psychological phenomena had a more sympathetic reception. It was, however, his conception of language as both a vehicle of and constraint on thinking, hence as playing an active, creative role, that found its most interesting and original continuation in Ajdukiewicz’s conceptions of language, meaning, conceptual apparatus and linguistic world-perspective; these led in turn to the doctrine of radical conventionalism and the thesis of the existence of not intertranslatable languages. Ajdukiewicz’s views will be outlined in the next section. At the moment it should merely be pointed out that though Fleck did not refer to Twardowski in his book it is inconceivable that he should have been unfamiliar with Twardowski’s philosophy, especially with the programme to base philosophy on psychology and with the distinction between mental acts and their products. This distinction and Twardowski’s idea of language as not merely a means of communication but a necessary instrument of abstract
thinking by which thinking itself is determined to a large extent, were close to Fleck’s conception of thought-styles and thought-collectives. However, while allowing or even encouraging the psychology and sociology of knowledge, they would be appealed to in order to criticise any radical psychology or sociology within philosophy as incompatible with the view of the relative autonomy of psycho-physical objects being signs of mental life and with the view that relative autonomy makes it possible and legitimate to study those signs independently of and along with psychological and sociological phenomena. Since this anti-psychologist tendency, so strong in the first half of this century in the area of the philosophy of logic, mathematics, science, humanities, art and linguistics, was mainly the product of Husserl’s and Frege’s philosophy, neither of whom can be regarded as a positivist, one should be wary to see in positivism (logical or otherwise) the main obstacle to a wider reception of Fleck’s epistemology. It should also be clear by now that the Lwów school could not itself be characterised as logical-positivist by any stretch of the imagination. The only positivist element in Twardowski’s own philosophy was the programme which discouraged metaphysics and system-building and favoured concern with specific areas and problems. It is this programme alone together with the ideals of clarity and precision that he shared with all his former students both in Lwów and Warsaw. He certainly did not share the enthusiasm for logic as a substitute for philosophy, expressed most emphatically by Jan Łukasiewicz and dominating the Warsaw School.  

Among the philosophers active in the inter-war period in Poland but not associated closely with any of the philosophical schools mentioned already, was Leon Chwistek (1884–1944). A mathematician, logician, philosopher as well as painter, writer and art-critic, he acquired an international reputation mainly due to his contributions to mathematical logic, especially his version of the simple theory of types (Chwistek, 1921, 1922, 1927/1928) and to the development of a nominalist and constructive system of mathematics (Chwistek, 1923/4, 1932/3). Both were originally inspired by a lecture given by Henri Poincaré in Göttingen University in 1909 on the foundations of mathematics at which young Chwistek was present (Chwistek, 1935, 1948: 78–9). Poincaré’s pre-intuitionism also indirectly inspired Chwistek’s criticism of the concept of (absolute) reality and of (absolute) truth and resulted in a “relativistic doctrine” which Chwistek dubbed “the theory of plurality (multiplicity) of realities”. (Chwistek, 1916, 1917, 1918, 1921, 1923, 1924, 1927). Chwistek’s rather loose style of philosophising about the multiplicity of realities was severely criticised by T. Kotarbinski. In return, Chwistek poked fun at the pedantry and verbalism in Kotarbinski’s textbook of logic
and methodology, revered in Warsaw (Chwistek, 1935, 1948: 34, 125–6, 262–3). Chwistek was also critical of Tarski’s semantics which was another reason for disagreement with the Warsaw school. On the other hand, his reflections on ‘existence’ and his epistemological relativism were critically but much more sympathetically examined by Ajdukiewicz in whose radical conventionalism the concepts of absolute truth and of epistemological relativism were, in a way, reconciled through linguistic relativism. Though Chwistek rejected Poincaré’s geometric conventionalism (as a variety of idealism) he seemed to welcome Ajdukiewicz’s radical-conventionalist doctrine of the variability of the rules of language and linguistic relativism (Chwistek 1935, 1948: 37). The mentioned disputes concerning epistemological and ontological relativism were particularly close to Fleck’s philosophical thinking. Moreover, Chwistek and Fleck must have been in personal contact.\(^7\)

Apart from philosophers who held university chairs in philosophy there was a large number of academics with definite philosophical interests and often original ideas who were specialists in other areas. So, for example, the philosophy of law was represented by Leon Petrażycki; the philosophy of the social sciences by the sociologist Florian Znaniecki (1882–1958); the philosophy of history and the humanities by Zygmunt Lempicki; the philosophy of medicine by W. Szumowski.\(^8\) The philosophy of medicine deserves special mention here firstly, because of the medical background of Fleck and of his (1935) book, and because it had quite a long established tradition in Poland. When universities opened in Poland after the first World War, the third generation of members of the Polish School of philosophy of medicine started to lecture in them. The first generation consisted mainly of non-academics, medical doctors. The founder of the school was Tytus Chałubinski who lectured in Warsaw before 1867. The leading representatives of the second generation were: Zygmunt Kramsztyk (1848–1920), the editor of the first European journal of the methodology of medicine *Krytyka Lekarska* (Medical Critique) founded in 1897, and Władysław Biegański (1857–1917), author of numerous books on the logic, methodology and philosophy of medicine. When in 1907 *Krytyka Lekarska* ceased to appear and its role was taken over by the *Medical Weekly* published in Lwów, Biegański, Kramsztyk and Szumowski were on the latter’s editorial board along with the philosopher K. Twardowski. Soon after World War I ended, chairs of the philosophy of medicine — the first in Europe — were founded in Krakow and Poznań Universities. The journal *Archive of the History and Philosophy of Medicine* — in which Fleck was to publish some of his articles — started to appear in Poznań at the same time.\(^9\)
However original Fleck's ideas may have been, it would be difficult to believe that the very existence of the philosophy of medicine in Poland and of special journals devoted to this type of philosophy, did not have an effect on Fleck.

In the next chapter three style-related philosophical views of science will be reported. They are characteristic of Polish philosophy of science in the nineteen-thirties and exhibit certain traits which have sociological counterparts in Fleck's epistemology.

3. PHILOSOPHY IN POLAND IN THE INTER-WAR PERIOD: THREE STYLE-RELATED CASES

Jan Łukasiewicz: Creative Elements in Science (1912, 1915, 1934, 1961; English 1971) — This philosophical essay, though popular in nature, was very influential in Poland for several decades after its first appearance in 1912 in the Festschrift commemorating the 250th anniversary of the founding of Lwów University. Łukasiewicz's philosophical writings all go back to his Lwów years, prior to World War I. Afterwards he concentrated on mathematical logic and on the history of logic. He owed his position in Polish philosophy mainly to the fact that he was a great authority in the area of propositional logic, its foundations and its history. In 1920 he published his system of three-valued logic ($L_3$), a year before J. L. Post's system appeared; in 1922 (1929) he generalised it to $n$ values (system $L_n$). Unlike Post, Łukasiewicz was led to his discovery of the three-valued logic by philosophical considerations. In an article dated 1910 'Über den Satz von Widerspruch bei Aristoteles' (Bulletin International de l'Académie des Sciences de Cracovie Class Philosophie, 1910, pp. 15–38), Łukasiewicz examined the main assumptions or principles, as he called them, of Aristotle's logic, and arrived at the conclusion that by varying some of them, for example, the principle of bi-valence (the meta-law of excluded middle), one obtains systems of non-Aristotelian logic, analogous to non-Euclidean systems of geometry. He also argued there that a three-valued, non-Aristotelian logic was necessary to accommodate contingent statements about future events (Aristotle's famous example "There will be a sea-battle here to-morrow") and to uphold the thesis of indeterminism. These ideas of Łukasiewicz are relevant to our interest in Fleck's epistemology, since, firstly, they show how using logical analysis Łukasiewicz had arrived at the view that classical, two-valued logic was not the unique, absolutely valid logic, the view which Fleck (motivated by other reasons) opposed to Carnap's (Fleck, 1980: 121), secondly, since
the construction by Łukasiewicz of many-valued systems created in logic the situation analogous to the one which had been brought about in geometry by Lobatchevsky, Bolyai and Riemann and which, eventually, led to geometric conventionalism.

The essay 'Creative Elements in Science' came before the construction of the three-valued logic, but after Łukasiewicz's 1910 paper on the assumptions of Aristotle's logic. The main negative idea of Łukasiewicz's essay was to criticise the widespread view that the aim of science is truth and that the job of the scientist is merely to record so-called facts by describing them in true statements. The main positive idea of the essay was to show that all of science, from top to bottom, is permeated by creative elements and that, to be successful a scientist — far from being a mere reciter of facts — needs, no less than an artist, creative imagination. Neither truth nor generality are sufficient for a statement to acquire scientific status; practical applicability or usefulness is neither sufficient nor necessary for that purpose. To be of scientific nature, apart from being true (or at least, not being false on available evidence), a statement must be capable of either arousing or satisfying, directly or indirectly, the intellectual curiosity of all men who are at the given time sufficiently sophisticated and competent in the given area.

Every intellectual problem that cannot be solved by immediate experience gives rise to reasoning. If you are surprised by the fact that the side and hypotenuse of a square are not commensurate, then you want to have this fact explained and this means to look for statements which would logically imply the fact to be explained. If you are afraid that the path of the Earth will be crossed by the tail of a comet, then you will try to infer from the known laws of nature what might be the consequences of this fact. A mathematician who is uncertain whether the equation \( x^n + y^n = z^n \) is solvable in integers (different from zero) for \( x \) greater than 2, is looking for a proof, in other words he is looking for other accepted statements from which Fermat's theorem would be derivable. A man who is in doubt whether he can trust the evidence of his senses because he experiences hallucinations occasionally, may want to test their objectivity, perhaps by looking for the consequences of the supposition that his experiences are not the result of hallucinations, for example by asking other people if their experiences are similar to his. Explanation, inference, proof and testing are the four types of reasoning. These do not differ formally, since in any one of them there are at least two statements connected by logical implication. The difference between them is rather psychological, since it concerns what is regarded as given and unquestioned at the moment and what is being looked for.
Every reasoning contains a new and creative element but this is foremost true of explanation. So-called incomplete induction is an example of explanation, for this is a reasoning in which we are looking for a universal statement of the form 'Every S is P' which logically implies singular statements of the form 'S₁ is P', etc. The universal statement may be interpreted either as an abbreviation of a number of such singular statements or as a statement about (causal) relations. In either case the statement goes beyond singular statements based on observation — which was already pointed out by D. Hume — and so does not merely record the facts given in experience. In either case by formulating the universal statement we construct something new. Consider now Galileo's law of free fall "The velocity of all freely falling bodies is proportional to the time of their fall", which describes a functional relation between the velocity \( v \) and the time of fall \( t \) as expressed by the formula \( v = gt \). By substituting various values for \( t \) we obtain from the formula an infinite number of consequences, most of which have never and will never be tasted by anyone. But the creative novelty of this law consists also in the particular form of the formula expressing it. Since there are definite limits to the precision of any measurement, it is never possible to verify precisely that the velocity is proportional to the time of fall. Therefore even the particular form of the relation asserted by the law's formula is not a mere recording to observed facts but is invented by our creative intellect. Furthermore we know that the law under discussion may only be approximated by the results of observation, since it presupposes conditions such as the vacuum and the invariance of \( g \), which are never satisfied: for this reason also the law does not simply record facts but rather constructs a fiction. No wonder then that — as we know from history of physics — the law under discussion was not induced from observed facts at all but was conceived a priori by Galileo. It is only after he had invented his law that Galileo tested its consequences against observed facts (at this point Łukasiewicz makes a reference in his essay to E. Mach's *Die Mechanik in ihrer Entwicklung*, 6th ed., p. 129ff.). This is the role of experience in every natural science: to stimulate novel hypotheses and to be the basis of their testing.

Another form of explanation is the formulation of hypotheses. To form a hypothesis is to assume the existence of an object or phenomenon which has not been ascertained by observation and the existential assumption is made because, in conjunction with other statements, it logically implies the fact to be explained. As examples of hypotheses one can mention the assumption of the existence of the planet Neptune (before it had been observed), the assumption of the existence of atoms and electrons and of the ether; all
paleontology rests on hypotheses which in conjunction with universal statements, usually taken from everyday life, explain the facts of experience, i.e., documents, remains, institutions and habits existing today.

All hypotheses are free creations of our intellect because they refer to phenomena which have not and will not be ascertained by experience. Hypotheses are permanent elements of our knowledge and do not become established truths through verification. For by finding that the consequences of a hypothesis are in agreement with observed facts, we do not turn the hypothesis into a truth.

Logical principles are a priori constructs of our minds and if mathematics is reducible to logic, so is all of mathematics. An examination of mathematical concepts leads to the same conclusion: point, straight line, triangle, all the objects investigated in geometry have only ideal existence and are not given in experience; even less so non-Euclidean figures, irrational, imaginary or complex numbers, integrals, derivatives, etc.

Now the question may be asked: which among scientific statements (judgements) are the results of mere recording of hard facts? If the generalizations, laws and hypotheses and so all empirical theories and also all formal theories are not the results of the recording of facts but the results of creative activity of our intellect, there seems little room left in science for purely factual statements. The answer to this question is apparently easy: a purely factual statement may only be one expressed in a singular statement referring to a fact immediately given in our experience, for example “there is a pine tree here now”, “this magnetic needle is now deflected from its original position”, “there are two windows in this room now”. A cloer analisis of these statements will, however, reveal non-observational, creative elements even in them. “Pine-tree”, “magnetic needle”, “two” are concepts which result from the creative activity of our intellect. Every fact described in words is a product, however, crude, of the activity of the intellect. A bare (hard) fact, unaffected by our mind’s activity is only an idealised concept.

However, science is not exclusively the result of our intellectual activity. In spite of all idealistic systems we feel that there is a reality independent of man and that it must be sought through experience. The problem of determining the role played by experience and by our intellect has always been one of the major tasks of philosophy.10

Two kinds of statements may therefore be distinguished in science: those which are supposed to record facts (though not pure facts) and those which are intellectual constructs. The former are true since truth consists in the agreement between thought and existence. What about the statements of the
second type? We cannot establish definitely that they are false. The constructs 
of our intellect need not necessarily be pure fiction, but there is no ground 
for establishing their truth either. Nevertheless they form a part of science if 
they are suitably related to the statements of the first category and do not 
imply any false consequences.

The aim of science is to construct a system of statements which satisfies 
human intellectual curiosity. The role of factual-reconstructive statements is 
to stimulate intellectual curiosity. The role of the constructive or creative 
elements of science is to satisfy human curiosity. Both kinds of elements 
form a complex whole because they are logically related. At the basis of this 
structure there are statements about singular observable facts; over and above 
them there is the superstructure of the theory which explains, orders and 
predicts facts.

The difference between poetry and science does not consist in that to 
create the former a greater amount of imagination is needed. Copernicus who 
has moved the Earth, Darwin who in the mist of time has discerned the 
evolution of species, deserve to be compared with the greatest of poets. A scientist, 
however, unlike a poet always makes use of reasoning. He cannot and need 
not justify every one of his contentions but he has to relate it with the help 
of logical ties to the structure of science.

Apart from the essay 'Creative Elements in Science', an appendix in his 
Elements of Logic (1929) concerning induction is also relevant to Łukasiewicz's view of science. It is claimed — so Łukasiewicz argued there — that the 
method of natural science is induction, as deduction is the method of mathematics. This view is mistaken. So-called induction is not at all a method of 
reasoning which would legitimise in a clear way the acceptance of a statement 
on the basis of other statements, previously accepted. In spite of the efforts 
of Keynes, Nicod and others, there is no logic of induction. Those who believe 
in induction claim that the rule of incomplete induction warrants the accept-
ce of a generalisation on the basis of singular statements provided that the 
probability of the generalisation, given those singular premises, is greater 
than the probability of its negation on available evidence. However, for such 
a rule to be of any use with reference to scientific inferences, one would have 
to be able to measure the probability of scientific hypotheses and theories 
given available evidence. There is no way of doing this if our theories and laws 
are understood as strictly universal. The same objection applies to all other 
known rules of induction, for example, to Mill's methods. If there is no logic 
of induction, how do we account for the rationality of scientific method? 
Scientific laws are universal statements which are not verifiable by any finite
conjunction of singular, observational statements. Each of those singular statements is, however, deducible from the relevant universal law. Hence universal laws are hypotheses which are tested by singular observational consequences derived from them. The method of science consists in the search for such universal laws which yield observed facts as their consequences. This providing of laws to explain facts is not based on any rules of induction: it is sometimes called reduction or — by Charles Peirce — abduction and is guided by the researcher’s intuition. Although there are no rules for the acceptance of hypotheses, the rules of deductive logic often lead us to the falsification of previously accepted hypotheses; these rules are the modus tollens and the law of transposition. The search for the universal laws of nature could be compared to the deciphering of a coded message when the code is unknown (Appendix para. 11, 191—7).

The relevance of Łukasiewicz’s views reported here to our interest in the genesis of Fleck’s epistemology is this:

First of all, in his criticism of the absolute concepts in epistemology Fleck objected to R. Carnap’s presumed belief in an absolute logic. From the point of view of comparative epistemology, logic — just as other elements of knowledge — should be socially determined, hence, changeable. This seems, however, to be flatly contradicted by the fact that although Aristotle’s 2400-years’ old logic is not identical with the Frege-Russell logic, it is a proper part of the latter. Now Łukasiewicz not only exhibited the tacit assumptions of classical, two-valued logic shared by Aristotle’s and Russell’s system but also proved that they are not absolutely necessary and that by changing some of them one can construct equally consistent and, in a sense, adequate logical systems just as — by replacing the parallel postulate with its contraries — one can construct non-Euclidean geometries. We cannot be certain that Fleck knew of the existence of Łukasiewicz’s many-valued logics. However, the discovery was so widely known in Polish philosophical circles at the time, especially in Lwów and Warsaw, that it would be very surprising if Fleck were unaware of it, the more so that the men whom he was most likely to consult on such matters, viz., L. Chwistek and H. Steinhaus, were both interested in it (in Chwistek’s 1935 Limits of Science there are references to both Łukasiewicz’s 1910 article and to his subsequent research on the foundations of propositional calculus). Assuming, on the other hand, that Fleck was not aware of the existence of Łukasiewicz’s many-valued logics, it is still important to note that the idea of the relativity (conventionality) of logic could be — and in fact was — reached by a route different from Fleck’s socio-historical analysis and that it was not, therefore, seen by philosophers in Poland at the time as unusual.
Secondly, Łukasiewicz's 'creative elements' in science correspond to Poincaré's conventions, and his conception of knowledge is closest to Poincaré's. In fact he explicitly refers at the beginning of his essay to Poincaré's *Value of Science*11 and to Poincaré's polemic with LeRoy there — an important fact since Fleck's 'active connections' correspond to Poincaré's conventions and since LeRoy's conception of a theory is the closest epistemological counterpart to Fleck's closed, irrefutable belief-systems.

Finally, there are certain features of Łukasiewicz's view of science which clearly differ from Fleck's. So, for example, Łukasiewicz believed that science consists of logically interconnected elements even though many of those elements — not unlike works of art — are freely created by our imagination and others, supposed to reproduce observed facts, are moulded by the conceptual systems of our changing language. Moreover, unlike Fleck, Łukasiewicz often refers to all humanity when talking about the aims of science and attributes to its heroes, the prophets, great poets, leading scientists, etc. the progress in intellectual life. When he denied the adequacy of logical analysis in the study of the evolution of scientific concepts and of their interrelations and when he denied individual heroes a significant role in discoveries and in progress, Fleck may have had Łukasiewicz's essay — among others — in mind.


Ajdukiewicz, who, apart from philosophy, had studied in Lwów mathematics and physics, started his philosophical research in the area of the foundations of mathematics. In 1913 he spent one year at Göttingen and heard there D. Hilbert's lectures. Through these lectures he became interested in Poincaré's criticism of Hilbert's formalist philosophy. His 1921 habilitation dissertation was concerned with a formal and pragmatic-methodological analysis of the concept of proof and related foundational concepts.

In the late twenties and early thirties Ajdukiewicz published a series of articles, three of them in *Erkenntnis* (1934–5), in which he presented his conception of language, meaning and the epistemological doctrine which he called radical conventionalism. I shall outline briefly the origins of his views and their main features.

The traditional empiricist view of science (represented, for example, by Newton and Hume) was based on the assumption that our mind is reliable in its cognitive activity only as long as it records facts of experience and that
our language is fixed and plays a passive role in cognition as a means of expressing and communicating thoughts. Hume pointed out that as soon as we transcend facts, as we do when we reason about causal relations, our arguments lack any logical foundation; when we produce ideas which cannot be traced back to sensations, these ideas (for example, the ideas of substance, physical necessity, etc.) lack any sense and are worthless. Hume also assumed that the distinction between so-called matters of fact and relations between ideas is given and fixed — unchanging. Newton claimed that his laws of motion were deduced from facts and made general by induction and one of his rules prohibited explicitly the avoidance of inductive conclusions with the help of alternative hypotheses which our mind could manufacture. These assumptions of traditional empiricism, subsequently reaffirmed by 19th-century positivists like Comte and Mill, were questioned in the second half of the 19th century, especially by conventionalists. Henri Poincaré pointed out that many problems, for example, concerning space and time measurements, which traditionally had been regarded as purely empirical, i.e., decidable by experience and logic alone, are in fact solvable only if some conventions are first laid down. The use of different conventions may affect the solutions of such problems. Hence, geometrical empiricism — by which Poincaré meant the traditional empiricist view of geometry — is untenable. Should experiments contradict the conjunction of Euclidean geometry and optics as part of our system of physics, it is up to us to decide whether to abandon Euclidean geometry or to modify optics. Experience and logic alone do not impose any unique solution. Poincaré also pointed out that although the assumptions of pure, metric geometry originated as empirical generalisations, eventually they have been elevated to the status of terminological conventions; as empirical generalisations they had been subject to empirical revision; as conventional principles they were no longer reversible. Similar changes in epistemological status affected other statements, some of them in physics, for example, the law $\mathbf{F} = m \mathbf{a}$ may be used as a definition of 'force' and the principle of the conservation of energy may, similarly, have a conventional status. Nor are these changes, in turn, irreversible: a geometrical axiom may be given an empirical status, if we so decide. The epistemological lesson, according to Poincaré, is this: Contrary to Hume, there is no fixed boundary between statements of fact and relations between ideas (conventions, in Poincaré's terminology). The same statement which at one point in the history of science is factual, at another may be conventional and vice versa. The status of a statement does not depend on the statement itself but on the role it plays in the system of science at a given moment or period of time. Scientists actively
intervene and assign the epistemological status to statements. This means, however, that unless there is universal consensus on these matters, one can — theoretically at least — defend any statement whatever in the face of apparently contradictory evidence. Refutation, for example through so-called crucial experiments, is possible only if there is agreement on certain conventions and on the decision not to manipulate our language. As we know, observations similar to Poincaré’s, over two decades later, led Quine and others (who, however, never acknowledge Poincaré’s insight) to the rejection of ‘the empiricist dogma’ according to which all statements may be classified into synthetic and analytic. Ajdukiewicz, three years before Quine’s first (pre-war) paper on the subject (‘Truth by Convention’), came to the conclusion that the distinction can be retained and the changeability of epistemological status pointed out by Poincaré accommodated, provided one makes use of a suitable conception of language and meaning. A few years later he also showed that Quine’s position, according to which there are no analytic statements, may also be accommodated within this framework if it is restricted to a special class of languages (Ajdukiewicz languages without axiomatic meaning-rules). Before we discuss this, however, another link should be mentioned. Poincaré’s conventionalism was of a moderate type. He believed that there are so-called ‘bare facts’ which remain unaltered (or rather, empirical laws as generalisations of such facts) under the changes of conventions. This was denied by E. LeRoy, a follower of Poincaré and Bergson. In ‘Science et philosophie’ (Rev. de M. VII, 1899) and in ‘Un positivisme nouveau’ (Rev. de M. IX, 1901), LeRoy argued as follows: The traditional empiricist and Comtian positivist philosophy of science contrasted ‘positive science’ consisting of facts, with theories and hypotheses. Facts were claimed to be objective, independent of theories and simply discovered and collected by impartial observers. However, Claude Bernard had pointed out already that this was a naive view of science since abstract concepts play an important role in the study of even the simplest facts (SP, 513). A visit to a scientific laboratory would show that there are no bare or pure facts and that scientific facts are created by scientists from an amorphous material of experience, (SP II, 515; PN, 145). Reality is accessible to an observer only through the mediation of conceptual forms or schemes which are contingent on our past experience as individuals and as a race, on our aims and prejudices as men of action, on everything life has imprinted on our minds (SP. II, 516).

Ajdukiewicz, stimulated by Twardowski’s views concerning the active role of language and by Łukasiewicz’s article on the creative elements in science, took LeRoy’s side in the dispute, denying the existence of ‘bare facts’ as
invariant under the change of conventions and arrived at the conclusion that he could express the insights of Poincaré concerning the changes in epistemological status and LeRoy’s claims concerning ‘facts’ perhaps better in certain respects, in another way provided that one is willing to operate with suitable concepts of ‘meaning’ and ‘language’, more rigid perhaps but also more precise than the popular conceptions. Let us look he wrote, at a language spoken by community as consisting not only of a vocabulary and syntax but also of a system of dispositions to accept (or reject) the sentences of the language under specified conditions. So, for example, if one has a toothache one would be normally disposed to accept in English the sentence ‘It hurts’; again, if one has accepted the sentence ‘a is greater than b’, where a and b are numbers, then one is normally prepared to accept also the sentence ‘b is not greater than a’. Finally, when talking about numbers, one is usually expected to accept unconditionally that ‘for every x, x=x’ or — when talking about Euclidean configurations — one is expected to accept unconditionally the parallel postulate, among other things. Refusal to accept these sentences under specified conditions would indicate that one is not speaking English, or the language of arithmetic or of Euclidean geometry. Naturally, there are statements (especially in an empirical language) whose acceptance is not governed by language rules; all hypotheses are of the latter type. Assume, therefore, that the languages which we consider are defined not merely by their dictionary and syntax but also by a system of rules of the style illustrated, viz., empirical, deductive and axiomatic meaning-rules. The meaning-rules in question are relations which coordinate with every sentence of the language some conditions for its acceptance, i.e., either certain data of experience, or other sentences, or both or the empty set. With the help of the dictionary, the syntax and the meaning-rules one can set up for a language of the type under discussion a matrix which will characterise the language. Every expression of the language, every sentence in particular, can occupy only certain definite allowed positions within the matrix. Two expressions are then defined as synonymous (having the same meaning) iff their interchange does not alter the matrix of the language (except perhaps for the order of its lines) i.e., if they are isotopes in the matrix. The meaning of an expression of the language is defined as the ordered pair, the first element of which is the matrix of the language and the second the equivalence class of positions which the expression is allowed to occupy within the matrix (the relation of having the same meaning is an equivalence relation, hence it defines equivalence classes of positions).

Two points should be emphasised at this juncture: (1) although in the late
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In his examination of language changes Ajdukiewicz distinguished those which involve the introduction of new expressions, not synonymous with any existing already, and those which do not involve new expression. Moreover, he distinguished changes which do not alter the meaning of expressions already in the language and those which do, where change of meaning was understood as change of the language matrix (or, equivalently, the change in the total scopes of language-rules; a total scope of a rule is the set-theoretical sum of the scopes of rules of the same type). Without the introduction of a new expression a language may change, for example, when an empirical generalisation has been elevated to the status of a conventional postulate (Poincaré's example). In this case, according to Ajdukiewicz's conception of meaning, the matrix of the language is altered (the total scope of language rules) since a statement originally governed by an empirical rule, or one which had the status of an hypothesis, has been transferred to the scope of some axiomatic meaning-rule. It goes without saying that, consequently, we no longer deal with the same statement: while an empirical generalisation was revisable, it is logically impossible to reject a conventional principle since this would alter the meaning of expressions in it. Consider now a change of language in which a new expression is introduced; this may occur either without the meaning of existing expressions being altered or with the meaning-change of other expressions. According to Ajdukiewicz, there are two types of language which behave differently in this respect, viz., open and closed languages. A language L is said to be open with respect to another L' iff L' contains all the expressions of L with the same meaning and also some other expressions, not present in L (and such that at least one of them is meaning-related to
some expression in \( L \). A language \( L \) is simply open iff there exists an \( L' \) with
the mentioned properties. A language which is not open is closed. An open
language can be enriched with new expression without the meaning of existing
expression being altered thereby. On the other hand, when a closed language
is enriched with a new expression (not synonymous with any existing), then
either the meaning of some expressions in the language is necessarily altered
or the language becomes disconnected, i.e., it splits into two (or more) closed
languages, unrelated with each other. Ajdukiewicz then proved that two
closed and connected languages are either completely intertranslatable (their
matrices are isomorphic) or have absolutely no expressions (with the same
meaning) in common. The epistemological doctrine of radical conventionalism
affirms, firstly, that all empirical problems, including decisions on so-called
facts, involve conventions, hence their solutions are not uniquely imposed
on us by experience, secondly, that since knowledge, scientific knowledge
especially, has to be expressed in some language and since there exist closed
and connected languages which are not intertranslatable, there may exist
scientific theories which are not comparable on logical and empirical grounds.
As examples of such theories Ajdukiewicz mentioned relativity and quantum
mechanics with respect to classical mechanics. This was the second claim —
after LeRoy’s — that there are from time to time disruptive changes in science.
Both were made within the conventionalist tradition. In general, according
to Ajdukiewicz, a transition to a new language — not intertranslatable with
the old one — occurs if within the old language a contradiction should arise
between two sentences whose acceptance is required by some meaning-rules.
Such a contradiction cannot be resolved within the old language since its
rules do not allow the rejection of either of the two contradictory sentences.
It cannot be resolved by a transition to a new language, intertranslatable with
the old one, because then the contradiction would be present also in the new
language. If, for example, in the emergence of special relativity the impossibility
of measuring the absolute velocity of the earth (originally an empirical
generalisation) has been raised to the status of a new principle, the special
principle of relativity, which contradicts the assumption of classical mechanics
that absolute velocity (with respect to the ether) is measurable, then this
contradiction could only be resolved by abandoning the language of classical
mechanics and the transition to a new language (of relativity) into which no
classical expression is translatable. It should be noted that for Ajdukiewicz,
unlike for contemporary proponents of the incommensurability thesis
(Feyerabend, for example), the claim of the logical non-comparability of
classical and relativistic mechanics, was conditional, not absolute; for should
the contradiction in point occur between two statements both of which are
empirical hypotheses, i.e., not dictated by meaning-rules, then relativity
theory could emerge from the critique of classical mechanics by the abandon-
ment of relevant classical hypotheses, without the necessity of a transition
to a new language, not translatable with the original one. In this case the
change would involve only an enrichment of the old language with new ex-
pressions and some minor changes within the existing language which could
not alter the meaning of old classical expressions. The question which of
these two interpretations fits better the historical case of relativity vs.,
classical mechanics, is not easy to answer and certainly there is no mechanical
way of doing this. The answer depends, among other things, on how the
languages of the two theories are reconstructed in terms of meaning-rules.

To sum up, Ajdukiewicz affirmed, with Poincaré and LeRoy, that experi-
ence can compel us to accept a certain statement (with a definite meaning)
only as long as we use a fixed, unchanging language. But a language usually
changes. Unlike French conventionalists, Ajdukiewicz proposed to study
changes in science with the help of a sequence of languages with fixed mean-
ing-rules. This would make it possible to distinguish empirical changes from
language changes (which possibility Quine denies, since he considers ordinary
language) as well as intertranslatable languages from those which are not
intertranslatable (are ‘incommensurable’ in present terminology). The concept
of truth, if used at all, would have to be defined separately for each of the
languages which are not intertranslatable. This does not mean that ‘truth’
would be relative. For relativity of truth implies that there exists a statement
which in one language is true whereas the same statement in another language
is false. Rather, it means that one could avoid accepting as true a statement
by abandoning the language in which it occurs (and is governed by some
meaning-rule) and adopting another language in which the same statement
no longer occurs.

If we now consider the relevance of Ajdukiewicz’s epistemology to Fleck’s
views, the following points should, perhaps, be made: (a) Fleck’s thought-
styles may be seen as psychological and sociological counterparts of Ajdukie-
wicz’s not intertranslatable languages (conceptual apparatuses and linguistic
world-perspectives). Since a language, in Ajdukiewicz’s sense, is not a product
of an individual but of a community and yet an individual has to think and
communicate in some language, the individual’s thinking is thus made pos-
sible and constrained by the community whose language is being used. (2) all
so-called facts, especially scientific ones, are created — as are theories — and all have their histories, not only according to Fleck, but also according to Ajdukiewicz and the French conventionalists. (c) Fleck’s psycho-sociological account of the tenacity of mature belief systems may be seen as having a logical or epistemological justification in the conventionalist criticism of traditional empiricism and in the conventionalist thesis to the effect that experience can enforce unique solutions of problems only within a given and fixed conceptual framework (apparatus) and that since various elements of the conceptual frameworks can be altered, i.e., one can change one’s language, it is always possible to avoid the acceptance of a specific statement as true (by changing language) or to defend against falsification any part of one’s belief-system. (d) Ajdukiewicz operates in his comparative epistemology without the concept of truth: statements are accepted or rejected in accordance with meaning-rules or as hypotheses; the task of an epistemologist is — according to him — not to pass judgements as to which of rival or succeeding conceptual systems is true or false but to study evolutionary trends or to provide a humanistic (understanding) explanation; Fleck’s position is similar except that he wants his epistemologist to be a sociologist.


My third case is a joint work of two members of the Warsaw School, Poznański and Wundheiler, both former students of T. Kotarbinski. Their article, entitled ‘The Concept of Truth in Physics’, is an interesting anticipation of the holistic view of science, now usually attributed to Quine. As in Quine’s case, it was inspired, to some extent at least, by Pierre Duhem and his famous conventionalist thesis. It is also a good illustration of the variety of philosophical views within the Warsaw School, as one of the main points of the article is the criticism and rejection in the area of empirical science of the absolute or classical concept of truth, whose famous definition Alfred Tarski had given in 1933 (in Polish; German translation in 1935). Another point was to argue that a physical theory has a structure and properties completely different from axiomatic theories in mathematics, a claim one would not expect within a logic-dominated school of thought. Here are some of the central claims of Poznański and Wundheiler.

The traditional view according to which truth is absolute in the sense that
it is independent of the knowing subject, of the truth of other statements and of the state of science at the given time, is inapplicable to physical theories. It has to be given up like other absolute concepts such as that of absolute space, time, etc. Likewise one has to give up the view which pictures a scientific theory in analogy to a deductive system with statements classified into elementary and non-elementary, the former being intuitively evident, accepted independently of others and serving as the basis for the rest of the system so that any circularity in validation can be avoided. In contrast to this view the authors claim that all statements about objective phenomena can be questioned and the validation of each appeals to other statements in the system. So-called elementary statements about pointer-readings, coincidences, results of counting, etc. are no exception. Every statement in physics is an element of a connected system and can only be said to be true or false jointly with the rest of the system. The truth or falsity of an isolated statement can never be determined.

Since even elementary statements are not evident, they may be questioned and tested. This is done by examining the reliability and competence of observers and of the instruments they use. In theory such a process could be carried on indefinitely, though this is obviously not the case in practice. The testing of non-elementary statements is even more complicated. What physicists call a 'simple fact', e.g. the result of the measurement of length, force, of electric current, etc. is, in reality, a very complicated affair whose testing involves the acceptance of a sizeable part of physics.

General hypotheses can never be definitely verified by facts, though they can be falsified. This claim requires, however, an essential qualification. As Pierre Duhem pointed out (La théorie physique, 1906, Ch. VI) an experiment never falsifies or confirms one single hypothesis but always a group of statements, one of which is the hypothesis we want to test. This should be obvious from the fact that, firstly, to deduce from a hypothesis testable consequences we need auxiliary assumptions; secondly, to interpret the reading of instruments as a physical fact relevant for the testing of our hypothesis we always implicitly assume several laws and theories (as someone said: instruments are 'frozen theories' — 'geflorene Theorien'). Consequently, should the verdict of the experiment turn out to be negative for our hypothesis, we can always save it — theoretically, at least — by modifying some of the auxiliary assumptions, necessary for the deduction of the consequences or for the interpretation of instrument readings as relevant facts. For example, to test Newton's gravitation theory we appeal to astronomical observations; however, these presuppose the laws of optics; hence, a negative outcome of an experiment
may be blamed either on the gravitation theory or on optics. The conclusion is, that we can never verify or falsify any isolated statement in physics; at most, we can conclude that a system of statements is jointly true or jointly false. In physics (and other empirical sciences) as H. Weyl said “Die Wahrheit bildet ein System” (Philosophie der Mathematik und der Naturwissenschaften, Handbuch der Philosophie, München-Leipzig, 1927, p. 111).

Unlike in a mathematical deductive system, in a physical theory one cannot order statements either with respect to whether they serve as first premises or with respect to the type of statements on which their validation rests. In the validation of abstract, theoretical hypothesis we rely on both elementary and non-elementary statements; in the validation of elementary statements we have to appeal, explicitly or implicitly, again to laws and theories as well as to other elementary statements. Consequently, whereas an axiomatic theory in mathematics may be diagrammatically represented by a pyramid, with a few axioms (or axiom-schemata) at the top, and spreading downwards, so to speak, the diagram of a system of physics would be a net, similar to a spider’s web but more complex, more dense in some parts than in others; each statement is connected in all directions with other statements; there is no sub-set of designated statements corresponding to the axioms of a mathematical system. In contradistinction to proofs within an axiomatic system, the process of verification or falsification is neither uni-directional nor terminable. We may wander throughout the web without having the right to stop at any definite node for a final rest. The saying, quoted previously, that truth constitutes a system should be understood in the sense that what we manage to establish in our validation procedures is the consistency or coherence of the system at the moment, the conformity of laws and theories among themselves and with available evidence. A systematic validation of any statement would involve the whole system, though in practice we rest contented with surveying a small part of it.

Sometimes the process of validation, even though it involves only a small part of the system, does not terminate. This occurs in those cases when after a number of testing steps we return to the statement which was our starting point. Such testing procedures are cyclical; they would be represented within the web-diagram by loops and are most common when the statement under test is theoretical. Such a procedure is obviously analogous to what in logic is called a vicious circle in a proof. A circular or cyclic procedure, though forbidden in deductive theories, in physics is not only regarded as legitimate but is an essential instrument for developing our physical knowledge (Cf. A. Eddington: The Nature of the Physical World, 1928, p. 260). Cyclical
procedures establish within the whole system of physics quasi-isolated subsystems. Most common perhaps are such cyclic systems in optics, but they abound elsewhere also. By analogy to implicit definitions of primitive terms in mathematics, we can say that the truth of physical statements can be established only implicitly, through coherence with the whole system. Finally, the existence of cyclic procedures indicates that there may be two mutually inconsistent cycles, hence two mutually inconsistent ‘truths’, each saving the phenomena equally well, to use the ancient phrase. Strictly speaking, this would be an idealisation, since most alternative cycles in physics differ somewhat with respect to their properties and some are somewhat better than others.

Coherence or system-adequacy (Systemfähigkeit) is not the only criterion of truth in physics. The other, complementary criterion is consensus of competent and reliable scientists, particularly often used in relation to elementary statements but, in general, applied in order to terminate otherwise interminable testing procedures. The importance of the second criterion was particularly stressed by Campbell (Physics. The Principles, Cambridge, 1920, p. 29) when he said: “The subject matter of science consists of those judgements for which universal assent can be obtained”. If inconsistency should occur between some statements based on consensus and other statements in the system, then it can be resolved in many different ways, either by revising some of the statements in the system or by examining the circumstances under which consensus was obtained. (Among the laws which govern consensus are the following: (a) Only people with normal sense-organs, sufficiently intelligent and disinterested qualify as respondents for consensus, (b) there is consensus on elementary statements, (c) there is consensus on logic.)

In effect, to say of a statement in physics that it is true means either that it is consistent (coheres) with the system or that consensus has been obtained respecting it. ‘True’ may, therefore, be replaced — so far as physics is concerned — with ‘accepted at the time’ or ‘part of the system’. It should be noted that when the system of physics changes, many of the statements remain. Those statements may be said to be relatively independent of the cognitive subject or of the state of science or of time. The outlined concept of ‘truth’ has the advantage that, firstly, it is easily applicable to science as it actually exists, secondly, that it is — unlike the absolute concept of truth — an operational concept. Its definition rests on the examination of the procedures (operations) actually used to decide whether or not to accept a statement as part of the system of physics.
If we look now for parallels with Fleck's epistemological views, the following points seem to be worth noting: (a) As in the previous case, we have here the conventionalist view (this time inspired by the Duhemian thesis) of science as a closed system which can adapt in various ways to new results of experience; consequently, any part of the system may be defended against falsification. (b) As in Fleck (1935, 1980: 44) some methods of validation within the system of science (physics) are claimed to be circular, i.e. have a property, analogous to a vicious circle in a proof which, from the epistemological viewpoint, results in a *petitio principii*. However, the authors go beyond the position taken by Fleck and claim that — unlike in axiomatic systems — cyclic (circular) procedures are valuable in physics. (c) As in Fleck's epistemology, the absolute concept of truth is replaced by the idea of scientific consensus and system. (d) Elementary statements are claimed to require laws and theories for their validation and so-called simple facts are shown to be complex and theory dependent.

4. CONCLUDING REMARKS: THE RECEIPTION OF FLECK'S THEORY

Failure on the part of Fleck's contemporaries in Poland and abroad to appreciate his theory of thought-styles and thought-collectives was due, one may conjecture, to several reasons and causes. Its radical departure from the prevailing logical-empiricist philosophy of science must have been partly responsible but should not be overestimated as a factor in itself. As has been shown in previous chapters, the philosophy of science in Poland in the interwar period was neither predominantly nor dogmatically logical-empiricist in character, contrary to popular belief. In particular Lwów — where Fleck lived and worked — was in the twenties dominated by Twardowski (who scorned 'logical symbolomania') and in the thirties by Ajdukiewicz (whose radical conventionalism itself differed considerably from both the Vienna and Warsaw empiricism), with Chwistek doing his best to upset any philosophical orthodoxy, especially the one 'made in Warsaw'. It was rather Fleck's decision to publish his 1935 book in German rather than in Polish and in Switzerland rather than in Poland, that made its 'style-inadequacy' relevant to the negative reception of the book in European philosophical centres before World War II. On the other hand, that same decision proved to be responsible for the later effect it had on Kuhn's 1962 *The Structure of Scientific Revolutions* and for the revival of interest in the theory it contained. As regards Polish sociological community in the inter-war period
(especially Florian Znaniecki in Poznan and the Ossowskis in Warsaw), there does not seem to be any evidence of Fleck ever attempting to communicate with it or to elicit its support.

If seen from the perspective of the philosophy of science in Poland in the nineteen-thirties, Fleck’s theory of thought-styles and thought-collectives would have been evaluated in, roughly, the following way:

(i) The epistemological content of Fleck’s (1935) book is not new when considered against the background of Polish philosophy in the thirties. It was anticipated in some respects in Jan Łukasiewicz’s early philosophical writings, especially in his (1910) article and in the Appendix to his (1928), in other respects in Kazimierz Ajdukiewicz’s radical conventionalism and in the Poznański and Wundheiler epistemological analysis of physics. This applies especially to the criticism of traditional empiricism, to the idea of thought-styles and their tenacity, to the distinction between the active and passive elements in knowledge etc. (Cf. points (a), (d) and (e) in the short account of Fleck’s epistemology at the beginning of our first chapter). In other words, philosophers affected by the conventionalist tradition would be in agreement with those components of Fleck’s epistemology but would find little to learn from him; those, on the other hand who reject conventionalism and related views would also find his writings unconvincing.

(ii) Fleck’s epistemological naturalism on its own, i.e. the view that epistemology should be part of empirical science, is not — in principle — objectionable to the majority of the philosophers of science in Poland as it is not to those in Vienna. Evolutionary epistemology and a pragmatic approach to the methodology of science are integral parts of Kotarbinski’s and Ajdukiewicz’s philosophy. However, Fleck’s apparent dislike of abstract (logical) models of science and its changes is unjustified since such models form an important part of the naturalist approach to science, at least they do if one takes physics rather than medicine as the paradigm of natural science. Moreover, Fleck’s epistemological naturalism, fashioned after Durkheim’s positivist exemplar, is too restrictive if it excludes the methods of “understanding epistemology” (strongly emphasised, for example, by Ajdukiewicz). Finally, should his naturalism allow only “natural history” type of analysis of science, then again this would be unnecessarily restrictive.

(iii) Though scientific knowledge is produced within social collectives, its products viz. theories and conceptual systems, can be studied not only with the help of sociological methods. Sociology of knowledge, though relevant to epistemology must not be identified with the latter, as Fleck seems to be doing. Such identification is particularly objectionable if it is
based on so called “strong thesis of the sociology of knowledge” according to which since knowledge is a social phenomenon it is merely part of ideology (in Mannheim’s sense of “total ideology”). If ‘ideological’ here means ‘distorted’ (hence ‘not true’), or if it means ‘merely expressive of the existential basis prevailing at the time’, then the strong thesis is either paradoxical or descriptively irrelevant (since it refers to itself). Perhaps, however, Fleck does not hold the strong thesis of the sociology of knowledge but rather the following doctrine (expressed in Section 4 or Chapter 2 ‘Introduction to Thought-Collectives’): Knowledge is socially determined in the sense that it can only be understood with respect to its content and evaluated with respect to its validity in the historical context of the thought-collective which is its bearer. This historist-relativist doctrine seems to be a stronger version of Ajugkiewicz’s thesis of radical conventionalism which implied the existence of not inter-translatable conceptual systems. Therefore all the criticism levelled at the latter would equally apply to the former. Moreover, rather than maintain the relativity of truth (as Fleck does) which — given the classical conception — results in confusions, it is more satisfactory to maintain the absoluteness of truth and to emphasise — as Ajugkiewicz did — the relativity of meaning. The identification of ‘truth’ with ‘confirmed by evidence at time t’ (as Poznański and Wundheiler do with respect to physics), amounts to a preference for a non-classical conception (in terms of coherence or of constructive-effective procedures).

(iv) Fleck’s collectivist view of scientific discoveries, presumably modelled on discoveries in biology and medicine, does not square with all historical evidence. As regards the history of physics, though it is in good agreement with the discoveries in quantum theory, it is difficult to reconcile with the established account of the discovery of relativity theory (as it came to be seen in the twenties and thirties). The onesidedness of Fleck’s reliance on biology and medicine as paradigms of scientific inquiry is further illustrated by the fact that his account of the style of publications in “journal science” (as cautious, modest and undogmatic) does not fit at all the style of Einstein’s 1905 paper on Special Relativity, the object of innumerable epistemological and historical analyses over the last seventy five years or so.14

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NOTES

1 Like Fleck, some of them mistakenly identified conventionalism with the claim that
all assumptions in science and mathematics were absolutely arbitrary, Cf. J. Łukasiewicz: 'In Defence of Logistic' Łukasiewicz (1970: 244–6).


3 Since the word “Koppelungen” which occurs in Fleck’s definition of ‘knowledge’ is not a common expression, its use may have relevance for the question of the origin of his epistemological ideas. In The Principles of Mechanics (1894) Heinrich Hertz represents the action of forces between systems in terms of connections or couplings, “Koppelungen”, between them so that the observed motions appear as constrained and obeying the law: from among the movements compatible with the connections those are realised for which the sum of masses multiplied by the square of accelerations takes the minimum value (this law is equivalent with the law of least action if the system is holonomic). Hertz’s foundations of mechanics were subjected to an epistemological analysis by Henri Poincaré in Les idées de la Hertz sur la mécanique (1897) part of which was reprinted in Science and Hypothesis (1902). On the basis of one of G. Koenig’s theorems in the kinematics of articulated systems (systemes articulées, Gelenk-systeme) Poincaré shows there that with any mechanical explanation one is always given infinitely many, a result of great importance for the conventionalist view of science. Hertz’s idea of “Koppelungen” between mechanical systems and its relationship with the multiplicities of “dynamical models” for any material system were reported by F. Lindemann in his annotations to the German translation Wissenschaft und Hypothese (1928: 329) of Poincaré’s work. Fleck’s source could have been either Hertz’s original or Lindemann’s annotations to Poincaré, though he does not refer to either.

4 This applied also to most philosophers in Poland in the interwar period, with the exception of K. Ajdukiewicz; on this last point and the misinterpretations of conventionalism in general, see Giedymin, Science and Convention, especially Preface and Essay 1, ‘On the Origins and Significance of Poincaré’s Conventionalism’.

5 They comprised, apart from the already mentioned scholars, a number of former students of T. Kotarbinski (e.g. D. Sztejnharg-Kotarbinska, S. Ossowski, M. Ossowska, A. Wundheiler, A. Poznanski) and of K. Ajdukiewicz (M. Kokoszyńska, I. Dambka, S. Łuszczewska-Rohmanowa).

6 For an account of Twardowski’s philosophy, see S. Łuszczewska-Rohmanowa; 1977, pp. 86–125, see B. Skarga (ed.) (1977).

7 Hugo Steinhaus was a friend of both, Chwistek being married to Steinhaus’s sister, Olga. My attention was drawn to this fact by Thomas Schnelle in a private conversation in February 1981. Chwistek wrote one of the first (favourable) reviews of Fleck’s (1935) book and in Chwistek (1935, 1948) there is a reference to Fleck’s (1929) article.
For an account of the views of some of the mentioned, see Polska Mysł Filozoficzna i Społeczna (ed. by B. Skarga), Vol. 3, Warszawa, 1977, Książka i Wiedza.


At this point Łukasiewicz refers to Kant in the following passage: "The Copernican ideas of Kant who tried to show that it is rather the objects that are being accommodated to the cognitive process and not vice versa, are in good agreement with the claim that there are creative elements in science. However, I attempted to establish this claim independently of any particular epistemological theory solely on the basis of common sense realism and with the help of logical analysis" (1912, 1970: 13, footnote 21).


For a more detailed account of Ajdukiewicz's views, see Giedymin (1978).

The fact that Kuhn (like others) has been unaware of this important component of the conventionalist philosophy, is probably due to the fact that he looked at 20th-century philosophy of science through the positivism-antipositivism stereotype.

Since critics of Edmund Whittaker's account of the discovery of special relativity have been unanimously defending the traditional discovery - by-one-man-view, one may conjecture that Fleck's collectivism would no more recommend him to historically minded philosophers of science in the 70's and 80's than it did to the logical empiricists in the 30's. My own view on the matter (expressed in Giedymin, 1982, 6th essay) in terms of simultaneous discovery by Einstein, Lorentz and Poincaré, is in conformity with Fleck's and Robert Merton's account of scientific discoveries.

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1. There is one thing that is certain: the ideas of Fleck, as fresh and original for his time as they were, were passed by unnoticed by Polish philosophy. The question asked here is: Why?

I have been reflecting on this question for some time. Polish philosophy in the twenties and thirties meant primarily the imposing Lwów-Warsaw school — a logical-philosophical school of international stature. There must have been something in Fleck’s thought which was somehow alien and alienating to this school. But what was it? My first answer was: his epistemological relativism! By this I mean the notion that no one style of thought could be designated as the correct one; they are all equally valuable, or at least — equally justified.

There are several circumstances which support this explanation of the cultural-historical phenomenon which I should like to call the ignoring of Fleck. Twardowski — the founder and head of the aforementioned school — spoke out explicitly against this kind of relativism (in the influential essay of 1900 ‘On So-called Relative Truths’). The same viewpoint was held by Kotarbinski, Twardowski’s successor as head of the school. And in the only essay in which any notice is taken of Fleck — i.e., in the 1927 essay by Twardowski’s pupil Isidora Dambska ‘Is an Intersubjective Similarity of Sensations an Unavoidable Presupposition of the Natural Sciences?’ — precisely this relativism is held against him, and his entire theory is simply rejected as incorrect on that account.

But this obvious explanation of the ignoring of Fleck appears on closer examination not to be the correct one. Relativism was certainly one reason why Fleck was rejected, but it was not the reason and not even the major reason for this rejection. For Ajdukiewicz, one of the three or four major figures of the Lwów-Warsaw school, was also working in Lwów at the same time as Fleck, and he also held a kind of epistemological relativism, which he described as ‘radical conventionalism.’ In 1934 — thus almost simultaneously with Fleck’s book — Ajdukiewicz published his widely-read essay ‘World View and Conceptual Apparatus’ in Erkenntnis, the journal of the Vienna Circle. We read there: “the scientific world view is conventional down to every detail, and can be changed by an appropriate change in the conceptual

apparatus. . . . Each of these scientific world images can therefore with the same right demand to be recognized as true." If we replace the words 'conceptual apparatus' in this statement by Ajdukiewicz with the term 'thought-style', it could just as well have been taken from Fleck's book as from Ajdukiewicz. And yet Ajdukiewicz was not ignored; on the contrary!

2. The second explanation which I thought of for the puzzling phenomenon was not of an epistemological but of a social-political nature. The spirit of the Lwów-Warsaw school was thoroughly liberal. An essential part of the political liberalism was the firm belief in a fundamental human rationality. The world is no tower of Babel: in the final analysis, we can always come to an agreement with one another and solve problems together. And this is actually quite simple: we need only take each other into consideration and discuss things with one another patiently and rationally. Mutual understanding then arises automatically. (Or as J. M. Keynes said of Bertrand Russell, an idol of the school: Bertie entertains two ludicrously incompatible opinions. He believes, on the one hand, that the affairs of men are conducted in an utterly irrational way. And on the other he thinks there is a simple remedy for that: namely, to conduct them rationally!)

Now this liberal-rationalistic belief in a fundamental possibility of agreement is questioned by Fleck. There are various styles of thought, i.e., there are groups of people and areas ('thought-collectives') between which no possibility of agreement exists. Patience and discussion do not help here, for what they produce is at most an appearance of agreement, where the same words have a different meaning, and thus hide the opposition and slumbering animosity of the ways and styles of thought.

The liberal conscience refuses to recognize such a state of affairs. And since it cannot argue well against it, it represses it. The ignoring of Fleck could therefore be described as a case of social-psychological repression, and thus provide an excellent example for his own theory. That is to say: the styles of thought of Fleck and the Lwów-Warsaw school were simply different. And with reference to thought styles, 'different' means divorced from one another — by a break which can only be overcome by the spirit of the times which allows the genesis of a new, broader thought-collective.

3. The socio-psychologistic explanation for Fleck's being ignored by Polish philosophy may sound very plausible. And yet it can hardly be satisfactory. For it gives a cause for this lack of attention, but no reason! Of course, if one first equates all styles of thought in a relativistic fashion, one can no longer ask for such a reason. Reasons would then be internal to the style, as it were:
they would be considered valid only within the same thought collective. This relativism, however, I do not share.

I believe that those of the Lwów and Warsaw schools had their reasons — aside from their particular inclinations or aversions. Although they did go too far in their ignoring of Fleck, they could have justified doing so to a certain point. This is related to the aforementioned international stature of the Polish school. For its logical-methodological standard — of that which it considered worthy of discussion in epistemology — was uncommonly high. This sometimes resulted in a subtlety (Spitzfindigkeit) that looked almost like a logical neurosis. Such abuses of the logical standard however must not dim our vision to that in it which is of lasting value. Fleck's writings could not satisfy this logical standard, due to the fact that his ideas were not yet ripe enough to do so. And this is the major reason for their remaining so widely ignored in Poland.

I would like in closing to support this with an example.

4. The central concept of Fleck's theory is without a doubt that of thought-style. But how is this defined? In Fleck's book we find an explanation (p. 130) that he calls a 'definition': "Style of thought is directed perceiving." If this is supposed to be the definition of a major concept, thus of the cornerstone of a theory, it is quite worthless. For in the first place, it is a disguised circular argument: the concept of 'being directed' which it uses is not explained in its application to perceiving, and is thus as questionable as that of the style of thought itself. 'Directed perceiving' is for Fleck, 'style-adequate perceiving' (stilgemaß wahrnehmen). We thus have a definition in which 'style of thought' is defined as 'perceiving in accordance with a thought-style' — an obvious case of a vicious circle. In addition to this, this definition does not agree very well with the rest of what we read in the book about styles of thought. It seems to be too narrow in its limiting that which is in accordance with the style of thought to perceiving. And what should we then say about construction of theory? There is more of such sloppiness in Fleck, but we do not want to be petty here. We are not concerned with the words, which may be sloppy, but with the thing, which is worthy of reflection.

What is a style of thought? As opposed to the 'thought-collective', the former is not a sociological concept. In fact, the thought-collective can only be defined on the basis of the thought-style. The first comparison which arises here is that to language: styles of thought are like languages. Styles of thought can be incompatible (unverträglich) with one another; languages can be untranslatable one to the other. And both are structures (Gebilde) which have grown historically and are not arbitrarily made. (One grows into
a style of thought as one grows into his native tongue.) But languages and styles of thought are not only structures which have grown; they are also formally structured. Both have a logic: each language has a consequence relation (if one says A, one must say B; "the first we are free to do or not, to the second we are slaves"), each style of thought has a compulsion of thought (Denkzwang).

How could this be described exactly? According to Fleck, each theory is embedded in a thought-style, but one can just as well say: each thought-style contains a theory as that part of it which can be articulated (the rest, which cannot be articulated, is then that which he calls being experienced (Erfahrensein), 'know-how')

Fig. 1.

The thought-style is something difficult to describe, but with theories the matter is simpler. One could also try to describe thought-styles indirectly, through the theories embedded in them. (As one can characterize the consequence relation of a language in formal logic by means of the totality of the deductive systems belonging to it.) This would lead then for example to the following questions.

Let $D_1$ and $D_2$ here be different thought-styles, and $T_1$ and $T_2$ be the corresponding theories embedded in them. Even within this simple framework we can ask questions which are not wholly trivial. The following case, e.g., can occur:

Fig. 2.
The relation between thought-styles appears here indirectly in the following way i.e., by means of the relation of the theories. The center is comprised of the mutually recognized axioms of the same area of concern (Sachgebiet). The center part of T₁ is comprised of propositions which in D₂ appear as false, but still as meaningful. The left part of T₁ finally, is comprised of propositions which in D₁ are considered true, but which in D₂ are rejected and condemned as meaningless nonsense.
ŁÓDZ AS CULTURAL AND INTELLECTUAL
BACKGROUND OF THE GENESIS OF FLECK'S IDEAS

In the name of both my Polish colleagues and myself, as the secretary of the First Division of the Polish Academy of Sciences of Social Sciences, I should like to take advantage of the honor granted me in presenting the opening talk to extend my most heartfelt thanks for the invitation to participate in this undertaking to the organizers of the Ludwik Fleck colloquium.

I have purposely introduced myself as the director of Poland's largest research institute for social sciences. I find it simply peculiar to appear here as a guest instead of an organizer. We can all the more appreciate the courtesy of the German organizers, in particular of Professors Lothar Schäfer and Thomas Schnelle, who allowed a representative of Polish science to open this conference.

This is truly a phenomenon demanding its own sociological analysis, and probably one by political science as well: how could Ludwik Fleck's existence and the bountiful fruits of his research have been so quickly and thoroughly forgotten in his own country? After contacting Dr. Schnelle I consulted our specialists — doctors, biologists, philosophers and sociologists — for information about Ludwik Fleck, and everywhere I found total or practically total ignorance. A certain consolation can be found in the fact that Prof. Zdzisław Cackowski, present here, and his co-workers in Lublin are involved — for the first time since 1979 — with this figure: this fact, however, is to be seen as an exception which confirms the rule. I should only like to point out here that the collection 'Szkoly w nauce' (Schools in Science), published a few months ago by the Committee for Scientific Research of the Polish Academy of Science, whose area of concern is closely connected to Fleck's ideas concerning thought-style and thought-collective, makes no mention whatsoever of this scientist. Not a single one of his works appears in the bibliography. It gives one pause that our sociologists of science and researchers were not tempted to study Fleck by Thomas S. Kuhn, whose Structure of Scientific Revolutions was translated into Polish in 1968, and who is well-known among our scientists.

One could make a good case for the idea that the explanation of our country's lack of interest in Fleck's research lies in many of Fleck's ideas or methodological principles — e.g., those concerning the historical aspects

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of the development of science, the psychological aspects of research, in par-
ticular those on the genesis of specific information-‘climates’ and emotionally
stimulating or repressive encounters, and finally the sociological problematic
of the diffusion of scientific knowledge in various milieus.

Before saying anything about the city of Lwów as the cultural and intellec-
tual background of Fleck’s ideas on the sociology of science and the theory
of research, I would like to say that this task is tremendously complicated
and practically unrealizable. In light of his statements on fact (die Tatsache),
one must draw the conclusion that giving a comprehensive and precise recon-
struction of the cultural and intellectual atmosphere of a particular milieu
— in our case concerning one of the largest cities in Poland — provides a
researcher with insurmountable difficulties.

The only attempt at a sort of monograph of the city in our scientific litera-
ture is found in Florian Znaniecki’s Miasto w świadomości jego obywateli (The
City in the Consciousness of its Citizens). This description is based on so-called
personal documents, in particular on statements by the inhabitants of Poznan.
The insufficiencies of this method are well-known: a determination of the
cultural climate of a city on the basis of its citizens’ feelings — even when
these are of a representative section of the population — appears quite risky.
On the city of Lwów we do not even have a monograph of this sort.

The right to work out the influence of a certain milieu on a scientist’s
world of ideas is usually reserved for historians of science; they present science
in the broadest framework of the cultural phenomena of the time in question.
Researchers generally agree that

the picture of scientific issues based on historical analyses is not sufficient even when the
documentation appears complete. The reason for this is to be sought in the fact that the
documents — so-called source material — are not ‘photographs’ depicting the objective
state of affairs and socio-political and cultural influences. The greatest doubt by far,
however, is raised by the fact that the documents relevant for a historian of science
reflect only to a slight degree the climate of small social groups, the informal structure
of scientific organizations and many of the influences relevant to the activity of research.
(Czesław N. Nosal)

Accordingly, the historical aspects of science’s development must be
supplemented by other sources of information and models of thought. Both
methodologists of research and psychologists contribute to these supplements
insofar as they concern themselves with the mechanisms of working up
information in the process of creative thinking and with the decisions and
organization of the creative personality. In summary: a satisfactory answer to
the question of the cultural and intellectual background’s influence on Fleck’s
thought demands a multi-faceted, historical, sociological, psychological and methodological analysis; we do not have such a model for scientific research and the source of scientific discovery at our disposal.

A simplification of our search for the intellectual premisses of Fleck's philosophical and scientific-sociological world of ideas might be found in the evidence he gave concerning himself. According to the information of Mr. Schäfer and Mr. Schnelle, however, he unfortunately left only very modest autobiographical source material. We are thus left only with the presumption on how the atmosphere of Lwów at the turn of the century may have influenced the creative personality of Ludwik Fleck.

Fleck was born in 1896. His youthful years, to which a decisive influence on the development of the psychical and intellectual personality are always ascribed, were spent under the brand of the lack of national freedom. Lwów was Lemberg, the administrative capital of Galicia, which received autonomy in 1860, extended in 1870. The development of the educational system, science and culture within the framework of this autonomy, differentiated the Austrian part of the realm from the Russian and Prussian ones, where — due to the elimination of Polish schools and the Polish language as the official language, among other things — deliberate Russianizing and Germanizing were effected. The legendary penury of Galicia did not of course permit a broad development of culture and the system of education; the populace, did, however, exploit the existing possibilities to the fullest, in particular the intelligentsia. This group was relatively larger in Galicia, measured against the other parts of the realm. Its distinguishing characteristic was that next to the impoverished nobility and citizenry, ever greater numbers of farmers took their place there as refugees.

Like Krakow, which was somewhat pompously depicted as the 'Polish Athens', Lwów could claim old cultural and scientific traditions. The city contained the third oldest university in the country (after those of Krakow and Wilno), founded in 1661 by King Jan Kazimierz on the foundations of the Jesuit College. Since the 16th century, the city had had an important publishing center, which was considerably expanded in the course of the 19th century.

In many cases booksellers acted as publishers, on their own initiative and without financial backing, in which capacity they often had an inspirational effect on scientists and writers. The Herman Altenberg Book Store, for example, in business in Lwów since 1880, was later taken over by Alfred Altenberg, and published the valuable works of Szymon Askenazy, Piotr
Chmielowski and Wilhelm Feldman, to name a few. In some cases the city authorities also acted as patrons, who financed, among other works, Fryderyk Papée’s *Historia miasta Lwowa w zarysie* (Sketch of the History of the City of Lwów) of 1894, and Mieczysław Baranowski’s *Historia szkół ludowych miasta Lwowa* (History of the Primary Schools of the City of Lwów). On the initiative of the citizens the Stanisław Staszic Society and the Piotr Skarga Society were founded (in 1889 and 1908, respectively), both of which had a lively publishing trade. A special society for publishing educational and scientific works, i.e., textbooks, was formed in 1873. There were more of such publishing and popularizing societies, created also by the assistants and students of the university and technical college.

Thus the Society for the Promotion of Polish Science, founded in 1901, published 57 works, 43 of them on history, juridical history and the history of literature.

At the turn of the century Lwów had one of the largest collections of libraries anywhere in Polish territories. The University library, founded in 1784 and destroyed in 1848 during the bombardment of the city, had a collection in 1916 of over 240,000 books, thanks to its patrons. At the university and the technical college, professorial chairs and the students’ scientific circles had their own libraries; the scientific societies had their own separate collections. By far the most important was the Osolinum, a scientific, publishing and documentation institute founded in 1817, part of which moved to Wrocław after the last world war. The library of the Osoliński National Institute was created exclusively thanks to the patronage of the citizenry; Józef Maksymilian Osoliński laid the solid foundations of the further development. Besides books, valuable manuscripts, autographs and diplomas were stored here. Some of the journals published by the Osolinum still appear today.

After Warsaw and Kraków, Lwów was the largest center of the press in Polish territory. Here modern sport was also born, in 1867, with the founding of the gymnastic society ‘Sokół’, which in time spread throughout the entire country. Here were founded the first football teams (1903), ‘Pogoń’ and ‘Czarni’, and the first scout troops (1910).

In the 1830’s a circle of romantic writers was formed in Lwów, inspired by liberal-democratic and revolutionary ideals. Its most famous member was Seweryn Goszczyński. Toward the end of the century, the literary Adam Mickiewicz Society, founded in 1886, became the center of literary life. It was this society which began publishing one of the leading literary journals, the *Pamiętnik Literacki* (Literary Journal), in 1902. Both organizations are still active today; after the war they relocated to Wrocław.
Theatre and music in Lwów flourished very well. The first theatres were already being established at the end of the 18th century; in Fleck's time the Municipal Theatre and several other theatres were founded. The Musical Society of Galicia, established in 1858, founded the City Conservatory in 1880; in 1902 the Philharmonic was opened; in 1913 the first chair for musicology in Poland was created.

A peculiar characteristic of the city's cultural life was its relative openness to the outside world. Polish scientists and artists from the Prussian and Russian parts of the realm, who were often forced to emigrate due to the lack of employment opportunities, often chose Galicia as their place of residence, especially Kraków and Lwów. Several of the cultural and educational organizations threatened with liquidation in the Russian or Prussian parts of the realm moved here; on the other hand, similar organizations formed in Galicia moved the radius of their activity out to other Polish territories. This was the case, for example, with the 'Macierz Polska', founded in Lwów in 1880, which played a considerable part in the development of education and culture throughout Poland. As a part of the Austro-Hungarian empire, Galicia (in particular its political, cultural and intellectual centers, Krakow and Lwów) had certain and easily realizable possibilities for contact with the most important European centers: Vienna, Budapest, Prague, and other cities of the Hapsburg multinational state.

Lwów was a place of intensive cultural diffusion, also due to the centuries-old, complex ethnic structure of the state. Besides the Polish majority, Ukrainians, Germans, Jews, Armenians and representatives of other nationalities lived in the city. The religious structure was just as widely differentiated. The ethnic-religious groups within the population varied with respect to their customs, cultural value systems, political attitudes, aspirations, etc. This found expression in the variety of cultural and intellectual life, the architecture, the specific dialect and even the sense of humor. Without exaggeration, one can categorize the moral and cultural visage of the city as being unusual and unique. Each of the significant nationalities created its own cultural organizations, theatre, educational organization, publishing houses, scientific societies, etc.

The Austrian authorities, acting by the classical rule divide et impera, consciously sharpened the antagonistic relations which to some extent already existed among national groups – in particular the two largest, the Ukrainians and Poles. Simultaneously, however, a kind of social contract was being formed among the nationalities, which justified a stronger social position in accordance with greater achievements. This fact could have had a
particular significance for the cultivation of Ludwik Fleck's personality, he being of Polish-Jewish descent. We can hardly imagine that he would not have felt the effects in his earliest years of that particular situation, which forced everyone to face certain choices and options. In such a situation, sober and critical spirits usually take on a neutral, independent attitude, one which is ready to compromise and is characterized by generosity and tolerance. This attitude seems to be a factor in the formation of the philosophical-sociological thought of Fleck and of many Lwów intellectuals contemporary with him.

Another fact which supports this view is that the situation in Lwów was considerably more liberal than it was in Krakow, where the ethnic-religious situation was characterized by an obvious preponderance of Poles and where the conservatives could dominate politically and intellectually for decades. One need only think here of the formation of the city councils in Krakow and in Lwów. While the Krakow council had 40 members, the democratic statute of the Lwów council permitted the election of 100, i.e., an incomparably broader social representation.

It is difficult to evaluate the degree to which the tendencies sketched above in the political and cultural-intellectual life of Lwów in the time of division found their continuation in independent Poland. Since it even came to an armed struggle between Poles and Ukrainians concerning where the city belonged geo-politically, both claiming it, national differences suffered a considerable escalation. In 1919–20, this struggle was decided in Poland's favor; in the consciousness of Ukrainians, however, Lwów remained the potential capital of the west Ukraine. The Polish state subsequently did everything possible to strengthen the Polish elements of the city, which represented a kind of island in the sea of Ukrainians inhabiting the province.

In science, culture and art, Lwów maintained its high level during the period between the wars. An important center of theatre and music developed here; with Schiller and Horzyca at the top, it was, next to Warsaw, the most important such center in Poland. In the 20's a group of writers with leftist inclination began to work here, publishing its journal Sygnaty (Signals) in 1933. In 1936 the anti-fascist Congress of Workers in the Field of Culture was convened in Lwów — a group which found a widespread echo both within Poland and beyond its borders. Jan Kazimierz University, the Lwów Technical College and the Ossoliński National Institute remained the most important scientific centers. Many professors continued with their work from before the war, others went to different universities throughout the country.
Historical research (Ludwik Kubala, Szyman Aszenazy) remained a strong point of the university, as did the history of Polish literature (Antoni Malecki), earth science (Eugeniusz Romer) and medical science (Rudolf Weigl, Ludwik Hirschfeld).

Its greatest fame, however, Lwów reaped from its two schools: the mathematical school, dating from the lectures of Waclaw Sierpiński in 1909, which were later further developed by Stefan Banach and Hugo Steinhaus, and the philosophical school, later called the Lwów-Warsaw school, of which Kazimierz Twardowski is considered the founder. Among its students were Kazimierz Ajdukiewicz and Tadeusz Kotarbiński. Stefan Zamecki gives the names of 29 philosophers in a list which he says is incomplete. It is perhaps characteristic that Ludwik Fleck had close contacts with representatives of both of these schools.

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In this work, I will investigate the question of the intellectual sources which influenced Fleck in the development of his theory of ‘thought-style’ and ‘thought-collective’. In this investigation, I develop the thesis that this influence was of a quite dominating philosophical nature, one exerted by the Polish philosophers working in Lwów between the wars.

The major theme of Fleck’s philosophy of science was the question of the genesis and development of new knowledge. In order to answer this question Fleck undertook what was probably the first sociological investigation of the production of scientific knowledge. According to this approach, knowledge is bound up with the interactions of the people producing and possessing it. In short: knowledge and science are essentially social and cultural phenomena. On this view, the starting point of knowledge and of explanation of the world is not the individual but the collective. Fleck vehemently attacks the ‘heroic legends’ (1934: 181) which have traditionally been woven around new discoveries or inventions in the sciences in reporting them: “A kind of superstitious fear prevents us from attributing that which is the most intimate part of human personality, namely the thought process, also to a collective” (1935a: 60, Eng. 43). The individual is therefore elevated to a genius, to a “kind of conqueror like Julius Caesar, winning his battles according to the formula ‘I came, I saw, I conquered’” (1935a, Eng. 84). On Fleck’s view, very little can be explained, also in the sciences, with this kind of thinking: it is only when one takes the socially and historically conditioned thought-collective into account that the progress and achievements of science become understandable.

If one inquires into the historical development of this approach to explaining the development of scientific knowledge it becomes self-evident that it cannot be reduced to the genius of its author. Rather, we must ask what was the specific thought-collective in which Fleck took part, and which, on the basis of the make-up of its members, produced the interaction of thought out of which Fleck was able to formulate his well-known concepts.

However this does not mean that we should look for identical statements by scientists in touch with Fleck in order to prove that their theory was also his. Fleck’s notion of the thought-collective was misunderstood in this way:
the members of such a collective are distinguished not only by intellectual similarities but just as much by divergent backgrounds. They bring these various orientations from other thought-collectives along with them into the thought-exchange: indeed, this is what first makes such a thought-exchange possible. The communication among a collective's members, out of which new ideas are creatively formulated, is thus distinguished by the fact that each member interprets the thought of the other somewhat differently, transforming it and bringing it into different relations with other thoughts and then bringing it back into the discussion in a changed form.

The question of the developmental history of Fleck's 'theory of thought-style and thought-collective' is thus first of all a question of the communications he maintained before and during that theory's formulation. On this basis we can then investigate how this communication was interpreted by Fleck himself, and thus how it led to Fleck's specific synthesis of it.

The biographical information we have on Fleck's life and work (cf. my article in this volume, pp. 1–38) suggest that his philosophy was developed under the influence of three philosophers of Lwów, Fleck's birthplace and home until the second world war, even if he never quoted them. They are Kazimierz Twardowski (1866–1938), Kazimierz Ajdukiewicz (1890–1963) and Leon Chwistek (1884–1944). Although all three were of greater importance for philosophy in Poland in general and beyond its borders, there is little known about them outside Poland.

The claim that it was precisely Polish philosophers who had a decisive influence on the person considered to be one of the founders of the sociological perspective in the philosophy of science at first sounds paradoxical: Polish philosophy between the wars is primarily known for its contribution to the development of neopositivism (Ajdukiewicz) and for its logicians (Łukasiewicz, Tarski, Chwistek). Fleck polemics vehemently against both of these directions in his publications. As shall be shown, however, it was precisely this background which had the decisive influence on Fleck's theoretical development — and this despite the fact that Twardowski, Ajdukiewicz and Chwistek represented wholly contrary approaches: Kazimierz Twardowski subscribed to his teacher Franz Brentano's descriptive psychology, Kazimierz Ajdukiewicz, with his 'radical conventionalism', maintained an extreme nominalist position in the period relevant for Fleck (1926–1936), and Leon Chwistek, finally, was known for a radical nominalism in formal logic.

We get a totally different picture if, instead of questioning the conceptions these philosophers developed, we consider their questions in the answering of which they developed these concepts. In this way a totally new perspective is
obtained, for it now becomes clear that their thought does indeed seem to be inter-related. The questions around which the thought of these three philosophers centered were all concerned with the 'what' and the 'how' of the factors determining 'reality'. They differed among themselves only in the answers which they gave to these questions.

When this perspective of the ontological determination of reality is put in the foreground of the discussion of each of these three philosophers, it becomes clear why Fleck's thought also centered around this question: in doing so, he continued the discussion which Twardowski, Ajdukiewicz and Chwistek carried on before him, each in his own way. Thus we can correct the widely accepted impression that Fleck wrote about the collective character of knowledge and knowing but was himself an example of the very opposite: an individual creator seeking his equal in the radicality of his individualism.

It may of course be asked to what extent it is legitimate not to take the conceptions propagated by the philosopher under interpretation as the starting point of this interpretation, but to take as such a question which is induced by this interpretation, thus to take up the thesis that this question is the ultimate foundation of the philosopher's thought. Such a thesis, if it is to be proved, must of course be proved with reference to its contents. Fleck provides us, on the other hand, with a methodological proof. This may seem surprising at first glance: after all he very clearly describes how the firm convictions of a thought-collective determine its perceptions — indeed make them so one-sided that they become a 'harmony of deceptions'. But this is only one side of the medal: Fleck states that the thought-collective performs two opposed activities in dealing with new problems of knowledge, moving back and forth between them in its scientific work: on the one hand, corresponding to the need to maintain the harmony of the system of opinion, the scientist attempts to reconcile the assumptions of the thought-style in question by a 'tying together of ideas', so that the new problem can be reduced to a concept agreeing with this style. But where this is not further possible, this failure has an effect on those foundational assumptions. And this is the other side of the medal: the scientist examines whether the explanation of the problem and the thought-style can be brought together by a change in one or another of the assumptions which up to that point had been undoubted, or by replacing an assumption by another. The thought system's tendency to persist and the incentives to modify it thus mutually affect each other. Neither is ever possible by itself.

Thus on Fleck's view scientific progress means the working out of unclari-
ities inherent to a thought-style. The result of this proceeding is the "mutation in thought-style": "from the present confusion . . . new concepts will arise" (1935a: 38; Eng. 26). If we want to understand the intellectual content of an actual thought-collective, then it cannot suffice to describe only its thought-style, and the system of meaning corresponding to that thought-style. *The problems with which the thought-collective is struggling at a given time* must also be taken into consideration. The system of meaning and the thought-style are much easier to grasp than the problems being worked on: they are formulated explicitly at the heart of the theory, for the instruction of the future generations and its 'popularization' for laymen (1936: 86). The problems being worked on, however, are characterized by their being unclear, not yet unambiguously formulated in the style of the collective in question, and not yet capable of a 'solution in an adequate style-manner'(1935a; Eng. 100).

Such problems are disturbing because they create unclarity, but their value is probably greater for the intellectual development of the sciences than is usually assumed on the basis of their necessarily ever imprecise designation. For this work it is sufficient to establish this for the theoretical sciences, i.e., those sciences which are not prominent in practical application. Here, with disciplines such as philosophy or theoretical sociology, we see immediately that the further development of these disciplines is directed to the working out of problems frustrating to the thought-style in question. In practical sciences, and especially in medicine, a researcher-collective is much more willing to accept theoretical contradictions, as long as practical results are assured. [Fleck also pointed this out, cf. Fleck 1927a.] It is always the aim of the theoretical scientist, on the other hand, to prove its cognitive structures to be as comprehensively assertible as possible. Success here is measured by the degree to which immanently determined explanations are possible.

Such problems will thus often lie at the heart of the scientific work in such fields. The strength of the thought-style must be proved with them. Yet they often remain unanswered. In confronting them, the thought-style 'wears itself out': it is changed so as to gain more explanatory power. The result of such a process can finally be that the problem, while still not solved, is no longer perceptible to the transformed thought-style.

This does not mean that the framework in which and with which the working out of such crucial problems occurs is not important in the intellectual influence on scientists of future generations, and also for those in theoretical disciplines. Its importance however can diminish greatly in the light of those problems.
On the basis of these considerations I can now formulate the thesis of this article as follows:

*The three great philosophers of Lwów, Twardowski, Ajdukiewicz and Chwistek, influenced Fleck's thought above all by the problems which were central to their own intellectual work.* For it was with these central problems, suggested by their thought-styles, that Fleck, too, struggled. For this reason is is primarily these problems which determined the development of the content of his theory.

Fleck took them over from his teachers — and changed them, by placing them in a different framework. They are indeed more than mere 'pre-ideas' which Fleck describes in their heuristic function for the history of science. For Fleck took over not only these problems, but also a whole series of conceptually fundamental assumptions.

It thus becomes clear that the communication network in which Fleck developed his 'theory of thought-style and the thought-collective' was of a philosophical nature: the conceptions he developed and the problems which he attempted to solve were found primarily in these three philosophers. In taking up these questions he systematically entered into the questions which remained open within the framework of their approaches, in order to ask them on a new foundation and bring them to a coherent solution. These are the questions of the foundation of reality, knowledge and truth.

Fleck supplemented what he had taken over with important new perspectives, which he took from very different sources and related to his primary ones: these are the sociologicizing and historicizing perspectives. The recent sociology of knowledge, which Fleck knew only superficially, was one among many other, probably coincidental, resources which provided him with the necessary instruments to ground the philosophical conception of these questions which he had vaguely conceived.

To investigate the influence of these three philosophers on Fleck, we must answer three questions with respect to each of them:

(a) Which are the central problems which were taken over by Fleck?
(b) Which conceptual assumptions of the theoretical framework did Fleck take over?
(c) Which new hypotheses did Fleck develop in order to be able to solve these problems within a different theoretical framework?
1. IS THE REALITY WE RECOGNIZE SOMETHING CREATED?
KAZIMIERZ TWARDOWSKI'S DESCRIPTIVE PSYCHOLOGY

The themes of Twardowski's works were the nature and status of psychic phenomena such as representations, judgments, notions, cognition and knowledge. He thereby continued the work of his professor, Franz Brentano, on descriptive psychology. The central problem of descriptive psychology and of Twardowski's school as a whole was the notorious ambiguity with which the ontological status of 'objects' was loaded. Pomian gives a pregnant formulation of this dilemma in his short characterization of Twardowski's philosophy:

whether the 'object' of the representation is something given or is produced by the art of representation is a question that seemed to trouble both Brentano, and, I daresay, all the adherents of his school. It also became Twardowski's point of departure in his 'Zur Lehre vom Inhalt und Gegenstand der Vorstellungen' (1973: 37).

In this 'Theory of the Content and Object of Representations' (1894), Twardowski assumes a Kantian position. He accordingly distinguishes three levels of being: that of the 'things-in-themselves', that of the objects to which psychic phenomena are related, and that of the content of psychic phenomena (1894: 35). In fact, however, Twardowski nowhere attempts to more precisely define the level of the 'things-in-themselves.' He limits himself to one sentence, that the 'thing-in-itself' is that 'which affects our senses' and is thus something different from the object. He concentrates instead on the two other levels — that of objects and that of the content of psychic phenomena.

With his distinction between 'object' and 'content', Twardowski wanted to overcome the vagueness clinging to Brentano's concept of object. According to his distinction, the content of psychic phenomena is clearly proved only by the act through which a subject creates a psychic phenomenon. For example, in the representation grounding psychic phenomena for others, the content of the representation is confirmed by the activity of representing. Its existence can only be traced to the psychical activity.

In opposition to this and at first quite ambiguously Twardowski discusses the ontological status of the 'object' corresponding to this content. Just as with Brentano, it remains unclear whether an existential determination independent of the psychically active subject is to be attributed to the object or not. In order to distinguish 'object' from 'content', Twardowski emphasizes first of all that all psychic phenomena are directed to an object, which is "assumed to be independent of thought" (1894: 9, my emphasis). Immediately
afterward he takes this back: ‘to be an object’, indeed ‘to be’ at all, is identical for Twardowski to ‘being the object of representations’ (1894: 37). Thus any being independent of representations, i.e., independent of the psychic activity of subjects’ representations, is rejected as impossible. There is no need to refer the object back to any ‘thing-in-itself’; the object is linked to the experience of the subject.

Twardowski does state the limitation that the existence of an object is thus only ‘phenomenal’ or ‘intentionally’ proved. The distinction must be made between ‘phenomenal’ and ‘actual’ existence (1894: 24f.). With this ‘actual existence’ Twardowski returns to his original characterization of the object as being ‘independent of thought’. Apparently by this he alludes to the ontological level of the ‘things-in-themselves’ – that level which embodies an immovable objectivity totally independent of the subject. But Twardowski must admit that his ‘objective level’ remains of no interest to the psychically active subject: the subject is unable to grasp it, or whatever influence it might have. Statements on whether something ‘actually exists’ are thus nothing more than expressions of belief – judgments made by the subject. It is these judgments alone which attribute or deny ‘actual existence’ to objects.

Thus Twardowski ultimately sees all existential statements concerning objects as being grounded in the experience of psychically active subjects: the phenomenal existential determination coming from an act of representation, the ‘actual’ existential determination from an act of judgment. But representations and judgments are both exclusively ‘activities of the mind’.

How this mind of the subject constructs the objects in acts of representation and judgment, however, is not thematized by Twardowski. Questions as to how they are produced and which factors are involved in their production not only remain unanswered, they are not even explicitly asked. If Twardowski had pursued them, his dilemma would apparently have been the following: on the one hand it would be possible for him only to relativize the objects of existence to the mental activity of the subjects, given the foundation of the conceptions of descriptive psychology. On the other hand everything in him resists this kind of ontological relativism.

This conflict becomes blatant in his discussion of ‘cognition’ and ‘knowledge’. For him both phenomena can again only be psychic and not physical: “Cognition” he defines as “one or more true judgments which we make concerning any object” (1898b: 40). But an object is always judged only in so far as it is represented: representations “provide the mind with material, deliver content to it, whereas judgments, feelings and desires are various ways in which the mind stands in a relation to that content, either accepting it or
rejecting it" (1898b: 41). Twardowski thus defines cognition and knowledge as phenomena which are founded only in the mental activity of the subject. Yet he still wanted to salvage the objectivity of 'true cognition' and 'true knowledge'; indeed he polemizes against all 'so-called relative truths' (1900). He seeks a solution to this contradiction in two different ways:

The truth of a judgment, according to Twardowski's definition, is shown in its agreement with reality (1900: 424), where by 'reality' he means the 'actual existence' of objects, independent of the subject. The impression that truths are sometimes only 'relative' occurs, on Twardowski's view, only due to the unjustified neglect of the situational conditions which form the basis of the corresponding judgments. The truth value of statements can then only be determined if all of those conditions under which the statements occur are taken into account. It is only then that it becomes clear upon which judgments the statements are based. To these conditions belong time, place, historical and social circumstances, and finally the person making the judgment.

Thus judgments are always true or false only for the subject making the judgment, for this belongs to the judgment just as much as its subjective representations to which the judgment refers. So on the one hand Twardowski can salvage the demand that a judgment either be always true or not true at all, but never conditionally true, because the judgment is always valid only for the person making it. On the other, however, he must abandon the attempt to determine for more than one individual subject the truth value of such judgments as claim to be valid. Scientific knowledge can therefore never be described as 'true' or 'false': The dilemma of the objectivity of statements thus becomes the dilemma of their singularity.

It remains however not only with reference to statements whose validity goes beyond the individual subject that Twardowski must abandon any claims of ascertaining truth: that 'reality' with which a judgment must correspond in order to be true remains unattainable in most cases, even for the individual subject. The subject can attain certainty — evidence — only concerning his own psychic phenomena, perceived through inner experience. In all other cases "our judgments concerning the external world extend only to the objects that we represent" (1900: 441). To claim within this theoretical framework that there are no relative truths, only valid truths independent of here and now, thus becomes a pure postulate, incapable of verification and without importance for the cognition which is in fact possible for the subject: the dilemma of the objectivity of statements thus leads to the task of being able to assign truth values to any statements at all.
The Question of a Subject-Independent Truth

Ludwik Fleck confronts this ambiguity in Twardowski in rejecting that which so captivates the latter: since there is no practical possibility of a subject-independent reality being relevant to the psychic activity of individuals, a theoretical claim denying this must also be rejected. The way is then cleared to supply a satisfactory theoretical framework for the actually observed behavior of psychically active subjects: “Neither the ‘subject’ nor the ‘object’ receive a reality of their own; all existence is based upon interaction” (1929b: this volume, p. 49).

But Fleck does not merely link ‘reality’ and ‘existence’ to psychic activity in general, as Twardowski does. Rather, in his view they are seen to be constituted more precisely in the activity of cognition: “The thought-style creates reality not in a different way than other products of culture . . .” (1936: this volume, p. 112).

Individual and Collective

As opposed to Twardowski, Fleck also abandons the attribution of reality-constituting activity to the individual subject when he abandons the objectivity of cognition. He replaces this with super-subjective collectives, human communities. By taking this parallel step he can avoid being forced to admit any individuation of separate realities which ignores observable behavior. The assumption of a subject-independent reality is replaced by the collective, or, to put it more precisely, by the thought-style created by the subjects at work within the thought-collective. And he can claim in so doing to more closely approach the behavior we observe than Twardowski was able to do.

The sociological perspective which Fleck introduces into the investigation of such phenomena as were psychically defined by Twardowski is certainly Fleck’s own innovation. Yet we should not overlook the fact that that very innovation had already been prepared by Twardowski. During the first ten years of Twardowski’s scientific publications he defined all of these phenomena, such as representations, concepts, judgments, cognition, wishes, fears and hopes, as psychic, as being bound up with the mental activity of individual subjects. Then, under the influence of Husserl’s Logical Investigations (1900/01), he attempted to account for the fact that many of these phenomena are indeed investigated by the human sciences independent of their psychic character. In his paper ‘On Activities and Products’ (‘Über Tätigkeiten und Erzeugnisse’, 1912a) he distinguishes between psychic activity, producing a phenomenon, and ‘conserving’ that phenomenon in a psychophysical form.
Phenomena such as judgments, concepts or cognition are accordingly psychic in their genesis and in their actualization by a subject, but can be 'conserved' in a physical form — as, e.g., in a written sentence — between genesis and actualization. According to Twardowski, this makes it possible to investigate them, but it also creates a false impression of independence from the productive or actualizing activity of the subjects.

In this connection Twardowski takes up the problem of intersubjective meaning-conferral for the first time: of course each of the subjects who, e.g., read a sentence activates his own, different psychic phenomena (1912a: 122). Such processes, however, can lead to abstractions the result of which is the intersubjective interpretation of the psychophysical phenomenon:

... we also say that a sentence calls up 'the same' thought in different people, whereas in fact it calls up as many thoughts as there are individuals, in whom these thoughts are not ever identical. But we abstract from that in which these thoughts differ, and consider as the meaning of the sentence only those parts of the thought which agree with one another and with the corresponding elements of the thought of the individual using the sentence (1921a: 122f.).

It is precisely this idea that Fleck takes up: conferrals of meaning are on his view neither individual nor interpersonal occurrences; they are superpersonal interpretations. With its thought-style, a thought-collective provides the foundation on which an individual belonging to the collective can understand a phenomenon. The collective replaces the individual; cognition thus becomes a social activity instead of an individual one. But it remains, for both Twardowski and Fleck, a human activity.

It is especially interesting, however, that Fleck takes up the tension which Twardowski could not solve between super-individual and individual aspects of cognition in order to more narrowly determine how cognition comes to be. Thus Fleck emphasizes at first that in all of the talk about the collective-social character of cognition the single individual is also necessary for its achievement. He thus introduces two different roles by which the individual becomes essential to the process of cognition. The task at hand, which is always fulfilled by an individual, is first of all the simple act of confirming what any research on the basis of a certain thought-style must show to be inevitable or 'constrained' results. "The constrained results correspond to passive linkages, and constitute that which is experienced as objective reality. The act of ascertaining is the contribution of the individual in cognition (1935a: 56; Eng. 40). The collective's share, on the other hand, is creative, viz., the creating of the presuppositions, i.e., the positioning of active linkages, which lead to cognizable 'inevitable results'."
But even the collective’s share in cognition, the second role mentioned above, is composed of individual acts, even if these cannot be neatly ascribed to the single individuals involved: active linkages and thought-style come into being in the mutual communication of thought among collective members. Every exchange of thought within a thought-collective thus leads to the securing of its system of opinion, to its ‘de-individualization’ (1936). Even more interesting, however is the circulation of a thought beyond the borders of the thought-collective, for “it is always related, in principle, to its transformation” (1936: this vol., 85). It is in a relation of tension that it enters into the communication that strengthens the posited active linkages. Every expression of thought, Fleck says, is socially bound by both its origin and its destination. Communication is thus always communication between individuals. Only if both belonged to the same thought-collective, which was totally isolated from the world around it, would the interpretations of thought be identical in both the transmitter and the receiver. But we normally cannot assume such thought-collectives: every individual belongs simultaneously to different thought-collectives. The influence of these competing orientations cannot simply be screened out. The evaluations of other styles of thought are thus also involved, via the individual, in the communication within one of these thought-collectives. In this way, mechanisms come into play which work against the rigidifying of a thought-collective’s system of opinions: in the communication of thought, thoughts and ideas ‘circulate’ from individual to individual. Every ‘receiver’ of such ideas in the collective understands them somewhat differently, relates other associations to them — and thus interprets them with another meaning.

Thus Fleck sees one stage among the multitude of individual interpretations which is essential to the development of cognition: on his view communication means not only understanding one another, but also misunderstanding one another. It is above all the misunderstandings that lead to the changes in meaning which make new knowledge possible.

With the aid of this social perspective, Fleck is also able to explain why the different interpretations by the persons involved in the circulation of a thought do not become mutually independent thoughts in spite of their manifoldness. Twardowski had left this question open, too: he only spoke of the fact that abstractions lead to super-personal interpretations of thoughts, not of how this can occur. Fleck calls the social factor he sees at work here the ‘social’ or ‘intellectual atmosphere’ (1935a: 104; Eng. 76; 1935d: this vol., 75f.) 75). This is the sociological instrument with which Fleck is able to prove the inter-subjective validity of knowledge: it leads the development of a thought
into the path of certain historically given, socially predominant ideas and co-
ordinates the perception and action of the collective’s members accordingly.

*New Definition of Truth*

In the same way Fleck can also oppose new evidence to Twardowski’s further
dilemma of on the one hand maintaining an objective reality but on the other
denying it any practical relevance to knowledge. Since Fleck abandons the
claim to any reality independent of the subject, he cannot define the truth
value of any knowledge as agreement with this objective reality. Instead of
interpreting reality as being independent of the subject, Fleck interprets
it as a social product of the thought-collective. A statement’s truth is then
determined by its agreement with the thought-style in the framework of
which it is expressed. ‘Truth’ is therefore meaningful only in relation to a
specific thought-style: it appears at that point where style-adequate presup-
positions clearly determine the working out of a problem. Contrary to
Twardowski, Fleck is thus able to actually determine the truth of a statement.
Of course he has fundamentally changed the understanding of ‘truth’ in the
process.

*What is a Fact?*

Corresponding to this analogy in the conception of truth, we also find one
here in the concept of a fact. Although Twardowski does not use this term
explicitly, what he understands to be a ‘fact’ is clear: any object of a represen-
tation, the existence of which is affirmed by a judgment referring to this
representation. The ‘fact’ is thus conceptually closely related to the ‘concept’:
according to Twardowski’s explicit definition, a concept is the ‘representation
of an object . . . which consists in the (substrate) representation of an object
similar to that object and in the representation of judgments related to that
similar object’ (1903: 13). The fact differs from the concept by its object’s
existences being positively judged. With the concept, on the other hand, the
object is changed due to the judgments’ abstractions; in fact these judgments
do not even need to be performed as such — it is sufficient to represent them
as judgments. Similarly to the concept, then, the fact, too, is a judgment of
an object.

Two of the most essential elements of Twardowski’s conception are also found in Fleck:
The first element has to do with the connection between ‘fact’ and
'experience': Twardowski sees the existence of represented objects as being grounded in the subject's mental activity alone. The phenomenal existence of an object follows from the representation of this object, an actually existing fact from the attributing of this existence in a judgment. Consequently, a fact is a subjective belief, something created by the subject. It is thus at the same time linked to the subject's experience. For Twardowski this linkage also works the other way round, for he defines 'experience' as "knowledge, based on one's own perceptions" (1897: 11), perception in turn as a kind of conviction by which the subject judges the objects represented.

Fleck relates fact and experience in a very similar fashion: for Fleck, Toulmin declares (1986: this vol., 277), "'facts' are what we make of our experience", through the exercise of judgement: Fleck describes the genesis of a fact in the cognitive process as the gradual transition from originally "unclear seeing" and "in adequate primary observations" to the "developed, reproducible, style-adequate seeing of Gestalten" (1935a: 123f; Eng. 91ff). This process is accompanied by a steady increase in experience: the knower, or, to be more precise, the cognizing collective, becomes involved in a struggle with the initially formless perceptions in so far as it brings associations from its hitherto-gathered experience to bear on them and attempts to draw interpretive lines in the unknown material. Where this is successful, a new fact's "style-adequate signal of resistance in thinking" occurs (1935a: 129; Eng. 98), or a "direct perception of form [Gestaltschen]" (1935a: 124; Eng. 92). Fleck thereby links the thought-collective's possible knowledge of objects to its members' store of experience, out of which this knowledge is literally 'worked up' (er-arbeitet) by the collective.

On the other hand Fleck defines experience as a "complex state of intellectual training based upon the interaction involving the knower, that which he already knows, and that which he has yet to learn" (1935a: 17; Eng. 10). His concept of experience is not completely unambiguous, since for Fleck, too, it is always composed of prior knowledge. Thus the relation between the knowledge of facts and the experience of the knower is circular in a way similar to Twardowski.

The second analogous element in both Twardowski's and Fleck's understanding of a fact is concerned with the connection between 'fact' and 'concept' (Begriff). As was stated above, both facts and concepts are in Twardowski's view evaluations of a represented object: they differ only in the manner of evaluation. The connection between them is made more complicated by the fact that a concept is itself also categorized as a representation: as a conceptual representation as opposed to a perceptual (anschaulich)
one. Accordingly, a fact can also be the evaluation of a concept. And on Twardowski's view, inasmuch as it is a 'scientific fact' that we are dealing with, this is indeed the case. For only conceptual representations can be determined sufficiently precisely, due to the number of (represented) judgments they contain. For this reason it is only with conceptual representations that thinking first becomes actually possible. The construction of concepts demands a mental labor possible only with language. It leads to replacing difficult unclear representations of judgments of concepts with clear representations of names symbolizing those concepts. Scientific facts thus ultimately stand for the evaluation of names, which in turn symbolize conceptual representations.

Fleck describes a fact as the result which inevitably presents itself as an inescapable consequence of the accepted presupposition, the active linkages. In the examples he gives to clarify his idea, he identifies those 'active linkages', the 'presuppositions of thought', with 'concepts', although without further grounding this identification. So, for example he says, "In our history of syphilis the combination of all venereal diseases under the generic concept of carnal scourge (Lustseuche) was thus an active association of the phenomena" (1935a: 16; Eng. 10). 'Concepts' in his view are paraphrases of active assumptions which lead in the cognitive process to the affirmation of resulting 'passive associations'. It is therefore not surprising when Fleck also describes a 'scientific fact' as a 'conceptual relation' (1935a: 110; Eng. 83). For the content of this definition corresponds to the latter's definition as a "style-adequate aviso of the resistance of thought" (1935a: 129; Eng. 98) to which I referred earlier: The 'resistance of thought' is the ascertaining of a relation resulting from the interplay of actively posited presumptions. When concepts are nothing but paraphrases of such active linkages, however, a fact must also be a relation of concepts.

The relating of concepts to one another can certainly be described, now once again in a Twardowskian sense, as a kind of evaluation of these concepts. Fleck's idea of the relation between 'fact' and 'concept' thus runs analogously to Twardowski's. When Fleck speaks in this connection of the ascertaining of a relation he is talking about a more precise, undescribed kind of conceptual evaluation, to which Twardowski does not specifically refer.

Observation and Concept Formation

For Twardowski 'representing' always meant both abstraction and synthesizing into a whole. But he does not see this characteristic only in representations:
he also sees it given in perceptions and observations. Concerning representations he affirms succinctly: "There is no adequate representation of any object" (1894: 83), where by 'object' he apparently means some unified 'object' existing outside the concrete representation. It is never the case that all of the elements of this object are taken up into its representation. This is why two persons' perceptions of one and the same object will always be different. In every representation certain details come to the fore while others fade into the background or disappear entirely. Twardowski only hints at the question of what determines the individual case and from what is abstracted there: psychological laws and practical needs (1894: 86) or past experience (1899).

Even if Twardowski does touch here on the elements determining abstractions as representation, he says nothing about such factors as they play a role in the synthesizing of representations into wholes. But there is no doubt in his mind about how the matter stands: every representation is holistic, for it always represents an object, and represents it in its totality (1894: 91). It is of course the phenomenally determined object of the representation that is meant here, but not that object outside of the representation. This characteristic, too, applies just as well to perceptions and observations: Twardowski defines perceptions as beliefs, and thus as judgments, in which sense impressions are synthetically melted down into wholes. As he says at one point, they have a Gestalt quality to them (1898a: 125). Observations are further characterized by the perceiver's being consciously directed to an object.

These same elements of psychic activity, abstraction and synthesizing into a whole also determine the formation of concepts for Twardowski: concepts differ from perceptual representations due to the fact that the abstraction leading to them is consciously performed. Concepts are thus created in thought (1899: 54ff., 1919: 204). Since Twardowski also considers concepts to be necessary presuppositions and elements of thought, and thus of scientific knowledge, as stated above, he therefore also makes the characteristics of representations the presupposition and elements of the same. To form a concept, or to gain knowledge, then means (1) to consciously interpret the object to be represented, since it can only be represented when abstraction is made of several of its possible representable elements and (2) to melt down and fuse the represented characteristics of the object into a whole.

In Fleck's idea of scientific perceptions and observations, we find both of the characteristics of representations and perceptions given by Twardowski: they come into being through a simultaneous abstraction and synthesis into a whole. Fleck grasps both of these factors in his concept of 'directed seeing',
or, as he usually says, 'directed perceiving', by which perceptions always have the property of being wholes. It is precisely with relation to this point that Fleck discusses the necessarily presupposing character of our knowledge. Thus, Fleck, like Twardowski, emphasizes that perception, as the seeing of Gestalten, always presupposes abstraction: "To catch sight of any definite form from any field one has to be in the state of a specific mental readiness which is likewise composed of a more or less compulsory abstraction as regards the possibility of other forms" (1933d: this vol., 61).

In the seeing of Gestalten, the enhancement of certain characteristics of the observed object goes hand in hand with the repression of other of its properties. Particularities are even added if a Gestalt agreeing with the knowledge hitherto obtained can thereby be perceived. This allows every perceived object to become a whole: "Psychology teaches us that every perception is, in the first case, the seeing of wholes" (1947e: this vol., 130).

And the same goes for the forming of concepts: perceiving and grasping in concepts go hand in hand. Thus concepts and rough ideas also come into being through processes of abstraction and syntheses.

As opposed to Twardowski, Fleck extensively discusses the question of the factors influencing the course of abstracting and adding in thought. He sees these factors above all in knowledge hitherto obtained: "In order to see, one has first to know . . . what is essential and what is inessential; one must be able to distinguish the background from the image; one must know to what category does the object belong" (1947e: this vol., 129, 130).

Otherwise seeing would remain 'unclear looking', a useless staring at the manifold singularities which cannot be ordered in obvious Gestalten. But if one does learn to see certain Gestalten, one immediately loses the ability to see any others (1935d: this vol., 74ff): that which contradicts the thought structures is rejected as irrelevant, overlooked, or if necessary simply ignored. It is then only another small step to the 'harmony of deception' (cf. 1935a: 40ff.; Eng. 28).

Unlike Twardowski, Fleck sees the genesis of a directed readiness for perception as a social procedure, and not as an individual one. It takes shape in the common thought-style of a thought-collective. "We look with our own eyes; we see with the eyes of the collective body" (1947e: this vol., 137). The consciousness of the conditionedness of perceptions subsides due to the thought-style which provides their foundation (1935d: this vol., 55–78). Analogously to the way new knowledge is formed, new Gestalten come to be as the Gestalt motivations of one thought-style are transferred over to another realm. This, however, is only collectively possible.
Twardowski's and Fleck's views on representations and perceptions thus differ most essentially in regard to who is seen to be their bearer? Twardowski assigns them to individuals, Fleck to social collectives. A far-reaching agreement exists between them, on the other hand, in their seeing perceptions as the result of abstracting and synthesizing mental activity. On the basis of this fundamental agreement it cannot be very surprising that both share a problem which remains unsolved: if only certain characteristics of the object are included in perception, others being screened out by an abstraction, how is it possible to even speak of such non-represented characteristics in any significant way? Fleck does give an answer to this question, but it seems more an expedient last resort than a real answer: He says it is possible by a comparison of perceptions from various thought-styles (cf., e.g., 1947g).

Science as a System of Knowledge

There is finally one last concept of Fleck to be discussed, the structure of which he again laid out similarly to the corresponding bit in Twardowski in order to secure it through the sociological perspective: his vision of science as a system of knowledge. Fleck describes science, like every other system of knowledge, as a system of opinion (1935a: 40ff.; Eng. 27), whereas Twardowski describes it as a complex of judgments (1912b: 18). This complex of judgments is a "unified and ordered collection": all judgments are grounded, viz., on one another. They thus proceed one from the other. These foundations, however, necessarily come to an end in judgments which cannot be grounded any further (1923: 369). Every grounding of scientific judgments is thus ultimately based on primary principles which cannot themselves be 'scientifically' proved. Twardowski does not discuss the question of whether these principles are to be seen as subject will or determined by some other factors. Later Fleck discusses precisely this question.

Fleck describes science, as we have seen, as a 'system of opinions', and not as a 'complex of judgments'. Systems of belief are brought into existence by the social processes stabilizing the thought-collective. They claim to comprehensively explain their area of concern. The stronger the thought-styles which lie at their root, the more suggestive their power over the collective's members: they develop into 'inflexible structures', which seem to exclude any contradiction. All knowledge, every statement contributes to its fortification. They are bound up in the mesh of all hitherto made statements, which make it into a 'closed, harmonious system'. For this reason legitimation is always possible only within the given system of opinion. But legitimation is
thus always pressing up against the limits of this system: it always reaches a
point at which no further justification is possible, at which certain proposi-
tions must be accepted as the foundation of all justification. These explana-
tory presuppositions of the system of opinion are nothing absolutely constant —
they can be exchanged for other explanatory statements of the system. On
which presuppositions a thought-collective is concretely united depends on
the social and historical forces at work on it and in it. Or, to put it differently:
"The preconditions correspond to active linkages and constitute that portion
of cognition belonging to the collective" (1935a: 56; Eng. 40).

All of the other knowledge and statements within a system of belief corre-
pond to the passive linkages: they are connections which inevitably result
from the peculiar laws laid out in the active linkages, with reference to which
the scientist can feel 'passive'. For him they are the objectively ascertained
reality.

Science is thus a system of opinion for both Twardowski and Fleck. For
'judgments' on Twardowski's view are nothing but subjective opinions, the
attributing of properties to represented objects, intended by individuals.
Here, too, Fleck replaces the individual with the collective. Science thus
becomes a system of opinion not of individuals but of collectives. On the
other hand, Fleck agrees with Twardowski that all of the elements of the
system of opinion or judgment mutually ground one another, thereby neces-
sarily coming up against a limit at which they have to rely on non-grounded,
posited assumptions. The answer to the question of what constitutes this
limit became the theme of Fleck's epistemology.

2. CAN KNOWLEDGE BE RELATIVIZED TO FORMAL-LOGICAL
DEDUCTIVE STRUCTURES? KAZIMIERZ AJDUKIEWICZ'S
'RADICAL CONVENTIONALISM'

Kazimierz Ajdukiewicz, known primarily for his work in logic and especially
in linguistic analysis, is the youngest of the three philosophers of Lwów whose
influence on Fleck we are investigating. At the beginning of his career, or, to
be more precise, from 1926 until 1936, Ajdukiewicz developed and main-
tained a theory which he later rejected and which for that reason has largely
been ignored: radical conventionalism (published primarily in Erkenntnis).

Ajdukiewicz's purpose in this attempt was the clarification of the role of
language in the cognitive process. He understands cognition (Erkenntnis)
here as the 'assigning of meaning' to a linguistic expression, which results in
accordance with the semantic rules of the language used. Cognition, or now
more precisely: the meaning of statements, is to be explained as determined by the language's structural properties — and that in a radically nominalist fashion, without any semantic dimension: linguistic structures are strictly formalized deductive systems founded on axioms which can be precisely specified.

Since these axioms remain explicitly unrelated to empirical reality, even when statements about this empirical reality are formulated, Ajdukiewicz believes that their free choice and thus also their free exchangeability must be recognized. Statements about empirical reality can thus only make the claim of grasping it. In fact they cannot attain it: they remain totally independent of it, their claim forever unrealized. Consequently no authority on the basis of a certain linguistic structure could be determined to produce 'true' statements. Rather, it must be recognized that various languages exist side by side with their own peculiar structures and cognitive claims. Cognition is thus relativized to formal-logical, deductive structures. In other words: radical conventionalism is the attempt to ground a 'metaphysics-free metaphysics.'

As Ajdukiewicz admitted in 1936, he failed in this attempt: the 'formalized structural methodology' which he chose as the basis of his conventionalism involves implications which contradict what the relativism of cognition, reality and truth, based on it, proclaims. Ajdukiewicz was not prepared to extend this relativism to methodology as well. The resulting problem is shown particularly vividly in the example of the principle of contradiction contained in this methodology: trying to account for the observed phenomenon of varying truth assertions, this principle on the one hand did not allow him to admit such statements to be mutually contradictory. Instead of doing so, he assigned them to different linguistic systems, each of which is closed and completely foreign to the others, and therefore each is unable to clash with any other. Such an existential assumption about such linguistic systems, however, can on the other hand not be justified on the basis of the chosen, formalized deductive methodology.

With this explanation, Ajdukiewicz distanced himself in 1936 from his conventionalist position. The contradiction which he classified at this time as insoluble was, however, known to him earlier. Radical conventionalism was precisely his attempt to solve it: it was for precisely this reason that he limited himself to statements about language, not making any statements about 'reality' (1953b: 179).
The Radicalization of Radical Conventionalism

Fleck's aims, however, went considerably further: he rejects a formal-logical deductive methodology precisely because it implies the ontological assumption of a reality independent of the thinker. As will now be shown, he did appropriate the essential claims of conventionalism, but he replaced Ajdukiewicz's methodological foundation with a sociological one. He is thereby able to take into consideration the methodology with the help of which cognition is won, in the relativizing of cognition and knowledge. Instead of tying the semantic content of statements, i.e., cognition, only to linguistic structures, Fleck links both semantic content and linguistic structures to the social and historical conditions of the people who are the subjects of them. This in particular allows him to offer an explanation for how the language is determined in which those cognizing choose to express their cognition. Ajdukiewicz presumed that any number of them could be produced. Fleck, however, sees them to be socially and historically determined. Because Fleck explains the differences in the endowment of meaning on the basis of differences in the social groups producing and using language instead of on the basis of differences in the linguistic structures grounding languages, it is the collective thought-style, and no longer the language, which controls this endowment of meaning.

Both concepts — Ajdukiewicz's 'linguistic system' and Fleck's 'thought-style' — show similarities in their most essential characteristics: to both authors they are examples of a holistic orientation in which each element is connected to every other. Every change in one of its elements therefore leads to a meaning shift for the system as a whole, and thus actually to the changing of the linguistic system or thought-style. Understood in this sense, a transformation of meaning-conferral by language is what is meant by the continued development of scientific knowledge.

Ajdukiewicz and Fleck can also speak with mutual agreement about the incompatibility and incommensurability of different linguistic systems or thought-styles: they exist next to one another without the members (speakers) of various thought-collectives (languages) being able to understand one another. For this reason they can neither be compared to one another, because there is no criterion independent of them possible, nor be brought to agree with one another, because this would mean changing the semantic context of at least one of them.
Cognition: The Constrained Solution of a Problem

Above all however the parallel of Fleck’s conception with Ajdukiewicz’s is most striking in Ajdukiewicz’s understanding of ‘cognition’ as the ascertaining of ‘passive linkages’ which constrainedly come forth from the ‘active linkages’ posited. The vocabulary here is reminiscent of conventionalism, even if Ajdukiewicz himself does not use these terms.

For Ajdukiewicz it is the language that determines what cognition is, i.e. those propositions that can be formulated in a given language. All statements are elements of a holistically understood linguistic system. Every element is connected to every other; no element can be changed without changing the entire system.

In this system, cognition builds upon the semantic rules which constitute the language: i.e., the posited rules which must be recognized in themselves if one is to use this language. These are axiomatic semantic rules, which indicate the “class of propositions which must be recognized in themselves”, deductive semantic rules which stress certain relations between propositions as being conclusions, and empirical semantic rules, which point to experiential data as expressed in certain propositions of the language.

For Ajdukiewicz, it is more precisely the language constituted by these posited rules that determines what cognition is: namely, propositions which, when confronted with experiential data, are forced on us by these presuppositions as their consequence. Such a constraint concerning some cognition in particular takes place when the linguistic foundation posited, the conceptual apparatus, is sufficiently well-formulated to unambiguously answer the epistemological questions raised concerning the perception of experiential data. Cognition is thus the solution of a problem which is uncovered in the rules posited by the conceptual apparatus.

Quite analogous to this, Fleck’s definition of ‘cognizing’ is “to ascertain those results which must follow, given certain preconditions (1935a: 56; Eng. 40), resulting from the presuppositions of a thought-style given by its active linkages. Here, too, this ‘constrained cognition’ is the answer to an epistemological problem, the ‘dissolution of a problem’. For as long as a thought-style determines cognition, there is always “only one style-adequate solution to any given problem. ... Such a style-adequate solution, and there is always only one, is called truth” (1935a: 131; Eng. 100). If we are correct in claiming an analogy between Ajdukiewicz’s and Fleck’s ideas in which cognition is to be understood as the ascertaining of a constrained solution to a problem, this analogy must also be the case for the presupposed concepts
which both authors apply in this connection. I have already discussed the conceptual correspondences between Ajdukiewicz’s ‘language’ and Fleck’s ‘thought-style’. What now remains to clarify is the way in which both authors deal with the question of truth.

Like his teacher, Twardowski, Ajdukiewicz holds firmly to the idea that in addition to the reality described by cognition there must be another level of it, independent and immutable. Even more clearly than Twardowski, Ajdukiewicz rejects only the idea that this ‘empirical reality’ could ever be reached by us in cognition. All of our statements about this objective reality always remain mere claims concerning what we represent as reality. In the same way, every cognition always remains a truth claim relative to the language on which it is founded; to speak of ‘truth’ itself is meaningless. Fleck distances himself from this, in so far as he rejects any notion of a reality independent of cognition, making reality, too, relative to the thought-style of the cognizing individual. The same thing goes for ‘truth’; if there is no possibility to cognize such a reality, he argues, it is useless as a category (1929b: this vol. 45-57). It is rather the case, he says, that reality is formed in accordance with the activity of cognition, and the cognizing individual correspondingly changes that which is seen as truth. With such an unambiguously stated judgment against a reality independent of cognition, Fleck does not need to draw any distinction between a ‘truth claim’ (or the ‘positive assertion of a judgment’) and the ‘truth’, as did Ajdukiewicz: by definition both are the same in his conceptual understanding.

The Role of the Empirical: Is There a Subject-Independent Reality for Fleck, Too?

But what happens to the role which Ajdukiewicz assigns to that independent reality in the process of cognition in spite of the fact that is is unattainable? Indeed, it has no real influence on cognition, says Ajdukiewicz: its task consists rather in choosing those statements, from all the statements possible in a language, which accord with the world-view developed on its foundations. It thus decides which propositions of a language will be judged to be true. The constraining solution to a problem presents itself in its confrontation with the ‘experiential data’ that comprise this reality. It is in this way that the unambiguous answer which is considered to be cognition first arises.

Fleck argues against Ajdukiewicz that we cannot speak in any meaningful way about an independent empirical order if that order is by definition never a part of cognition. In spite of Fleck’s clear formulations, however, it is
precisely here — on the question of the role of the empirical in the cognition-producing affirmation of the style-adequate solution of an epistemological problem — that we reach the point at which Fleck must contradict himself, or at least becomes very unclear.¹

For, against his own postulates, Fleck, too, ultimately assigns an empirical reality the task of deciding, independently of the presuppositions of the thought-style, what is to be ascertained and recognized as a style-adequate solution, as a style-adequate passive linkage, and as a ‘fact’. Exactly as Ajudkiewicz theoretically formulated it, Fleck describes in his examples how the conceptual apparatus of the thought-style are confronted in research with experiential data in order to ascertain the constrained result of cognition on the basis of the foundation of the conceptual presuppositions and the empirical data. Thus he gives the example of how the atomic weight of hydrogen is determined to be 1.008 (1935a: 110; Eng. 83): one of the presuppositions which first play a part in the cognition as ‘active linkage’ is the (arbitrary) posit of oxygen’s atomic weight as 16. Other such ‘active linkages’ are the definitions of the oxygen and the hydrogen atoms. With such definitions it is determined what is being perceived to be such atoms in the first place. They are thus posited rules, which limit and define certain experiential data, Gestalten, given in perceptions — and fix these experiential data as expressed by the definition.

For Fleck, as for Ajudkiewicz, the cognizing individual is free to correlate definitions — propositions — with any sense data he chooses. What is posited are thus presuppositions essential to the quality of the passive connections ascertainable on their basis. Not only for Ajudkiewicz, but apparently also for Fleck the connections which can be ascertained are more than simply the result of the mutual relations of definitions. For adding together, as we always do, of the definition of an oxygen atom, the fixing of its weight as 16, and the definition of a hydrogen atom, apparently only leads to the fact that the concept of ‘the weight of the hydrogen atom’ is also a meaningful term within the framework of the chosen thought-style. The preceding correlation of the sense data and the term ‘the weight of the oxygen atom’ now prescribes how the sense data which, in conforming to the style, are called ‘the weight of the hydrogen atom’ are to be delineated within the totality of perception. Determining it to be 1.008, however results only after a renewed determination of the sense data has been given. The number is thus also determined on the basis of these empirical data, which are indeed delineated by the thought-style, but not subject to any further influence.

In other words: what is possible as cognition is qualitatively determined by
the cognizing individuals on the basis of the presuppositions of the thought-style but it is quantitatively filled in on the basis of Gestalten delineated by the same thought-style in perception.

The difference between Fleck and Ajdukiewicz in all of this lies in Fleck's thematizing the delineation of experiential data in the perceptual manifold as a problem: in Ajdukiewicz's terminology, he differentiates between the 'situation' in which the language (thought-style) desires the recognition of empirical statements and the perceiving of sensuous perceptions. For perceiving is still formless; it is the observations of Gestalten conforming to style which are first judged. As the same time, however, the experiences gathered even in these attempts by the researcher to give form to these observations have an effect on his presuppositions. Concepts and the observations which are possible with their help are thus adapted to one another in both directions. 'Reciprocity' in this sense, however, presupposes two initially mutually independent categories which have an effect on one another —, the cognizing subject (or collective) and the reality of that which is to be known. The delineation of Gestalten in the perceptual manifold presupposes that there is 'something' in which I can delineate. This is quite obviously something independent of cognition, although Fleck never says this, precisely because it can never be a part of significant observations 'in itself', but must always be 'stylistically re-formed'.

In spite of all his assertions to the contrary, Fleck thus remains with the same dilemma characteristic of Twardowski and Ajdukiewicz: the question whether we can assume a reality independent of cognition to which cognition is directed, or whether reality can be completely relativized to cognition. Or did Fleck simply leave that independent category unmentioned because any mention of it suggests that it — being mentionable — is perhaps indeed subject to cognition? 'Truth' however can only be attributed to the reality of which we have cognition: to the reality formed by the thought-collective's possibilities of expression.

3. IS THE MANY RATIONALLY PROVABLE? LEON CHWISTEK'S PLURALITY OF REALITIES

Leon Chwistek belongs to the ranks of the most unusual and most versatile members of the Polish intelligentsia before and during the twenty year period between the wars: his work spectrum extended from painting and art theory to reflections on the problems of architecture and poetry and to important contributions to mathematical logic. All his life he fought for
recognition, but he was discouraging rather than convincing to his contemporaries. Outside Poland, Chwistek is especially known for his contributions to logic, in particular to his theory of types.

Until today, however, it has been overlooked that he also worked on philosophical questions concerning a very different question: the question of reality. Just as with Ajdukiewicz, Chwistek wanted to give a rational explanation, as he called it, for the phenomenon of differing reality claims. In opposition to Ajdukiewicz, however, he abandoned the notion of a reality independent of experience from the very beginning. He tried to show in his logical works that this is precisely what cannot be shown as an existential conclusion. Since there is no logical justification for it, it is in his view metaphysical, and thus to be rejected. Chwistek links the concept of reality to experience instead, which he viewed strictly nominalistically: since there can be no reality independent of experience, we must also abandon the assumption that there is only ‘one reality’. His theory of the ‘plurality of realities’ claims that there are basically four ‘fundamental types’ of reality schemata, corresponding to four different kinds of experience. They represent systems consistent in themselves, constructed upon different axiomatic foundations (independent of Chwistek’s failure, admitted by himself, to convincingly show the axiomatization of these systems).

On this foundation Chwistek then developed his relativistic concepts of reality and, in parallel, of experience. He links the two together: ‘reality’ presents itself to us in schemata which are always interpretations of one of the four ‘fundamental types of reality’. In the schemata, the reality recognized at the time comes to a concrete presentation. Chwistek defines ‘cognition’ in this context as the knowledge of objects available to the human mind through its experience or its understanding. With the application of logical-deductive operations, however, the understanding provides only tautological re-formations of experience. ‘Reality’ is therefore that which is given to our experience at any given time. What can be experienced at any time depends on the axiomatic assumptions of the foundations of the reality type accepted at that time.

Chwistek distinguished the ‘reality of impressions’, the ‘reality of representations’ the ‘reality of things’ or ‘natural reality’ and ‘physical reality’ as these four ‘fundamental types of reality’. Chwistek considers each of these experimental or reality complexes to be a holistic, self-consistent system, each of which has its own network or meanings.

Just as all of the philosophers from Twardowski’s school, Chwistek, too, attempted to ‘eliminate’ all metaphysics and to help ‘rationalism’ achieve its
ultimate break through for all times. He explicitly labels his approach ‘critical rationalism’ (1935: 3). But his rationalism can no longer rely on any ontologically independent reality. Still he maintains that the four reality types he postulated are thoroughly rational, while any other claims are nothing but ‘irrational metaphysics’.

At first Chwistek tried to prove this rationality by showing that the schemata in which reality presents itself to us are logically constructed. But even in the later phase of his creative work, when he abandoned the hope of basing schemata wholly on the validity of formal logic, he maintained his claim: the rationality of reality which does not display itself in the schemata of logic is now seen to be guaranteed by ‘common sense’, a concept which he leaves further undefined. With its help, the schemata are constructed by the subject — in a way which is not further open to question. The affirmation that we have to do with four ‘fundamental types’ of reality schemata is also supposed to rest on ‘common sense’. And precisely because they are given by the rationalism of ‘common sense’, these four fundamental types of reality cannot “be conditioned and dependent on human imagination” (1921: 59).

It is ultimately the grounding of the semantic foundation which Chwistek worked out to secure formal logic and mathematics. (Certainty and clarity in logic and mathematics can only be obtained, according to Chwistek, if their expressions are analyzed, instead of being used in a purely intuitive way.) A theory of expressions is therefore the presupposition of the complete formalization of deductive systems. In such a theory, which Chwistek called semantics, the notions of mathematics and logic must be defined with an appeal to structural, semantical concepts. He therefore also described this approach as ‘semantic’ or ‘rational meta-mathematics’.

**Reality and Experience**

One of the most important conceptual agreements between Chwistek and Fleck is the linking of reality to experience. Like Chwistek, Fleck disputes the category of a reality independent of experience. A fact — understood as an element in reality — is described by Fleck as the affirmation of an experience. ‘Reality’ consists of an entire ‘network’ of such experiential affirmations. And this experience is possible, as is the case in Chwistek’s view, in many ways: there are various possibilities of experience, which do not correspond to one another. On Fleck’s view they correspond to the thought-collectives which ‘create’ them. ‘Experience’ thus becomes a necessary condition of ‘cognition’, as is also the case for Chwistek. Fleck’s thought-style takes the
place of Chwistek's schema: it orders what is perceived as a fact of experience. Thus it determines what is to be considered reality at any given time.

Fleck does differ from Chwistek in so far as he does not only see 'reality' as determined by 'experience' but sees both categories in a reciprocal relation: 'Reality' also has an influence on what becomes 'experience', just as it itself grew out of experience.

What the thought-style or schema indicates as 'experienceable' at any given time depends, on both Fleck's and Chwistek's views, on axiomatic suppositions. Fleck calls them 'active suppositions'. They provide the foundation on which a thought-collective constructs a closed 'system of opinion', by means of its thought-style: a system of assignments of meaning, internally consistent, which can only be convincing in its proofs within itself. One must "believe" in its foundation if one wishes to make use of it. Quite similarly, Chwistek referred to the limitation of all arguments by virtue of the assumptions at their foundation (1923: 186).

Chwistek and Fleck also agree on the bond of 'truth' with the reality at hand: with absolute reality, every absolute truth is also disputed. 'Truth' is on the contrary only possible within the framework of a given axiomatic system. It must thus be accepted that deviating truth claims exist side by side, each having just as much right to exist as the other. A change from one reality into another therefore means a 'revaluation' (Chwistek, e.g., 1921: 58), or a 'shift in meaning' (Fleck, e.g., 1936: 79–112).

We can, finally, also speak of an agreement between Chwistek and Fleck concerning the relation between cognition and perception as they see it: "We detach (occurrences)", according to Chwistek (1935: 189ff.), "in an artificial way from the totality of experience, which we cannot grasp." Chwistek calls this process 'schematization': formal structures are interpreted into perceptions, which he here calls the "totality of experience, which we cannot grasp." It is we who constitute what we describe as known objects, by means of our experience and the schematization it determines.

Fleck calls the process of interpreting the unordered whole of perception 'style-adequate seeing of Gestalt'. It is the thought-style which first enables us to posit fixed points in the chaos of perception. It is by means of the thought-style that it can first be ordered into recognizable contours. These contours are externally imposed on perception; we have them at our disposal in our experience.
The Dispute Concerning the Rationality of Foundations

On the level of their systems, then, on which both authors formulate concrete statements concerning the complexes of experience and reality, there is a considerable identity in their intentions. But this is not at all the case concerning the foundations of these statements: here our authors are very different, Fleck explicitly distancing himself from Chwistek's intentions, at least indirectly. The cause for this lies in the fact that two questions which are essential for Fleck must remain unanswered by Chwistek, namely:

- the question concerning the factors which direct the construction of a reality-determining schema toward some concrete content or other, and allow the subjects to change these realities,
- the question of the reason for the differentiation of relativistically understood realities and the four unconditioned 'fundamental types' of realities under which the former can be ordered.

It is only to the first of these two questions that Chwistek gave any kind of answer: the 'subjective perception of truth', 'direct individual experience' or 'very strong internal commands' are his most important statements. It was only after his more exact confrontation with Fleck's theory of thought-style that he attributed his schemata to the 'social life of men' (1937: 16).

The differentiation of the four 'fundamental types' of reality, and the construction of the reality schemata to be ordered under them at any given time, is grounded by Chwistek very differently: they are founded 'reasonably' through 'critical rationalism'

- first of all by the unmediated application of 'common sense', with which the subject forms his experience and constructs the schemata of reality,
- secondly by 'rational meta-mathematics', with which our understanding supplies linguistic-logical equivalents to the elements of experience. But the elementary assumptions of rational meta-mathematics are also grounded only in common sense, which must itself ultimately remain ungrounded.

Fleck directs himself against precisely this foundation with which Chwistek tries to legitimate the rationality of his statements relativizing reality and cognition. In doing so he, too, remains loyal to the trend of his times in opposing metaphysics and, true to the 'anti-irrationalist current', in defending rational explanations. Thus for example he rejects the 'ideal of absolute truth' at one point with the words: "What is the use of awkward metaphysics, if tomorrow's physics will transcend all phantasy?" (1929b: this vol., 57). And he defends himself carefully from the suspicion that he is going back to 'metaphysical' elements with his conception of cognition (1935a: 56; Eng. 42).
All three of the philosophers discussed here explicitly claimed to provide ‘rational’ foundations for cognition and knowledge: Twardowski saw them rooted in the description of psychic phenomena, Ajdukiewicz in the methodology of formal logical-deductive systems and Chwistek finally in a semantic meta-mathematics and in common sense.

Fleck rejects these foundations of rationality — but he maintained the claims made on the basis of them. The approaches of the three authors discussed are in his view not able to ground these statements rationally. He then uses the same argument against them which they used in their criticism of others: they are lacking in ‘rationality’ and remain imprisoned in ‘meta-physics’. Even if Fleck does not once mention any of Lwów’s three philosophers in his publications, the direction of his thrust is unambiguous.

Against logic, Fleck repeatedly raises the objection that an axiomatization of knowledge is impossible: to get knowledge, theoretical and practical experience are indispensable. This would however always bring . . . “into knowledge an irrational element, which cannot be logically justified” (1935a: 125; Eng. 96). This is why it is impossible for Fleck to accept a formal logical construction of knowledge. It is not only because non-formalizable elements come into play here: it is also never clear which ideas (conceptions) of a system of opinion (thought-style) are to be regarded as foundational, and thus as axioms, and which as conclusions, for they can always be exchanged one for the other.

This is also why a ‘scientific fact’ can never by understood as a ‘logical conclusion’ (1935a: 73; Eng. 53–54): scientific cognition is always ‘involved in becoming’ and for that reason logically uncontrollable. But a logical grounding of an affirmation, even after the fact, can in some sense be a criterion for that affirmation’s correctness. In his post-war essay, “Problems of the Science of Science” (1946b), Fleck depicts how an internally consistent scientific edifice can be constructed on the basis of certain assumptions which are thoroughly false from the viewpoint of an expert. It is for precisely this reason that the inner, formal logical construction does not tell us anything about the value of cognition’s claims.

Fleck had already criticized all of the over-estimations of formal logic’s possibilities in 1927 in his first work in the philosophy of science: in medicine in particular the connections between the phenomena to be explained have a great many levels. Every logical structure which is applied to this complex conglomerate of phenomenal inter-relations falls short for that reason. Therefore, as Fleck generalizes later, formal logical structures are never anything more in the development of phenomena than constructions external to the
phenomena to be explained. They cannot claim any more explanatory power than any other construction conforming to a thought-style. Only one who overlooks the arbitrary fashion in which a decision is made about one of these constructions in respect of the object observed can identify this objectification-strategy of the fact with actual objectivity (1935a: 189; Eng. 144f.).

Fleck does not leave it only with this criticism of logic. In other texts, he also sets the role of language straight as it functions in cognition. This applies to both Ajdukiewicz and Chwistek: in a strictly nominalist way, both identified cognition grounded on formal logic with linguistically fixed expressions. Chwistek did go further than Ajdukiewicz in doing so, since he not only regarded this relation between logical structure and language as conventional but felt that language should be considered as constitutive for logic:
• In the first place, Fleck opposes the conventional view of the connection between expression and the expressed. Linguistic expressions of thought are not chosen conventionally, but because they are perceived as sounds as corresponding to what is to be expressed (1935a: 39; Eng. 27; also 1936: 94).
• Secondly, Fleck argues primarily against Chwistek: Linguistic expressions, language as a total system, cannot be constitutive for logic because they lack the necessary unequivocalness. What is decisive in this argument is Fleck’s emphasizing the fact that this property of language is not only an evil which grows in practice, but one necessary to the genesis of knowledge: linguistic ‘misunderstandings’ become for him the condition for epistemological progress (cf. above). But logic can only refer to unequivocal propositions — and therefore be a truth criterion of expressive force within at most one thought-style.

Fleck also rejects Chwistek’s concept of ‘common sense’ as the source of rational explanation. He treated this concept most extensively in his post-war essay ‘Problems of the Science of Science’ (1946h). Here he portrays how a collective cut off from contact with the professional scientific world, and comprised of laymen in the field, constructed its own, internally coherent theory. At first it did not have the solid framework of a thought-style. ‘Thinking’ however is, on Fleck’s view, impossible without the security of certain presumptions (active linkages). If they are lacking, they must first be formulated. If the thought-style does not provide them, they must be obtained (taken) from other realms of thought, other thought-styles. The innovative character of the intercollective communication of thought rests upon this. The ‘collective of everyday’ personified in common sense (1935a: 143; Eng. 112) plays a special role here, because practically everyone belongs to it.
The role of 'common sense' as Fleck sees it, seems to be very similar to the role it plays for Chwistek: for Chwistek 'common sense' always takes over when a logical schematization for cognitive processes is impossible. Thus it is of assistance in the case of orientation into new areas of knowledge, for which there are as yet no worked out conceptual apparatuses. Or at least it intervenes in the choice of the proper schemata for a given cognitive purpose. It is thus a resource which can always be used when there are no orienting thought-schemata available.

But Fleck energetically opposes Chwistek's view that 'common sense' in the latter's understanding is to be distinguished from everyday language and from vulgar 'horse sense' understanding of common sense (Chwistek 1935: 24), and that it must be regarded as the category which fights against metaphysics and protects us from such metaphysical claims as that of non-relative truths (1935: 229). This kind of refuge is unthinkable for Fleck. The 'principles of common sense' are just as little isolated from constant change by social and historical processes as are formal logical structures. 'Common sense' as Chwistek understands it thus loses all meaning: it, too, can only be understood as a product of social and historical forces. Only under this condition can it be at all effective as a resource which offers some orientation when a thought-style (or thought-schema) lacks certainty. According to Fleck, then, it must be bound to a social carrier just like all other thought-styles. This social carrier is found in the 'collective of daily life' — in that from which Chwistek so wanted to distinguish common sense.

Only one who takes the social and historical conditionedness of all thought and knowledge as the systematic starting point of his epistemology, however, can claim to offer a rational explanation. Fleck's emphatic claim to make rationally legitimated statements can only be understood against the background of the philosophy which dominated his surroundings and with which he argued. For otherwise why should Fleck — who always emphasized the at least apparently rational quality of scientific knowledge, i.e., in the framework of a thought-style which itself can never be legitimated as rational — exclaim: "(The sociology of thought) will enable us to rationally direct the intellectual life of societies. Instead of the philosophy of life which undergoes constant changes and is dependent on moment and place, it will provide the idea of the mechanism of the formation of views. Instead of what separates it will point to that which will be common to all, and which brings them closer together" (1947e: 84; this volume, 151).

The sociology of thought attains its rationality as comparative epistemology
from the breadth of experiences which it is able to consider. It is no longer bound to only one standpoint, but rather opens the understanding to various, contiguously existing orientations. For this reason this epistemology even becomes a 'duty': "A rule of thought which allows one to make use of more details and more compulsory connections, as the history of sciences teaches us, deserves to be emphasized" (1935a: 34, Eng. 22). Whoever moves away from this viewpoint strays, on Fleck's view, into irrationalism.

4. SOCIOLOGICAL THEORY OF KNOWLEDGE AND PHILOSOPHICAL THEORY OF REALITY

Today Fleck is considered above all a classical figure in sociology, as one of the early founders of the sociological way of observing a phenomenon which before then was only investigated philosophically: scientific knowledge.

In this connection the statement made at the beginning — that the intellectual context of communication within which he developed his approach was primarily philosophical — must be at least surprising. But this statement was not meant to detract in any way from Fleck's sociological accomplishment. On the other hand, sociological concepts cannot come out of nothing, especially not if we take Fleck seriously.

It is well known that neither the European sociology of knowledge nor the negative example of the Vienna Circle's epistemology can have been Fleck's sources. The literature which he explicitly quoted clearly shows that he knew both only superficially. These circumstances, and in particular the biographical information we have obtained, were the reasons for investigating the philosophers active in Fleck's immediate surroundings, in order to understand their role as intellectual stimulators to the development of his thought. At the heart of this investigation, besides the concepts these philosophers developed, were the unanswered questions and difficulties which they attempted to solve. For the thesis of this article was that Twardowski, Ajdukiewicz and Chwistek influenced Fleck especially by the problems which stood at the heart of their own intellectual efforts. The unanswered questions within their attempts stimulated Fleck, and also determined the content of his work in developing his theory of thought-style and the thought-collective. Fleck took up their questions — and changed them, by placing them in a different framework. He constructed this framework, his new systematic foundation from which these questions would be led to answers, from the concepts taken from these philosophers, and from those available to him with his slight systematic knowledge of the sociology of knowledge: he sociologized the theory of
knowledge and the philosophy of science in order to put the philosophically problematic categories of ‘reality’, ‘cognition’ and ‘truth’ in a new, unified, light from this perspective. The sociological way of looking at things was a welcome instrument to him, with the help of which he hoped to be able to make himself heard in an originally philosophical discussion.

It was forty years after he worked out his ‘theory of thought-style and thought-collective’ that Fleck first became known as a great sociologist, as the author of a sociological theory of knowledge and a sociological philosophy of science. It was not much noticed that he was also a philosopher, the author of a philosophical theory of reality. This comprises the heart of his approach. With it he takes up the nominalist and constructivist orientations of his milieu, in order to prove the superiority of his theory concerning the same problems as were worked on by his predecessors: the attempt to set forth a thoroughly coherent concept of reality. And just as in the case of his predecessors, his approach, too, found itself insolvably confronted with precisely this problem: the tension between reality as something first completed by the work of cognition and something somehow existing before cognition remains in spite of all of his emphasis on the relativist standpoint also a part of his theory.

NOTE

1 I am especially grateful to W. Baldamus for the consideration of the following problematic.

CITED LITERATURE

For all of Fleck’s quotations see the bibliography of Fleck’s publications in the appendix to this volume (pp. 445-457). A complete bibliography of Twardowski’s works is given in Gromka (1938); of Ajdukiewicz’s in Czezowskii (1964) (again in Ajdukiewicz (1960/65), in English in Ajdukiewicz (1978) and of Chwistek’s in Estreicher (1971).

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This statement, taken from Ludwik Fleck's classic book of 1935, might be read as the guiding slogan of his whole enterprise. If the theory of knowledge is to bear fruit, he tells us, it must not be founded on some Phantasiebild of Science: some a priori definition, or 'demarcation criterion', like that which Karl Popper has always insisted on. (Popper's Logik der Forschung had appeared in the previous year.) Any epistemological theory developed on an a priori basis alone faced insurmountable problems: it would do no more than explore the consequences of some arbitrary initial conception, selected to indicate what Science must be, if it was to fit the prejudices of the individual philosopher in question.

Instead, the theory of knowledge should investigate what Science actually is: this meant studying the historical processes, modes of thought, linguistic styles, social organizations and institutions, channels of collaboration and publication, and relations with the larger community, out of which the 'discoveries' and 'truths' of Science emerge, and through which they become accredited as 'facts'. So Fleck put all a priori explorations aside in favor of investigating wirkliche Erkenntnis. In doing so, of course, he was breaking sharply — and consciously — with the orthodox philosophy of science of the 1920s and 1930s: notably, with the prevalent idea that philosophers are required to 'justify' Science, and to provide it with intellectual 'foundations'.

What is the historical context and philosophical significance of Fleck's new move? Like Wittgenstein, he turned his back on the Humean empiricism of Ernst Mach and the Wiener Kreis, and placed himself squarely in the tradition of Kantian, or 'transcendental' philosophy. As Kant had taught us, the philosopher's task is not to meet radical Cartesian doubts about the very possibility of knowledge. Rather, we should take the existence of
knowledge for a fact; and then we can investigate the 'preconditions' of that existence — by asking on what conditions such knowledge is überhaupt möglich, or 'possible at all'. The answer to this question will go beyond, and behind, the actual theories of the sciences: the conditions for the practical relevance and applicability of (for instance) Euclidean geometry or Newtonian mechanics are not themselves part of Euclid's or Newton's empirical-yet-mathematical system, nor can they be incorporated into it without changing its nature. But they form a legitimate subject of study for those who are interested in standing back and assessing the intellectual implications of such theories.

This task can be approached in two different spirits: either, in the hope of finding some new 'justification' of science, or in a more detached and 'scientific' frame of mind. On the one hand, one may start with certain anxieties about the intellectual status of Euclid's or Newton's work; and seek some way of allaying those doubts, by arguing that the 'preconditions' for the applicability of their ideas are either necessary, or at least good enough to resolve our problems. Or, on the other hand, we may begin with all the assurance we require about those systems; and go on to investigate their 'preconditions' for their own interest. The former approach encourages the normative ambitions that have formed one major element in the transcendental tradition, from Kant up to Popper: it assumes that we are, now, in a position to define the 'essential and necessary' nature of Science, in terms that will have permanent validity. By contrast, the latter approach turns the study of transcendental Preconditions itself into an empirical enterprise with corrigeable results: we need no longer assume that Science has an 'essential and necessary' nature, which is to be established deductively, but only an 'actual' (wirklich) nature which remains to be discovered as we go along.

Ludwik Fleck firmly opted for the second of these programs. Like Nietzsche, he was sceptical about the normative ambitions of the philosophers. The anxieties that philosophers display about Science are misplaced, and the assurances that they demand are inappropriate. So, the only defensible program for epistemology is the empirical one: that of showing how the sciences in fact operate, and in virtue of what preconditions any particular science (say, geometry or mechanics) has in fact come into existence.

The force of such questions may, of course, turn out to be quite complex. In some respects, the natural sciences may be as they are, because human beings have the kinds of nervous systems they do; in others, because they have developed the kinds of cultures and institutions they have; in others
again, because of the kinds of languages or channels of communication they possess. That being the case, we cannot deal with them completely or at once: instead, we must look and see what evidence is available to us for answering them. So, behind Fleck's idiosyncratic mixture of historical and sociological observations there lay (I would argue) a quite novel program for the empirical study of epistemics, which has reached fulfilment only in the last ten or twenty years. Forty-five years ago, Fleck's analysis may have been little more than a personenlich Wissehsfragment, which evoked little resonance, or gemeinsame Stimmung, among his contemporaries. But, today, the Denkstil that Fleck inaugurated has become familiar to historians, sociologists and philosophers of science alike; so that we who are gathered to discuss his ideas in this colloquium can, perhaps, be thought of as comprising Fleck's own posthumous Denkgemeinschaft or Denkkollektiv.

II

Seen from the 1980s, the most striking element in Fleck's account of science is this concept of the Denkkollektiv, or Denkgemeinschaft: nothing in his book resonates more completely with our own contemporary preoccupations. Before having encountered Fleck's book, for instance, I myself wrote in some detail about the ways in which the complex structure of scientific professions affects the modes of transmission and transformation of scientific ideas. So I read Fleck's elegant comparison of a scientific Denkgemeinschaft to a column of troops on the march — having its vanguard, its main body, and its stragglers — with a mixture of joy and shame: with joy because this comparison was so congenial with my own account, with shame because I did not previously know that Fleck had developed these ideas thirty years earlier than I did. Certainly, much of Fleck's analysis of the different human 'circles' in which any science is embodied (from esoteric front-line researchers, out to the general reading public) and of the roles played by different kinds of scientific publications (from immediate research reports, out to handbooks and textbooks) is a permanent contribution to our ways of thinking about the modus operandi of the scientific enterprise.

More recent debates have, however, carried this discussion some distance beyond Fleck's original arguments; and, by now, we are in a position to draw some crucial distinctions more clearly than Fleck ever had occasion to do.

(1) To begin with, it is necessary to emphasize the distinction between scientific disciplines and scientific professions more clearly than Fleck did. In discussing the social and historical character of the natural sciences today,
for instance, we would be inclined to agree with Fleck that, in certain essential respects, the activities of scientific research and criticism are collective activities, and that, to this extent, the enterprise of science is essentially a collective enterprise. In any well-established natural science, the 'existing fund of knowledge' (*Wissensbestand*) is never the sole possession of any single individual; and one can properly judge the significance of the ideas and innovations introduced by any individual scientist — even a Newton, a Darwin, or a Helmholtz — only by seeing what contribution they made to the collective understanding of that science. Furthermore, the collective character of the scientific enterprise, as Fleck insisted, has epistemological implications also; since it makes the 'current fund of knowledge' (*jeweilige Wissensbestand*) a third, indispensable element in all scientific knowledge, alongside the individual 'knowing subject' and the 'objects to be known.'

Yet there remains an unresolved ambiguity in Fleck's statement of this point. He describes his 'third element' in scientific knowledge in several different ways.

The statement, 'Someone recognizes something,' demands some such supplement as, 'on the basis of a certain fund of knowledge,' or, better, 'as a member of a certain cultural environment,' and, best, 'in a particular thought style, in a particular thought collective'

and, for epistemological purposes, it makes a lot of difference which of these formulations we adopt, i.e., whether we refer to a 'collective fund of knowledge' on the one hand, or to an actual 'community of persons' on the other. For instance, if we were to define the intellectual content of some scientific discipline, such as biochemistry, directly in terms of the inventory of ideas and procedures accepted among a specific *professional population* (sp., those scientific investigators who now call themselves 'biochemists'), that would drive us into a needless relativism. For, having adopted this definition, we should have no way of explaining what connects the 'biochemistry' of one generation with that of the next, once the earlier population of scientific investigators concerned has been entirely superseded. A definition of 'biochemistry' that subordinated the intellectual content of the discipline to its human embodiment in a particular professional group would, thus, substitute the personal affiliations of the scientists involved for the intellectual continuity of the scientific itself.

Alternatively, however, if we define the biochemical *profession* as the totality of investigators, organizations, laboratories, channels of communication etc. devoted to the specific *disciplinary tasks* of 'biochemistry,' no such
problem arises; nor need it arise on Fleck's own account, either, if we look at his position carefully enough. For, when Fleck unpacks the terms Denkkollektiv and Denkgemeinschaft in detail, he is careful to insert some crucial qualifications:

The concept of the thought collective . . . is not to be understood as a fixed group or social class. It is functional, as it were, rather than substantial.

Between two members of the same thought collective on the same mental level, there is always a certain solidarity of thought in the service of a super-individual idea.

The specific intellectual mood of modern scientific thinking . . . is expressed as a common reverence for an ideal.

The link between the different individuals within any Denkkollektiv lies, therefore, in their shared commitment to a common set of ideas and ideals: viz., the collective project, intellectual agenda, or research program of the corresponding discipline. The connection between successive Denkkollektiven within any given discipline lies, correspondingly, in the historically developing project, agenda or program that directs the professional activities of the discipline. So, by characterizing the term Denkkollektiv as "functional rather than substantial" and making "common service to, and reverence for, the ideals of the science" the unifying element in Denkkollektiven, Fleck acknowledges the crucial distinction between 'disciplines' and 'professions.' In doing so, of course, he also hands back to philosophy the epistemological question, "How are the unifying ideas and ideals, intellectual agendas and research programs of scientific disciplines determined?"; and this is the question that preoccupied most historically minded philosophers of science during the 1960s and 1970s.

(2) In one respect, however, the new understanding of the collective character of science pioneered by Fleck has irreversibly changed the demands we rightly impose on any answer to this epistemological question. In discussing the programs and ideals constitutive of any scientific discipline, philosophers can no longer focus in the way they once did on the ideas and agendas of individual scientists. The disciplinary goals and tasks of classical mechanics, or chemistry, or evolutionary biology cannot be defined simply by reference to the personal ideas and ambitions of (say) Isaac Newton or Antoine Laurent Lavoisier or Charles Darwin. In order for those individuals to become founding fathers of entire scientific disciplines, there had to be Newtonians, not just Newton; oxygenists, not just Lavoisier; evolutionary biologists, not just Darwin. So, the Denkkollektiven of mechanics, chemistry
and biology transformed the original speculations of each individual scientist into a collective agenda for the corresponding profession.

Once such an agenda has been clearly formulated, however, it quickly becomes 'anonymous' and develops a life of its own. The best way of discovering the agenda of a science may be to study the shared activities of the research scientists concerned; but the science will not, on that account, be the exclusive possession of those particular scientists. A game or sport is there to be played by anyone who learns its rules and tactics, and a piano concerto is there to be performed by anyone who is able to master the score; so, too, a science is there to be pursued and refined by anybody whose interests, imagination and ambitions are seized by its ideals and challenges. After a time, indeed, those ideals and challenges have a way of imposing themselves on people. Just as many subsequent composers have been tempted to 'finish' Schubert's Unfinished Symphony, so too the unanswered questions of a science have the power to attract would-be 'answerers' from outside the current professional population. As the familiar phenomenon of simultaneous discovery indicates, scientific problems that are 'ripe for solution' have a way of getting themselves solved: if not by one individual, then by another, and if not by an individual, then by a research team. If the social and cultural conditions are unfavorable, of course, this may take longer than it might otherwise have done; but that delay is a function of the general historical situation, not a comment on the particular scientists forming the current Denkkollektiv of the science.

To sum up: although a scientific discipline may properly be a 'collective' enterprise, its history is not identical with the social development of the professional organizations enrolled in its service during the period in question. On the contrary, the central questions confronting social historians of the scientific professions presuppose, and arise out of, the intellectual history of the corresponding disciplines. Suppose, for example, that we ask, "Why did scientific physiology develop along such different lines in nineteenth-century Germany and France?", or, "How are we to explain the differences in organization between associations of field naturalists, on the one hand, and professional societies of theoretical physicists or laboratory chemists, on the other?": Such sociological questions about scientific professions can be answered satisfactorily only if we already understand the subject matters of the respective scientific disciplines. So, however 'collective' they may be, the intellectual character of scientific disciplines still needs to be explained, first and foremost, in 'functional' terms — i.e., in terms of their intrinsic goals, ideals and research agendas — and the sociological (or 'substantial')
aspects of scientific work remain, as Fleck himself implied, secondary and derivative issues.

III

Fleck’s pioneer arguments have also helped philosophers to move the locus of epistemological debate beyond two dichotomies that have bedevilled philosophy ever since Kant. For it is now clear that we cannot adequately define the goals of the sciences, or map their historical development, exclusively in terms of (1) either the language of idealism or that of materialism, or (2) either the language of individualism or that of collectivism.

(1) Fleck himself did not take up any firm position about the relations between ideal and material elements in science, or about the exact links between theoria and praxis. As a result, his argument is not perfectly clear. In his narrative about the discovery of the Wassermann reaction, for instance, Fleck emphasized the divergence between the original goals of Wassermann’s investigation and his eventual discoveries — or, rather, what Wassermann was eventually seen as having discovered — and he used this divergence to reinforce his own views about the collective character of the scientific enterprise. But, in one important respect, the actual facts of the matter appear to be more subtle than Fleck himself recognized. The predominantly scientific research program, in the course of which Wassermann’s initial search was undertaken, had very different goals from the predominantly medical agenda for whose sake the eventual discovery was recognized and acclaimed. Wassermann and his colleagues had aimed to improve their physiological understanding of syphilis; but “the insistent clamour of public opinion” was “for a blood test,” i.e., a technique for diagnosing and treating the disease. And the technical refinement of the consequent serological procedures through the “collective experience” of many workers which Fleck emphasized, quickly led to the development of valuable new medical procedures, without immediately resolving the physiological problems from which Wassermann had begun.

This distinction, between the goals of physiology and medicine, is easy and safe enough to state; but it is not nearly so safe to treat it as cut and dried, or to raise it to the level of a formal dichotomy. The core of any discipline is its research agenda, along with the shared ideals by reference to which members of the relevant Denkkollektiv recognize ‘improvements’ in the discipline; but, on occasion, the agendas of quite different disciplines may substantially overlap, especially when they address the same domain of phenomena with somewhat different aims in view. Historically speaking,
for instance, the intellectual agenda of physiological science and the practical agenda of medical pathology have frequently overlapped in this way; and a highly original and active worker such as Wassermann was happy enough if he could make contributions to either of these two disciplines, or to both at once. So, though Wassermann began his work on syphilis in the spirit of a physiologist — i.e., with an eye to the intellectual research agenda of physiological science — he was happy enough that his work led to the development of fruitful new techniques in medical pathology — i.e., techniques that were fruitful in terms of and by reference to the practical research agenda of medical pathology. In itself, the invention of the serological test for syphilis left the physiology of the disease in as much darkness as ever; but this failure to make theoretical progress on the physiological front was largely compensated by practical progress on the pathological front.

More precisely: contrasting the predominantly intellectual goals of scientific physiology with the predominantly practical goals of medical pathology does not imply that physiology is concerned solely with 'mental ideas' or pathology solely with 'material transformations.' On the contrary, the two disciplines can be distinguished as having distinct research agendas, while still recognizing that both disciplines have intellectual as well as manual concerns. In actual fact, the contrast between the research agendas of the two fields is not a direct contrast between theoria and praxis, with one discipline engaged in theoria to the exclusion of praxis, the other in praxis to the exclusion of theoria. Rather, the contrast is one of emphasis. In pathology, short-term technical gains may be valued, even when they contribute little or nothing to intellectual understanding; in physiology, minor improvements of understanding may be valued, even when they contribute little or nothing in the way of practical techniques but neither the science of physiology or the art of pathology values theoretical ideas alone, or practical techniques alone, to the exclusion of the other. So, in real life, technologies always include some elements of theoria, natural sciences some elements of praxis: any sharp opposition between the two kinds of enterprises is, correspondingly, crude and unfruitful.

(2) A similar destiny awaits the dichotomy between 'individualism' and 'collectivism.' From a first quick reading of Fleck's book, one might suppose that he had come down irrevocably on the side of collectivism; but that conclusion is not supported by a more careful examination of his actual text. Certainly, it is the heart of Fleck's argument that new results can win established places in a science only when they become the joint possessions of the relevant Denkkollektiv — or, at least, of the 'esoteric circles' that
operate at the center of the professional sub-group directly concerned — so that an idea which has been entertained by only one person is scarcely yet a ‘scientific’ idea at all, far less an ‘established’ one. But this view neither destroys, nor even limits, the role of the original individual in science. Indeed, when Fleck writes about activities at the cutting-edge of new scientific work, as represented in (say) the weekly journals that print first-hand research reports, his analysis is stated in ‘populational’ terms, and he allows the personal thinking of each individual scientist its own legitimate place:

The various points of view and working methods [represented in ‘journal science’] are so personal that no organic whole can be formed from the contrary and incongruent fragments . . . Journal science thus bears the imprint of the provisional and the personal.

It is through the shifting and bringing together of these ‘personal and provisional’ presentations that selected innovations are incorporated into, and so modify the current Wissensbestand. In principle, accordingly, every scientific novelty that ends by winning a place in the collective Wissensbestand of the science concerned could ultimately be traced back to ideas that were initially the sole property of the individuals whose personal research first led to their tentative introduction.

Hence, there is nothing essentially collective about scientific ideas and techniques, beyond the requirement that they be open to scrutiny and criticism from the standpoint of a complete discipline. Occasionally, novel ideas first enter scientific discussion during collective ‘brain-storming’ by groups of scientists no single one of whom would in fact have hit upon those ideas by working alone; yet alternatively, and probably more frequently, scientific novelties enter discussion as a result of the personal speculations of individual scientists, and are first put forward publicly by those individuals on their own personal accounts. At the very least, then, such ideas must be capable of being considered and criticized equally, either by individual scientists or by groups of scientists working in collaboration.

If Fleck gave the collective aspects of science a certain priority over the individual aspects, he did so because, in his time, it was desirable to redress an existing imbalance, and to counteract the excessively individualistic and idealistic views of ‘scientific thinking’ that had been widely current before he wrote. A single football player, such as Pele, may have a very ‘individual’ style of playing: that is, he may be in the habit of doing things that no other player either does, or in fact can do. But he does these ‘individual’ things as contributions to that joint activity of twenty-two individuals, organized into rival teams, that we call ‘football.’ Likewise, a single musician, such as
Mozart, may have a very 'individual' mode of composition: e.g., he may be able to run over in his head whole quartet movements that he has never yet put down on paper. But he demonstrates these 'individual' achievements publicly only through the contributions he makes to the collective heritage of chamber music. For Ludwik Fleck, again, a single scientist, such as Newton or Einstein, may have a very 'individual' way of doing the things he does for physics, but the significance of each individual scientist's contribution can typically be understood and judged only with an eye to the shared goals and collective standards of the corresponding scientific discipline. Correspondingly, the Denkstrukturen that individuals build up in the course of their scientific training, and develop further during their personal thinking about scientific issues, will typically be 'internalizations' of the Denkgebilden constructed for the purposes of the corresponding professional collective.

To pick up Fleck’s metaphor again: as the Army of Science makes its way forward, there are usually individual scouts ahead of the vanguard, who make their way forward singly, and survey the terrain along the line of march. Occasionally, indeed, these scouts may even lose touch, not just with the main body of troops, but even with the vanguard itself. One example is Gregor Mendel. When Mendel first speculated about hereditary 'factors' transmitted from one generation to the next within interbreeding plant populations, he found no colleagues ready and able to consider his arguments and criticize them constructively: it took some thirty-five years for a Denkkollektiv to grow up that could digest Mendel’s contributions, and integrate them into the current Wissensbestand of biological science.

Arguably, however, Ludwik Fleck himself was another similar example. When Fleck first wrote Genesis and Development of a Scientific Fact, he too found very few colleagues ready and able to consider his arguments and criticize them constructively. Like Mendel, he was framing the Denksstil for a Denkgemeinschaft that was yet to come. So, though the collective activities of an entire Denkgemeinschaft or Denkkollektiv may make indispensable contributions, that is not the whole story, or the only story to be told by historians of science. There remain, also, a few occasions on which the thinking of individual scientists may succeed in sketching out—or even formulating in considerable detail—who new theories and methods; and, just once in a while, the results may even provide the foundations for whole new disciplines, which will subsequently provide materials, and define a Denksstil, for groups of scientists collaborating as members of a shared Denkgemeinschaft.
IV

Up to this point, I have discussed those features of Ludwik Fleck's argument that are most clearly relevant today. But, for Fleck himself, the central issue was the nature of scientific facts. This provided the title for his book; the problem stated in his prologue; the chief topic distinguishing Fleck's position from that of the Vienna Circle; and the questions to which he kept returning. At the same time, Fleck's account of the nature of 'facts' remains the most opaque part of his position. Obscurities remain, in particular, in his distinction between the 'active' and 'passive' elements in knowledge, and in his claim that the outcomes of science are 'inevitable' (zwangsläufig).

It is clear enough what Fleck objected to in the Vienna Circle account of science. The 'facts' that scientists are concerned with are not (Fleck argued) merely given to any human observer in naive, uninterpreted experience. The illusions and misperceptions of young children, primitive peoples, and the senile certainly have little claim on scientific attention by comparison with the observations of experienced scientists and physicians. No (Fleck replied, echoing Kant), 'facts' are what we make of our experience, through the exercise of judgement. But, going beyond Kant, Fleck immediately saw how the form of our scientific judgment is shaped by the socio-historical imperatives of a particular scientific and social Denkgemeinschaft: e.g., the "uniform agreement in the emotions" within a given society that "is called freedom from the emotions," and provides a basis for that "formal and schematic" style of thinking that "is called rational" in that society.

In this respect, Fleck's account of scientific facts resembled the views that have been presented, in recent years, by N. R. Hanson and, in an extreme form, by Paul Feyerabend: in particular, the thesis that all so-called 'facts' are theory laden, so that scientists who accept different theories will share no obvious common body of facts for comparing the relative merits of those theories. In some passages, indeed, Fleck seems to have shared this view entirely: as when he declared that "every fact must be in line with the intellectual interests of its thought collective" and insisted that the 'resistance' (Widerstand) in the presence of which we speak of 'facts' represents both a 'thought constraint' (Denkzwang) and also a directly experienced 'form' (Gestalt).

At this point, however, we must ask why such terms as zwangsläufig and Denkzwang were of such importance to Fleck. By way of preface, let me make two introductory remarks. (1) The essential thing about a 'fact,' for Fleck, was that it is perceived as beyond dispute. In this respect, his use of
the term coincides with the way in which it is used (e.g.) by lawyers, for whom the 'facts' of any case have to be agreed, stipulated or established, before genuine questions of 'law' can be addressed. (2) Once a 'scientific fact' is generally agreed upon (Fleck further observed) it comes to appear inescapable: a well-understood fact, it seems, 'could not have been otherwise'. Just as Kant recognized that the results of Newtonian mechanics can be at the same time both mathematically necessary and empirically true, Fleck too had to account for the seeming 'inevitability' of scientific facts; and he did so in the same way as Kant. A 'scientific fact,' Fleck declared, is a 'conceptual relation' (Begriffsrelation): a relation whose expression may be 'stylized' according to the requirements of a particular scientific thought community, but a conceptual relation nonetheless.

What is the force of this point? The 'facts' to which scientists appeal in support of their ideas and theories are not given in advance; rather, they are clarified, brought into focus and given definite verbal expression only through the theoretical refinement of the science itself. Before the development of the science concerned, there was no systematic way to 'make sense of' experience in this particular field of study: by contrast, the concepts and terminology of a scientific theory enable us to make sense of phenomena that were previously confused, mysterious or arbitrary. To that extent, the basic facts relevant to any scientific theory are built into the concepts and terminology on which we rely to 'make sense of' the phenomena. So whereas, for the Vienna Circle philosophers, the truth of any scientific theory rested on the 'correspondence' of its implications with previously given facts, Fleck saw this truth as resting, in general, on the theory's overall 'coherence' and, in particular, on the newly distinct facts which it enabled scientists to perceive — facts that as a result lent themselves, for the first time, to clear and distinct linguistic expression and, in doing so, gave clear and distinct form to our experience, also.

Fleck's problem about 'scientific facts' is of course, in some respects, still unsettled. But there is much in his view that remains congenial. Certainly, the virtues of a natural science are much less typically shown by its capacity to organize previously known truths into more rigorous logical patterns than they are by its power to make new sense of previously unintelligible phenomena — phenomena about which we could not previously say anything clear at all, true or false. The development of more powerful scientific ideas thus gives us a means of recognizing, and talking about, genuine or 'true' relationships ('facts') in situations about which we earlier had nothing to say, not even something false.
Yet it may be a mistake to react too strongly against the naive 'correspondence' theory of the Viennese positivists; and, in his pardonable enthusiasm, Fleck himself sometimes failed to draw important distinctions among the different kinds of 'facts' relevant to any particular science. A well-established scientific fact was either or both of two distinct features — preferably, both. (1) It may be accepted as beyond dispute for the reasons Fleck himself gives: because it expresses a conceptual relation actively built into the theories that shape the Denksstil current in the professional thought community of the science concerned. (2) Alternatively, it may be accepted as beyond dispute for a rather different kind of reason: because it expresses an empirical relationship that demands passive acceptance from all scientists working in that science regardless of their current theoretical views. (3) Finally, in a few cases, it may be so accepted for both reasons at once.

Facts that are accepted as beyond dispute for the first reason only are like those features of a terrestrial map that depend only on the mode of projection by which the map was produced: such as the fact that, in a map drawn to Mercator's projection, all parallels of latitude and longitude intersect at right angles. Facts that are accepted as beyond dispute for the second reason only are like those features of the Earth’s surface that no complete map of the world could afford to omit: e.g., the Eurasian continent or the Atlantic Ocean. Facts that are accepted as beyond dispute for both reasons are like those terrestrial features — e.g., the Equator and the Poles — that provide the necessary linkage between cartography and geography, the 'map' and the 'mapped.' Cartographical features of the first kind will strike us as entirely compelling, or even inescapable (zwangsläufig), for so long as we continue to think in the terms imposed by our chosen system of projection: so, we may be tempted to say, "Of course the parallels of latitude and longitude intersect at right angles," forgetting that this geographical feature is precisely the one that Mercator chose to preserve, when he devised his particular cartographical projection. But, equally, these same features will instantly vanish the moment we adopt a sufficiently different system of projection. If we adopt a projection that preserves the proportionality of similar areas at different latitudes, for instance, we shall at once lose the orthogonal intersection of parallels of latitude and longitude as a result.

Correspondingly, scientific facts which present themselves as inescapable within one Denksstil, or system of theory, may not remain equally inescapable when we change to a quite different system. We can find a good example in the theory of chemical combination. Elementary textbooks of chemistry introduce the theory of molecular combination by quoting two seemingly
factual, or empirical, laws — viz., the 'law of constant composition, and the 'law of whole numbers.' All true chemical compounds (we are told) result from atoms of different elements combining in certain fixed ratios, and these ratios always involve integral numbers of the elementary atoms: one atom of K with one of Cl, two atoms of H with one of 0, etc. That much is a basic 'fact' of chemistry, accepted by all chemists as beyond dispute. Much later on, however, we are introduced to certain complex, quasi-crystalline substance, which are referred to either as 'interstitial compounds' or as 'solid solutions.' In their case, the laws of constant, whole number composition no longer seem to apply — for instance, their chemical formulae may be presented as K\textsubscript{1.36}Cl. What has happened? Has the classical theory of chemical combination been abandoned at this point? Not at all. By this stage our theoretical point of view has shifted and become more sophisticated. Having fully mastered the Denkstil of classical chemistry, we can relax the 'thought constraints' imposed by the elementary chemical laws, and admit compounds with fractional compositions.

About this kind of example, Ludwik Fleck was absolutely right. The elementary laws of chemical composition were never fully empirical at all. Rather, they were built into the concepts of classical chemistry, which deliberately chose to differentiate 'elements' sharply from 'compounds' and 'mixtures.' In this respect, the ambiguous terminology used in describing such substances as K\textsubscript{1.36}Cl is significant. If we call them 'solid solutions', we assimilate them to 'mixtures' and so preserve the Denkstil of classical chemistry: if we call them 'interstitial compounds', we modify the traditional Denkstil and so throw overboard the traditional laws (or 'facts') of chemical composition. Moving from the simpler chemical theory to the more sophisticated one, accordingly, is like moving from a simpler to a more complex system of cartographic projection: in both cases, making the move simply 'abolishes' certain facts that previously appeared 'inescapable.'

Yet not all the facts within the scope of any natural science are, surely, accepted as beyond dispute for this first kind of reason only. There is, in the general field of chemical substances and their transformations, a whole domain of natural relationships waiting to be studied, which physicists and chemists are free to describe in a variety of quite different theoretical terms — either in terms of chemical valency, or in terms of interatomic force-fields, or again in terms of shared electron orbits or of complex eigen-functions — and a mere change of Denkstil will no more cause these natural relationships to disappear than a simple change of cartographical projection will abolish the North Pole or the Atlantic Ocean. True: as we switch from one Denkstil
(or theoretical schema) to another, certain *descriptions* of those physicochemical relationship may acquire (or lose) either their meaning, or their truth, or both. Yet, on a deeper level, we can surely say that elementary chemistry, molecular physics and quantum mechanics have, in turn, given us progressively more discriminating accounts of *one and the same* set of facts.

Fleck himself recognized that our scientific ideas are in continuous flux, and that the same phenomena are continually being brought into sharper focus, and articulated in more discriminating language. The electron microscope, for instance, has made it possible to study human tissues in more detail than was possible with the best optical microscopes: as a result, physiologists can now 'see' many new 'things' and 'recognize' many novel 'facts' about those tissues. Yet it is still the same human tissues that are being studied; and, correspondingly, many of the things and 'facts' revealed by the electron microscope are simply earlier-known things, or facts, which can now be observed and stated in new and more discriminating ways.

Only if we regard all scientific facts as being *facts of the first kind* — i.e., as being radically 'theory laden' — shall we thus be justified in saying that every new theory, or *Denkstil*, brings with a *totally new set of facts,* which has to be defined and described in its own novel terms. And there is no reason to believe that Fleck himself fully accepted that radical view. Rather, he seems to have regarded 'scientific facts' as *facts of the third kind* — i.e., as the point of linkage between the theoretical and the empirical, the *Denkstil* and the *erlebende wahrzunehmende Gestalt.* For he recognized the 'intellectual constraints' that are placed on our thought in the very act of accepting any scientific theory, and the ways in which the resulting theoretical interpretations of experience can come to appear inescapable. But Fleck certainly did not wish to cut the links between successive theoretical accounts of the same phenomena; nor did he imply that the observations to which any science refers are *radically* theory laden. Being a working microbiologist himself, he knew at first hand how often it was desirable, even necessary, to view the same specimens and the same facts — both practically and theoretically — 'in a new light'.

V

In conclusion, then, how do matters stand today in the area where epistemology intersects the history of science? How do our ways of analyzing and interpreting the *nature* of scientific knowledge influence our ways of viewing
and describing its historical development? If formulated in general enough terms, the answers to those questions take the same form, nowadays, as they have done ever since the beginning of the modern debate, viz. in the writings of Francis Bacon, and the most urgent outstanding problems requiring attention today remain what they have always been: i.e.

What kinds of things count as 'discoveries' in science?

What kinds of changes constitute 'improvements' in our scientific understanding of nature?

How, accordingly, does scientific knowledge 'grow' or 'increase'?

In contrast to such sixteenth-century humanists as Michel de Montaigne, who was sceptical about the very possibility of a theoretical consensus in science—after all, one could hardly expect the founder of modern individualism to accept also the primacy of the Denkkollektiv! — Francis Bacon was a Founding Father of modern scientific optimism, and his vision of science was a collective one from the start. More than anyone else around the year 1600, he helped to launch the conception that the natural sciences — both the 'pure' sciences, which were sources of intellectual 'light,' and the 'applied' ones, which bore material 'fruit' — form an essentially cumulative and progressive element in human culture, society and intellectual life. And, from the time of Bacon on, a central task for the philosophers of modern science has been to explain in just what sense scientific knowledge develops 'cumulatively,' and in just what respect scientific change is, correspondingly, 'progressive.'

The central issue, here, is often presented by underlining a seemingly absolute contrast between the natural sciences and the fine arts. In discussing the art of sculpture, for instance, we feel no obligation to assume that what is later must also be better: that Rodin (say) must have known more about sculpture than Michelangelo, and Michelangelo more, in turn, than Phidias. (I recall the shock I felt when hearing someone introduce a Haydn string quartet with the comment that it was "surprisingly good, considering how long ago it was written.") Yet we do undoubtedly tend to assume that, in some sense, Einstein did know more about the science of physics than Newton, and Newton more, in turn, than Aristotle. And the question philosophers still need to address is, "In just what sense is the science of physics cumulative, while the art of sculpture is not?"

We can, of course, counter the seeming absoluteness of the contrast. For, arguably, this statement of the differences between arts and sciences fails, as it stands, to compare like with like. Either we should compare the scientific knowledge and techniques developed in physics with the scientific knowledge
and techniques involved in sculpture; or else we should compare the creative arts and achievements of sculpture with the creative arts and achievements exemplified in physics. On this alternative basis, one could indeed assume that Rodin was in a position to know more than his predecessors about the properties and characteristics of different kinds of marble, and about the technical procedures available for shaping it; and we could indeed acclaim Isaac Newton's *Principia* (perhaps) as a supreme product of the physicist's art — its *Venus de Milo* or *Sistine Chapel*, say — unequalled as an individual creative achievement by any single product of Albert Einstein's pen and imagination. The essential contrast, thus, appears to be, not one between the fine arts and the natural sciences as such, but rather one between the general techniques that are developed and employed, either for artistic or for scientific purposes, and the particular works that exemplify the superior exercise of those techniques, whether they are produced by scientists or by artists.

Still, it remains the case that — at any rate, in the modern period — the primary emphasis of the fine arts has been more on the production of superior exemplars of artistic skill ('works of art') than it has on a refinement of the underlying techniques, or an improved understanding of the materials involved; while the emphasis of the natural sciences has been more on the refinement of general understanding and techniques, whether intellectual or practical, than it has on the production of uniquely notable and individual 'works of science.' So, in thinking about the kinds of knowledge and understanding with which the natural sciences are concerned primarily, and the fine arts secondarily, we can still ask in what sense the historical development of such knowledge and understanding is 'cumulative': i.e., in what sense changes in that knowledge and understanding are, indeed, 'progressive.'

In the last fifty or sixty years, the views of philosophers of science on this subject have ranged across a spectrum. At one extreme, the logical empiricists of the nineteen-twenties and -thirties, particularly those associated with the Unified Science movement, conceived a programmatic hope that quantitative indices for measuring 'good' or 'bad' science could be devised. Using those measures, we should be able to demonstrated the cumulative, progressive nature of scientific change, by showing (e.g.) that successful new systems of scientific theory always have a larger empirical scope than the systems of theory they displace, while the extent of their rational support (confirmation, corroboration, verification) is always greater. The empirical scope of scientific explanations and their degree of rational establishment, taken together, will supposedly provide the index of intellectual 'merit' required for judging
scientific 'progress.' At the other extreme from this severely classical analysis, a less mathematically minded group of philosophers, chiefly in the nineteen-sixties, reacted against the artificialities of the logical empiricists' position by insisting — in a more romantic spirit — that the science of every epoch is sui generis: that, in every age, the scientists working in any field have their own tasks and problems, and the degree of success with which they attack them has to be judged in different terms, depending on what exactly is an issue for that science at the period in question. So, there is no hope of formulating a universal index — still less, a formal algorithm — for intellectual 'merit' in science, to say nothing of using it to measure scientific 'progress' with mathematical precision.

As a working microbiologist, with a profound sense of historical relationships, Ludwik Fleck occupied a middle position between these two extremes. On the one hand, he could not follow the logical empiricists in their pursuit of formal algorithms or indices for measuring 'intellectual success' in science. That pursuit was (in his view) too closely bound up with the Vienna Circle's belief that scientific theories had to be supported by the 'preexisting facts' that they succeeded in explaining; and Fleck's book was largely designed to undercut this belief that the 'facts' or 'phenomena' of science preexist, and are independent of, the ideas and techniques with the help of which we conceive and formulate them. Phenomena can be picked out as 'phenomenal' only against the background of some body of theory, against which alone they appear either remarkable or perplexing. Facts are in practice constituted as 'facts' as much by the language that is available for stating them as by the experiences as a result of which we find ourselves prompted to state them; and this language — like the mode of perception that goes with it — is one aspect of the current scientific 'thought style' which brings those particular facts and experiences into focus.

On the other hand, Fleck nowhere committed himself to the opposite, or romantic view of a natural science: as a kind of intellectual 'fine art' incapable of any inherently cumulative or progressive 'improvement.' True, in his understandable desire to undercut the Vienna Circle's position, he may sometimes have overemphasized the differences between the 'facts' and the 'phenomena' available to, and explained by, scientists who worked at different times, and adopted different Denkstilen. But the fact that he was prepared to argue against the logical empiricists as he did, in the nineteen-thirties, does not mean that he would have been prepared, in the nineteen-sixties, to follow Paul Feyerabend into his more radical views about the essentially 'theory laden' character of all scientific facts, phenomena and
experiences. Rather, Fleck's remarks about the overarching 'ideals of modern science' — the ideals which scientists of different views and periods collaborate in serving — make it clear that, for him, there were some larger tasks that were shared by (say) all quantum physicists or all microbiologists, regardless of their particular ideas, perceptions and Denkstilen. These differences in Denkstile might frustrate the Vienna Circle ambition to measure scientific 'progress' with mathematical exactitude; but, just because, progress' in science was not strictly quantifiable, that did not make it illusory.

In this respect, Ludwik Fleck's position is (I believe) basically correct; and the central debate in the historiography of science remains, today, much where he left it. On the one hand, the cumulative elements in the historical development of the natural sciences are not, in general, ones that we can measure numerically: still less do they permit us to develop a single, overall index of scientific 'progress.' On the other hand, a case can still be made out for saying, in a more qualitative sense, that — despite all the differences in their 'styles of thought' — physicists today do 'know more than' those who preceded them, and similarly for the other natural sciences. And the task for historically minded philosophers of science correspondingly remains the task of recognizing how it is that the historical development of any scientific discipline brings with it a refinement in the criteria by which 'progress' in the tasks of that discipline has been, is, and can be judged. In that particular task, the philosopher of science must concentrate his attention on wirkliche Erkenntnis, as Fleck exhorted him to do, rather than allowing himself to be distracted by any Phantasiebild of his own imagining; and in doing so he can use all the help from his colleagues in the history of science, in sociology, psychology and the other human sciences that he can get.
PATRICK A. HEELAN

FLECK’S CONTRIBUTION TO EPISTEMOLOGY

I. SITUATION OF FLECK

Ludwik Fleck situated himself epistemologically in opposition to the two most prominent schools of the philosophy of science of his time: the Logical Positivism of Carnap, Schlick and others of the Vienna Circle, and the Historicism of Durkheim, Levy-Bruhl, Jerusalem and the sociologists of knowledge (46–51)\(^1\). A brief statement of where he stood with respect to each is helpful.

Fleck objected to two of the fundamental principles of Logical Positivism: (1) the strict separation it imposed between scientific fact (expressed by sentences in a purely observational language) and both logic and theory; and (2) the dogmatizing of extensional logic as the unique and normative logic for scientific theory construction. In contrast, he stated the inner dependence of fact, theory and logic, and the dependence of both on a historical socially-conditioned process.

Though clearly much influenced by the sociologists of knowledge, Fleck objected to the strict separation made by Historicism between the natural sciences (Naturwissenschaften) and the social sciences (Geisteswissenschaften), stemming from the assumed fixity or objectivity of established scientific facts in the natural sciences — or better, the descriptive frameworks of the natural sciences — and the assumption that in contrast 'social facts' were the changing product of conventional and historical Weltanschauungen that leave the facts of natural science, however, unchanged (46–51)\(^2\). In contrast, he stated that the descriptive frameworks of scientific facts in the natural sciences were themselves the product of intentional processes that were both culturally conditioned and historical; they were not, however, mere conventions (84–98)\(^3\).

Fleck then is not a scientific realist in the contemporary sense: in his account scientific reality is not what exists independently of human systems of inquiry; scientific facts are recognized by being the passive response of the world (experienced as 'given') to the active deployment of a historically changing system of perceptual inquiry — a performance — initiated and perfected by a collective of scientific researchers. Neither is Fleck a

conventionalist (8–9, 100): the consensual character of a scientific account is constrained both by logical considerations (in the broad sense) and by 'irrational' conditions (96–97), that is, by conditions that are not reasons or elements reachable by rational analysis; such, for example, are the perceptual skills for the successful performance of acts of scientific observation that are learned under the supervision of experts. Nor is Fleck a pragmatist, though influenced indirectly by Peirce and James: for him, the cash value of a scientific account is directly in the power of perceiving new scientific facts, not just indirectly in the ability to infer useful or verifiable consequences. Historical and sociological studies of the development of scientific knowledge are important for Fleck: "epistemology without historical or comparative investigations," he writes, "is no more than an empty play on words, an epistemology of the imagination." (41) His main interest then is in epistemology, more specifically, in the inner logic (Denkstil) that connects the subject and object of scientific inquiry with its embodied conditions — intentional, behavioral, instrumental and genetic — in the historical evolution of the Denkkollektiv. His work then is not just a piece of the history or sociology of a particular science or scientific fact — in this case in the history of the descriptive frameworks used to define syphilis — it attempts to state universal conditions of a logical kind valid for all scientific development; it is as a consequence authentically philosophical.

II. FLECK'S EPISTEMOLOGY

Fleck's fundamental position is that facts (including scientific facts in the natural sciences) are always and necessarily correlative to powers of perception (perception, observation, scientific observation not distinguished), "a form to be directly experienced' (101), that new facts become known by changes ("stylization") in our perceptual powers, that such changes are always culturally and historically conditioned (84–99 and passim). Because essentially related to embodied intentions and special performance skills, they are never objective in the sense of being definable independently of human systems of inquiry, and never just personal and private (38–46, 127). In other words, for Fleck, the progress of a scientific inquiry is nothing other than the gradual transformation ("stylization") of the perceptual powers of the scientific collective (scientific researchers and their public). The relevant ("stylizing") conditions are for him: (1) anticipating intentions or 'proto-ideas' (23–25); (2) confused, often chance beginnings where a conceptual model and an experimental procedure meet (74–78, 89); (3) a certain
'mood' composed of feelings (of satisfaction or dissatisfaction with values to be realized by the inquiry) associated with a performance (the performance of scientific observations) (99); and (4) the acquisition of performance skills (called 'irrational' elements) in carrying out experimental procedures (96–97). Without the performance (in which scientific facts are made manifest to perception), Fleck says we have no more than "the words of a song" without "the tune" (96). Such stylized perceiving is what he calls a "Denkstil"; this is "[the readiness for] directed perception, with corresponding mental and objective assimilation of what has been so perceived" (99). The unique importance in Fleck's analysis of the conditions — intentional-behavioral, genetic-historical and physical-procedural — for the performance of scientific observation sets him off strongly from practically all contemporary schools of the philosophy of science. Fleck's emphasis was no doubt motivated by his professional experience as a microbiologist and physician, but the elements of his analysis are clearly relevant to all branches of natural science.

Fleck's analysis is epistemological: it is a study, he says, in comparative epistemology (27–38). It is a study of the inner logic of the descriptive intentions that govern the receptivity of the knower to the presentations of facts: the 'unthinkability' of a fact that does not fit the descriptive intention, the 'unseen' 'unnoticed' character of contradictory evidence, the tendency to see only corroboratory evidence and to be blind to non-corroboratory evidence, indicate that Fleck is concerned with the logical constraints and normative conditions of possibility for the recognition of facts, thus, with philosophy, rather than with a reporting that such phenomena occur, and thus with history or sociology (27–38, 86, 104, 106).

Fleck describes the process of developing a scientific account as a process of developing powers of scientific observation and perception, "becoming 'experienced'" (96), it starts with protoideas — often mutually incompatible — these are intentions or heuristic notions accompanied by feelings of values to be realized by the inquiry, that are initially vague but nevertheless sufficiently specific to direct the design, performance, interpretation and gradual perfecting of experimental inquiry, the process, though animated by possibly a manifold of different intentions (Fleck enumerates four or more for the history of syphilis [17, 79–81]), begins in deep obscurity and confusion but as the procedures of inquiry become better understood, better designed and more skillfully accomplished, experience is sharpened and clarified. Eventually one forgets the obscurity and confusion that accompanied the process. Depending on one's place in the Denkkollektiv, if, for example, one belongs to the core ('esoteric') group of researchers, one may come to
enjoy the ability to perceive directly, and — as it were — with objectivity the scientific structures that the process of inquiry has made manifest. (If we take objectivity in a classical sense as independently of individual, cultural and historical biases, the objectivity may only be seeming.) If, however, one is distant from the core research group, one does not enjoy direct perceptual access to the scientific facts, but one may come nevertheless to accept on the authority of the culture the reports of scientific experts about such facts as objectively true.

The outcome of the process whereby new scientific knowledge is constituted is a Denkstil and like the process itself, it is a social entity possessed fully only by a group — a Denkkollektiv — in which it resides and through which it operates (125–142, 158–159). Thus, though individuals make acts of perception, the content, possibility and significance of an act of scientific observation does not depend only on the object but also in certain inescapable and necessary ways on the Denkkollektiv: no individual then can come to perceive a scientific structure or discover a new one without the cooperation of a collective that articulates its results descriptively, and publicly shares, monitors and promotes the process of inquiry by a set of common interests, feelings and emotions, or, to use Fleck's term, a certain 'mood.' (49, 99) Thus, the content of what any individual comes to perceive is stated in terms constructed by the collective; this content then is public, not private, and the event referred to and so described by the individual is verifiable by any member of the collective (86). Scientific facts experienced by the Denkkollektiv can be expressed in reports: these reports, when true, are true (100), not in the sense of conforming to an objective reality (in the classical sense), but in the sense of conforming to what is made manifest as a perceptual object within a Denkstil. Scientific reality for Fleck consists then in these scientific facts as correctly reported (126–127).

The developing inquiry is always multi-contextual. "Worlds as such," Fleck writes, "do not have fixed meanings. They acquire their most proper sense only in some context or field of thought." (53) Truth is not a "convention," "not 'relative'", "not 'subjective' in the popular sense" but "(1) in historical perspective an event in the history of thought, and (2) in its contemporary context, stylized thought constraint" (100). A "fact," he writes, "must be expressed in the style of the Denkkollektiv" (102).

A Denkstil is shared by the members of the Denkkollektiv in different ways. There are esoteric circles, circles of expert researchers close to the facts and experienced in the performance of acts of scientific observation; there are also exoteric circles, circles of non-experts and of the general public who
are not experienced in scientific observation, but who dogmatize the facts
to a greater or lesser extent because they lack historical awareness of the
genetic process through which the facts came to be made manifest to expert
observers. A Denkstil is also shared in different ways by writers of journal
articles, of vademecum textbooks and of popular science (112–125).

A characteristic of every Denkstil is that it is concerned with the multiple
appearances of common identifiable structures (such as the signatures of a
positive Wassermann reaction) and also with systems of mutually related
structures (such as the biological organization that is being sampled by
the experiment). Thus, the validity of the Wassermann reaction — as an
identifiable structure correlated with the bacterial agent (*T. pallidum*) of
syphilis — depends (1) on a theory or model involving a number of known
and to a degree controllable variables, and also (2) in a very crucial way on
the skills ("the state of being experienced [*Erfahrenheit*]" (96)) of the
scientific workers who perform the test. The former articulate a theory or
model explaining the reaction and its connection with syphilis; this describes
a system of related microbiological and chemical structures — of bacteria,
antigens, antibodies, complements, chemical solvents, etc. The latter comprise
those (unspecifiable, 'irrational') conditions necessary for the 'empiricization' —

tornebohm's word — of the model in the case at hand and for the reliable
execution of the test: these involve being 'experienced' in preparing a good
sample of serum, making accurate titrations, in noting the biochemical signs
(flocculation, color changes, etc), in matching reagents, in performing the
control experiments with skill and accuracy, etc. (96–97).

The sharing of a common Denkstil is a necessary condition for unam-
biguous communication: clearly, only to the extent that persons share
Denkstilen may they inhabit a common world of reality (126–127).

Persons using different Denkstilen will fail to communicate, no one
will see reality as the others see it, and each will believe that the others are
somewhere in a land of fantasy (109–111).

According to Fleck's account, scientific inquiry is embodied: it involves
the organs of sensory perception and changes that take place in them in order
to make possible the establishment of a new perceptual system, it involves
feelings and emotions relative to the values to be realized by the inquiry,
and it involves schemes of action, sometimes with instrumental and other
technological procedures. All of these embodied structures, all 'irrational' in
the sense that they are neither reasons nor transparent to reason, help to
'stylize' the trained perceptual powers of the inquirer, and create that "state
of being experienced (Erfahrenheit)" (96) that "alone enables a [researcher]
to perceive [for example] the relation between syphilis and blood as a definite form” (96). The technological instruments and circumstances are important because, when the *Denkstil* is fully established, they “automatically carry out the greatest part of our mental work for us” (84): they become part of a structured embodied probe through which the scientist feels out the scientific facts, and in so doing receives perceptual information about actual states of affairs.

The inquiry is, in addition, a *historical* process. It moves from a state of obscurity and confusion, false assumptions and irreproducible initial experiments to one of relative stability and clarity under the guidance of a *Denkstil*. The process, though usually re-interpreted by hindsight as a continuous forward path, has on the contrary, Fleck holds, no inevitability; its success being explained neither by historical fact (the passive side of cognition) nor by cognitive intention or theory (the active side of cognition), but only by an inseparable embodied dynamic involving the two under the guidance of a common ‘mood’ (set of feelings, emotions, values, aims, etc.) (99) The process induces changes in the individual and collective subjects, not merely in the acquisition of formal or theoretical understanding but in embodied perceptual and technological skills connected with the learning of new experimental procedures. What it is one comes to perceive is, or can — and should — be recognized as the achievement of a cultural and historical process that is never complete, and never fixed. It is not fixed because old scientific interests may be deserted in favor of new ones, and the old ones may be forgotten because of changes in the ‘mood’ of the scientific collective; it is never complete because some constraints may be removed that permit more complex systems to be sampled experimentally, systems that may be more general and inclusive than the original ones discovered. Thus, “even specialized knowledge does not simply increase, but also basically changes” (64). Fleck says, “At the moment of scientific genesis, the scientific researcher personifies the totality of his physical and intellectual ancestors and of all his friends and enemies,” (95) the scientific fact that has been generated then “becomes an event in the history of thought” (97).

The inquiry, finally, is *hermeneutical*: it involves for the research community the interpretation and discernment, that is, the *reading*, of experimental signs. Fleck describes some experiments he and his assistant performed with colonies of streptococci in which he attempted to establish the existence of different strains on the basis of color and transparency. The outcome was different from what he had anticipated: he was able to establish the existence of different strains but not on the basis of the characters that originally
suggested the inquiry. From this example, he draws some conclusions about epistemology:

It shows (1) the material offering itself by accident; (2) the psychological mood determining the direction of the investigation; (3) the association motivated by collective psychology, that is, professional habits; (4) the irreproducible 'initial' observation, which cannot be clearly seen in retrospect, constituting a chaos; (5) the slow and labious revelation and awareness of 'what one actually sees' or the gaining of experience; (6) that what has been revealed and concisely summarized in a scientific statement is an artificial structure, related but only genetically so, both to the original intention and to the substance of the 'first' observation. The original observation need not even belong to the same class as that of the facts it led towards. (87)

From confusion about what should constitute the set of relevant experimental signs, and obscurity about the precise meaning of those signs, order and clarity gradually prevail by a process of coming to understand, that is much like the hermeneutical process of learning to read a strange text. Apprenticeship for the novice scientist is by way of the same route by which a language is taught, the trainee is initiated under a dogmatic didactic authority inculcating the constraints of correct procedures and correct principles of interpretation (54, 104). Fleck says, "The Holy Ghost as it were descends upon the novice who will now be able to see what has hitherto been invisible to him" (106). The comparison between the biblical interpreter or hermeneut reading the scriptures under the enlightening influence of the Holy Ghost and the experimental scientist 'reading' his instrumental procedures under the enlightening influence of the Denkkollektiv is clearly drawn.

Fleck, as a historian of science, has much to say about archaic Denkstile and their interpretation. An archaic Denkstit, no longer in current use, can be recovered, he says, only by returning to original sources, that is, to original texts and illustrations (120–126); from these a hermeneutical study will reveal the intentions, models, feelings, goals etc. of the archaic inquiry. The history of science is full of such Denkstile, now forgotten and abandoned or, as Fleck believes, continuing in a changed and disguised form in the midst of contemporary science; the modern idea of syphilis, for example, contains residues of all the earlier accounts, of the mystical-ethical framework (syphilis as the 'carnal scourge'), of the notion of 'syphilitic blood,' of the theory of a causative agent like a miasma, and of other accounts. Old medical and biological descriptions of such items as the clavicle bone, the qualities of urine, the character of the metal phosphorus, etc., reveal the conceptual models, he says, that unwittingly constrained the writer. Likewise, old illustrations, such as, for example, Vesalius's, that purport to picture scientific
facts reveal the artefactual character of the representation, for what is pictured is always selective of what was thought relevant according to some conceptual model and important to the interests of the time (127–145).6

To summarize: the process of inquiry is perceptual — and hence oriented towards direct knowing; it is social — and hence, mediated by a collective, by common empirical procedures and by a common descriptive language; it is multi-contextual — and hence, governed by a specific interest in probing the variable observational profiles of specific experimental invariances; it is embodied in the performance of standardized instrumental procedures (or technologies) of scientific observation and in learned bodily skills to manipulate and read those instruments in order to satisfy the feelings that motivate the inquiry; it is historical, because it is both a product of a developmental process and a stage in a continuing development under the guidance of the Denkstil, and it is hermeneutical, because it learns to make and interpret a system of signs that, like a text, serve as a 'transparent' medium between the scientist and the empirical objects of scientific investigation.

III. HERMENEUTICAL-PHENOMENOLOGICAL INTERPRETATION OF FLECK7

The summary given above of Fleck’s epistemology was guided by my own set of philosophical interests, just as Trenn’s translation reflects his own. Any translation of a work like Fleck’s will reflect the dominant interests, the Denkstilen, of the translator, and any philosophical critical paraphrase will likewise do the same. Fleck’s own philosophical work is not sufficiently rich in content or background allusions to fix unambiguously the philosophical tradition within which he was working or within which preferably he is to be read. A critic and interpreter, no less than a translator, has to choose a tradition within which to elucidate the work. Trenn’s choice was slanted towards an empiricist sociology of knowledge, and one with which, I surmise, Fleck would have had some trouble. The tradition within which I shall choose to elucidate Fleck is that of hermeneutic-phenomenology. This is a tradition usually held to be particularly appropriate to the Geisteswissenschaften and not at all appropriate to the natural sciences with which Fleck is primarily concerned. It is then a matter of great interest and importance to find that Fleck’s account of the natural sciences follows closely the kind of description that is given by hermeneutic philosophers of the social sciences or Geisteswissenschaften, and so Fleck’s account in a revolutionary move would seem
to place the natural sciences among the social sciences, in some respects eliminating the distinction between the two.

*Hermeneutic-Phenomenology* ‘Hermeneutic-phenomenology’ (H-P) is a term that refers to a philosophy that has the aims and methods of both a phenomenology of perception and a hermeneutic of textlike materials. In the account that follows, I am not claiming for my theses, textual support from the writings of Husserl, Heidegger and Gadamer, but only support from the tradition that they have mediated.

As a phenomenology, the concern of H-P is with the critical analysis of perceptual content in order to identify perceptual invariants in the appropriate manifold of profiles; thus, its aim is apodicticity relative to what is given essentially in perception; what is given essentially and apodictically is a perceptual essence, and how it is given is through a system of profiles.

By perception, I mean any form of direct knowing issuing in a descriptive statement about an object or situation that is directly manifest in terms of critical perceptual essences, with a presence mediated by an appropriate physical embodiment in somatic and technological processes, and in appropriate structures of the ambient energy fields. I do not mean visual perception alone.

As a hermeneutic, the philosophical concern of H-P switches from the perceptually given to the process and structure that results in the categorization of those objects given in experience; this process is described as a ‘reading’ of experience, and hence, it is interpretative and hermeneutical. Since a hermeneutic is always performed in a cultural and historical setting, it never attains an absolute trans-cultural apodicticity of meaning; there is then a certain relativizing of phenomenology vis-à-vis its original claim of being a ‘rigorous science.’

But just as the claims of phenomenology are weakened somewhat, so the claims of hermeneutics are strengthened, when they are given a critical empirical basis in perception; no arbitrary ‘reading’ of experience, however interesting and stimulating it may be, is acceptable unless it can be verified in experience. The phenomenological component takes away the arbitrariness, the conventionalism, of much that passes for hermeneutics today, and gives it roots and criteria in perceptual experience – even in the way the natural sciences are rooted in experience. Hermeneutics, without phenomenology, is arbitrary; phenomenology, without hermeneutics, is rigid and sterile.

Since I am concerned with a phenomenology of scientific objects, I want at the start to distinguish two classes of perceptual objects given in naive experience, or in the natural attitude. Some have well-determined (or
determinable) essences, each with its characteristic manifold of profiles, and others are pseudo-profiles and pseudo-essences destined to be supplanted at some future time by other more authentic perceptual structures. These classes are rooted out only by a phenomenological inquiry performed in the critical reflective attitude. Such inquiries often extend over time, sometimes over generations, or even over centuries. Those that are authenticated by this kind of inquiry, I shall refer to as 'authentic essences,' 'authentic profiles,' or 'authentic perceptual objects.' With the help of this distinction, we can divide 'revolutions' in science into those which definitively terminate an ambiguous tradition of inquiry and end the search for objects of a certain kind, and those that transform the current state of a tradition of inquiry in a notable way, retaining continuity in the reference objects of the inquiry. Examples of the first kind of scientific revolution are theories about phlogiston and caloric — these were replaced by theories about more appropriate physical structures — also theories about 'humors,' 'bad blood,' 'carnal scourge,' etc. all of which have been definitively abandoned. Fleck, however, writes as if all Denkstilen — even those that have been lost for whatever reason — are or were in their own time authentic ways of perceiving; for him it seems there are no pseudo-profiles and pseudo-essences, but only authentic essences, and these for him become incorporated into future projects and survive in those projects.

Turning now to hermeneutics: this is the science and art of interpreting texts and text-like materials in order to arrive, as Heidegger says, at the 'things themselves' about which the text speaks. The task of interpretation is led by fore-structures of understanding; these are (1) the conceptual frameworks suggested by the cultural traditions — the "biases and prejudices" we share, as Gadamer calls them, these are what Heidegger calls "Vorsicht"; (2) embodied perceptual habits or skills, these are what Heidegger calls "Vorhabe", and (3) a specific hypothesis suggested by clues in the textual material itself and by analogies with other textual material, this is what Heidegger calls "Vorgriff". "A person who is trying to understand a text," Gadamer says, "is always performing an act of projecting. He projects before himself a meaning for the text as a whole as soon as some initial mening emerges in the text." This search for a holistic understanding is the antithesis of the assumption that basically all knowledge is derived only by combinatorics from a definite and fixed empiricist or nativist (conceptualist) repertory, or by inference from such a basis. Though circular, the hermeneutic process is not a 'vicious' circle in the logical sense, it is a 'virtuous' or 'hermeneutical circle.' We are warned, however, that "all correct interpretation
must be on guard against arbitrary fancies and limitations imposed by imperceptible habits of thought. Our “first, last and constant task,” to quote Heidegger, is to direct our gaze “on the things themselves,” that is, on the objects, situations, goals spoken about, indicated or described.

But what are the ‘things themselves’? The ‘things themselves’ are the subject matter about which the text speaks, and what that is depends on the kind of text that is being read. Some texts are concerned with non-perceptual objects like theoretical or philosophical notions, programs, plans and possible futures, still others are involved merely with the sound and play of words: in none of these are the ‘things themselves’ perceptual objects. Some texts, however, probe meanings and structures constitutive of experience; the ‘things themselves’ then are manifest objects accessible to a general public. It is only in current or historical Worlds — including their scientific horizons — that perceptual objects have a home. To understand a perceptual object is to be able to recognize and name it and to affirm it in a perceptual judgment.

How does one come to understand, recognize in experience and name a category of perceptual objects referred to by a text? We ask: can the essence of scientific entities be understood from a study of textual materials and illustrations alone? Clearly, the appropriate fore-structure is our understanding of the World, and its manifold of horizons; this includes developed perceptual skills embodied in our mode of interactions with our World, and a common descriptive language. Consider the future development of science — what is now being considered in a speculative or tentative way — and its past — what was once affirmed but is no longer affirmed.

Consider such radically new and speculative entities as quarks and black holes, or archaic scientific entities like phlogiston or caloric, can these be understood simply from equations, diagrams, models, and commentaries on these? In concert with Fleck, I take the position that the essence of a quark or black hole cannot be encapsulated in literary, mathematical and illustrative materials alone. On their own, these speak only metaphorically (by conceptual models) of that which is the real essence, and what would be manifested (were quarks and black holes to exist), each as the invariant structure of a system of perceptual profiles made manifest through appropriate observational procedures using what I call ‘readable technologies.’

*Hermeneutics and the History of Science.* A similar problem arises with respect to the history of science. If the archaic scientific essence is just a pseudo-essence, I presume there is no way we could recover its perceptual essence, since it had none. If the archaic scientific essence is authentic, would we then have to learn to perceive nature as archaic scientists perceived it, in
order to interpret correctly what was written in the past? To the extent that
the recapturing of such a perceptual experience is possible, does this experi-
ence emerge from the study of the archaic textual and illustrative materials
alone?

Underlying this discussion is the question: what is science, what does it
do? There are those who hold that science theory- and model-making do
not have an ontological intent; that its intent is merely manipulative (Instru-
mentalism), or merely to construct abstract models that organize a given or
fixed empirical base (Empiricism). In keeping with such philosophies, archaic
scientific texts would then be interpreted without the necessity of experi-
encing their objects. If, however, Fleck’s analysis of the perceptual use of
instrumental procedures is correct, then (provided the archaic entities had
authentic profiles and essences), it would be necessary to come to perceive
the scientific entities in order to come to a correct understanding of the
text.

Thomas Kuhn, whose view of the development of science is so close in
many respects to Fleck’s, spoke about the transformation that took place of
his sense of the history of science when he came to understand that Aristotle
in his Physics was not making an unsuccessful attempt to do mechanics, but
was concerned with a much larger question, the character of qualitative
change in general, and of local motion only to the extent that it exemplified
the general qualities of change. To interpret Aristotle’s Physics correctly,
then, he holds that one must come to experience change (all, or for the most
part) as charged with intentionality. Such a transformation was, in Kuhn’s
words, a ‘hermeneutical’ experience.14 According to this view, a particular
set of archaic, but authentic perceptions might simply have been lost to our
culture by a change of cultural interests. Kuhn’s experience of the necessity
of a hermeneutics of archaic texts is comparable in certain ways with the
account given by Fleck (121–145), but it lacks an emphasis on the phe-
nomenology of perception.

The role of hermeneutics in scientific observation is also reflected in the
views of S. Toulmin15, Paul Feyerabend,16 N. R. Hanson17 and M. Polanyi18
who all insist that ‘observational’ transformations generally accompany the
widespread adoption of new scientific theories; but it is often not clear as to
whether ‘observational’ means authentically perceptible or merely detectable
by indirect means.19

The importance and necessity for an aspiring scientist to be apprenticed to
the experimental and theoretical life of an ongoing scientific community is
stressed by Fleck, as it is stressed by others, such as Kuhn, Toulmin, Polanyi
and others. Learning the "Denkstil," as Fleck puts it, or learning to operate within the 'disciplinary matrix,' as Kuhn calls it, does not, however, consist solely in the ability to construct or manipulate conceptual models, but comprises also the ability to use these models in empirical procedures to exhibit the appropriate scientific structure to a trained observer. Kuhn speaks of "some non-linguistic process like ostension" in which mastering exemplary or paradigmatic studies plays a large role. This ability to 'empiricize' conceptual models is learned, not through a system of correspondence rules or reduction sentences as suggested possibly by the 'Received View,' but, as Fleck puts it, by the scientist's becoming 'experienced' in the performance of scientific observations or, An experiment is not a piece of Nature, pure and simple, but a humanly contrived phenomenon. "In science, just as in art and life," Fleck writes, only what is true to culture is true to nature" (35). In keeping with Fleck and as I shall explain more fully below, Nature is made to 'write' a 'text' in conventional symbols for the trained scientist to 'read.' A 'reading' is direct access to meaning through a 'text,' and in the case of scientific observation, the direct access is mediated by somatic and technological processes, that is, it is perceptual.

Let me call the signs, symbols, changes attended to in the experimental (or 'stylized') procedures from which the scientific facts are 'read', the "text" "written" by the scientific object using the stylized empirical procedures; then the empirical procedures become a 'readable technology,' and the process of interpreting their meaning becomes analogous to the interpretation of a literary text. These 'texts' become transparent only to the initiated, as Fleck has shown by many examples in microbiology. The same phenomenon of transparency occurs in the use of standard physical instruments, but not always. It is not present at the start of an apprentice-scientist's training, but is at the end. During episodes of scientific development dominated by profound theoretical change, transparency, even for an expert, can give way to deep obscurity and travail, since what is presented to perception through the available empirical procedure is incompletely understood, and the necessity for further theoretical development is painfully recognized. Fleck gives a brilliant account of this kind of groping and obscurity in immunological and serological research, which is closest to that experienced by philologists in the interpretation of archaic texts or texts from strange cultural milieus.

The role of hermeneutics in natural science is not then restricted to the study of textual and illustrative material, for these alone do not bring the reader to confront the "things themselves" about which science speaks. Such materials alone can only confront the researcher with the model used
to understand nature — knowing what ‘lions’ are helps to understand the phrase ‘Lion of Judah’ but what this says of David, requires in addition the skill to apply it correctly to David; it does not describe, for example, David’s appetite. The ‘things themselves’ in so far as they are facts, that is, perceptual objects, are confronted only in and through the performance of empirical procedures.

It will be noted that Fleck has comparatively little to say about a theme that fills books in the philosophy of the physical sciences, models. In the discussion of models, an important distinction relevant to the question of continuity and discontinuity in theory shifts has to be made. This distinction is between an abstract model like a system of Newtonian point-masses, and the use of the model descriptively to make particular statements with realistic intent about the World. A mass-point is a constructed, abstract, imperceptible an therefore unreal entity; but the Sun-Earth system (to which this model for certain purposes can be correctly applied to yield true statements) is not a system of mass-points, but a representation of a mass-point model. The notion of representation (a particular use of the model) is an essential epistemological refinement and serves to distinguish between, say, the Earth-represented-as-mass-point and mass point as an element of an abstract model. It is often stated, following Husserl, Merleau-Ponty, Gadamer, Heidegger and others, that the sole use science makes of abstract models is to make them surrogates for the reality we perceive — “mirror images” in Mind, of reality. If this is taken as a statement about the essential nature of science, then it is a misunderstanding — though very prevalent — about the correct use of mathematical models in science. The point of the emphasis on experimental or perceptual acts in science is to insist that science is not about models (as ‘mirror images’ or surrogates for reality) but about reality understood through the appropriate descriptive use of models. Models are merely quantitative metaphors: they enable one to recognize certain kinds of facts given in and through perceptual experience, and they are necessary for the identification of those facts, since without the model/metaphor there is no way of identifying what it is one ought to perceive. Hence, differences in model construction need not imply an essential difference in the understanding of the domain of experience about which the models speaks, just as the difference between a Ford Model T and Mercedes 260S, or between a donkey and cart and a Porsche do not imply that transportation by road is an equivocal term. The continuity that is sought in the development of scientific theories is not to be judged by the syntactical congruences or non-congruences between theories-as-models, but by the greater or less
inclusivity of their empirical horizons (represented by the ability to order them under the partial ordering of a lattice of \textit{Denksstilen}). A \textit{theory} in the latter sense, is not an abstract model, but a pre-linguistic entity logically antecedent to models (though using models in its explicit articulation), and comprises those elements that Fleck refers to as the 'mood' of the inquiry, 'proto-ideas,' 'the purpose of the Denkhkollektiv,' etc.: I have paraphrased this as 'intentions,' or 'structure of intentions,' it is what connects and underlies a progressive sequence of conceptual models, where each successfully identifies the same class of referents, and each is more powerful than its predecessors in the line of development.

\textit{Progressive Development in the History of Science.} According to a view first proposed by Kuhn, scientific inquiry historically falls into two patterns: 'normal science,' which is linear development within a fixed and dominant 'disciplinary matrix,' it has an internal history sufficient to account for the development, and 'revolutionary science,' which is a non-linear mode of mutually competing and mutually incompatible explanatory matrices were the selection of the winning paradigm involves predominantly external (e.g., social and political) factors.

In contrast with Kuhn's sociological view is the normative (philosophical) view of the Popperians, for them, science is always in a 'revolutionary' phase, it is the systematic effort to supplant old theories by new ones of greater explanatory power, for which project the search for anomalies is perennially crucial.

Much discussion has centered on the existence of scientific revolutions, whether they occur, and if so, how they should be described and the role they play in the scientific enterprise. Fleck had nothing to say about scientific revolutions. This seems at first sight strange. However, from the point of view of an epistemologist, scientific facts once established by a \textit{Denksstil} as ontic possibilities remain so in principle, even if they are later lost to the human community by a change in its interests, orientations and environment. The \textit{loss} is not perhaps significant for the philosopher; it is, however, an important empirical phenomenon about science that would naturally spark the interest of a historian or sociologist of science. Scientific revolutions as described by Kuhn are then of interest primarily to history and sociology; scientific revolutions are also of great interest to the Popperians whose epistemology, like that of the logical empiricists, rests on the historical stability of basic statements of fact. Such revolutions, however, do not spark the special interest of philosophers like Fleck for whom reality is 'horizonal,' grounded, that is, on \textit{Denksstilen}. That science is not simply the accumulation of and
systematization of factual information is generally agreed, but it is less well understood that the new scientific models and theories, as Fleck has shown, transform the content of one's perception of the World, and so transform the basic scientific facts with which the scientific community has to deal.

Fleck and Kuhn agree that conceptual models change and are replaced: what constitutes the rationality of this process? Is scientific reality then like social reality, a product of the historical-cultural process?

The rationality of theory change is something too complicated to deal with here. It requires (a) the analysis of hermeneutical changes that preserve reference, (b) a theory about the definitive abandonment of traditions of inquiry, and (c) a theory of the synthesis of separately authentic descriptive systems that are incompatible, but nevertheless not absolutely, only relatively so. These last are systems that from their analogue in quantum mechanics I call 'complementary systems.' By 'complementarity,' I mean 'context dependent' perceptual processes. Such an account agrees better with the original insights of Bohr and Heisenberg, than with subsequent attempts in the empiricist mode to explain this peculiar aspect of quantum mechanics. The articulation of this view is to be found elsewhere in papers I have written and in my book.25

Revolutionary science, whether merely in historical episodes as Kuhn claims, or as a permanent state of science as the Popperians would have it, is hermeneutical. An anomaly that consistently frustrates the anticipations of scientific observation brings up for reflective questioning the 'cut' that separates the subject from the objects of science; this invites a search for new descriptive models, and a new interpretation of experience embodied in new readable technologies.

Revolutionary science is historical and dialectical, since the persistent and significant failure of a tradition forces the scientific community to reflect on the historical roots and path of development of the tradition in an effort to find negativity that has been overlooked. This is often contained, as Fleck points out, in an existing but minor tradition, incompatible with the former and therefore considered of less significance. The purpose of this return to historical roots is to recover the global intentions that were operative prior to the entrenchment of the major tradition and that continue to underlie it. This has been illustrated by Fleck in numerous examples relating to the history of syphilis, the ethical-mystical tradition which, he says, is continually active, the etiological-causative tradition, the therapeutic tradition, etc., all of these constitute, according to Fleck, the well-springs of the contemporary understanding of syphilis, and represent elements in the
partial ordering of the dialectical lattice that synthesizes for us those past traditions. The same return to historical origins can be found illustrated in the development of physical theories, for example, in the development of the atomic theory, in Einstein’s reflections about relativity, and Heisenberg’s about quantum mechanics.

Reality as a whole is what belongs to Worlds and is given directly in perception; it then shares the historicity of ‘stylized’ human perception. If scientific theories articulate physical reality by making its structures manifest to ‘stylized’ perception in and through appropriate observational procedures or ‘readable technologies,’ and if scientific accounts are historical, then physical reality is like social reality, it is roughly just that part of reality that is mediated by scientific instruments or readable technologies. It is then historical and cultural, but it is not conventionalist in the sense of Duhem, Poincaré, Reichenbach and Quine, for these overlook the constraints of Vorhabe, which are not antecedently known, or even possibly knowable.

**Natural science as Non-hermeneutical?** It is a well-entrenched tradition in the sociology of knowledge that goes back to Karl Mannheim that there is an essential difference between the natural and social sciences by reason of their subject matter, methods of goals.

Hermeneutical philosophers, like Heidegger, Gadamer, Habermas, hold a very similar view. The subject matter, methods and goals of natural science are for them essentially pragmatic, ordered to the control and manipulation of people and things; its characteristic method is the construction of theoretical model systems that create a surrogate picture of nature; the components of the scientific model are radically imperceptible and do not, and cannot, have a place in any historical World. (It is this last assertion that I have contradicted.)

It is not surprising that philosophical hermeneuts of our culture as well as sociologists of science agree in their description of the phenomenon of science in the West, and in the denial of any role in its internal development for social, historical and hermeneutical factors. This view is after all the one suggested by the particular cultural history of Western societies. Empiricism in philosophy and a pragmatic bent characterized the rise of science. One finds both, for example, espoused as a public ideology by the Royal Society at its institution. Empiricist philosophy at that time was, it must be remembered, as much a political ideology as a scientific one. Non-hermeneutical empiricist science made an early ally of the Enlightenment and soon became identified as the political tool of bourgeois liberation from the ‘authorities’ of the ancien régime, and from the philosophical (mostly hermeneutical)
traditions that were identified with princes, popes and divines. 'Conservative'
religion and 'liberal' science were born within this sociopolitical scene.\textsuperscript{32}

For those then who, like Gadamer and the later Heidegger, have expe-
rienced science only as functioning within the historical matrix I have
described, and who have not performed the kind of critique of historical
prejudice so strongly recommended by themselves in their own work, it is
understandable that they would accept the view that since the goal of natural
science in our culture is control of natural phenomena, the content of its
theories need be no more than a reconstruction of nature according to a
system of models that permit the achievement of this goal.

To place a hermeneutic of perception at the core of scientific observation,
as Fleck does, is to give science an ontological essence. Its current cultural/
historical profile in the West then appears just as one of its possible profiles,
one historical route science can take in human culture. This distinction —
 between the profiles and essence of science — opens the way for the 'redemp-
tion' of science from this Babylonian captivity to one cultural path.\textsuperscript{33}

Instead of the dumb and awed silence that many feel today before the
impersonal greatness of nature, a new possibility can be glimpsed, that science
might come to be experienced as a creative historical dialogue between a free
society and its World.\textsuperscript{34} Science would in this way come to be experienced as
full of human choices and prejudices, of political fights and religious feeling,
and the expression of human freedom. Such are the conditions for the kind of
playfulness that is at the center of an esthetic experience, where science
and art meet.\textsuperscript{35}

\textbf{NOTES}

\textsuperscript{1} Numbers in parentheses are page numbers in the English translation of Ludwik Fleck's
\textit{Entstehung und Entwicklung einer wissenschaftlichen Tatsache: Einführung in die Lehre
von Denkstil und Denkkollektiv}, Benno Schwabe, Basel, 1935. The translation in entitled,
\textit{Genesis and Development of a Scientific Fact} (transl. by Fred Bradley and Thaddeus

\textsuperscript{2} "All these thinkers trained in sociology and classics, however, no matter how produc-
tive their ideas, commit a characteristic error. They exhibit an excessive respect, border-
ing on pious reverence, for scientific facts" (47).

\textsuperscript{3} Referring to medical descriptions made by physicians in the past, Fleck writes, "Our
physical reality did not exist for them. On the other hand, they were prepared to regard
many another feature as real which no longer has meaning for us" (127).

\textsuperscript{4} It would not then be useful to read Fleck as if he were a Logical Empiricist \textit{manque}.
He is better compared with, say, American Pragmatists, or with S. Toulmin, N. R.
Hanson, M. Wartofsky, P. Feyerabend or I. Hacking. There are many similarities between
Fleck's conceptions and the hermeneutical-phenomenological approach of the present writer; cf. Heelan (1975a) and (1983).
5 I believe that Fleck meant the term 'psychology' here in an epistemological sense, as intentions, aims, etc., of the inquiry.
6 On pp. 120–121, Fleck discusses medical descriptions offered at an earlier period. It is significant that he focusses there on the conceptual model and not on the facts as they might possibly be re-experienced today.
7 This section depends heavily on chapter 11 of Heelan (1983). For a review of the various empiricist, hermeneutical and dialectical schools of the philosophy of science, see Radnitzky (1973).
9 Gadamer's view of 'bias and prejudice' is that they serve a necessary role as containing the authority of a tradition, they are then inevitable, but are in need of continual criticism. Gadamer is not as emphatic about the necessity of embodied skills or Vorhaben as is Heidegger. See Gadamer (1975), pp. 241–253.
12 Gadamer (1975), p. 236.
15 Toulmin (1972).
16 Feyerabend (1975).
17 Hanson (1958).
18 Polanyi (1964).
19 For a fuller discussion, see Heelan (1983), particularly chapter 13.
20 See 'Reflections on my critics' by T. S. Kuhn in Lakatos and Musgrave (1970), pp. 231–278. Kuhn writes, 'for [paradigm] I should now like some other phrase, perhaps 'disciplinary matrix': 'disciplinary,' because it is common to the practitioners of a specified discipline; 'matrix,' because it consists of ordered elements which require individual specification;' among the latter are "shared symbolic generalizations," "shared models, whether metaphysical, like atomism, or heuristic, like the hydrodynamic model of the electric circuit," "shared values, like the emphasis on accuracy of prediction," and particularly, "concrete problem solutions," ("problem-solution paradigms") or "exemplars") (pp. 271–272).
24 For an account of models and metaphors, see Black (1962) and Hesse (1963) and (1980).
26 Heelan (1983).
27 Ibid., chapter 11.
28 See references.
30 Cf. Heelan (1965) and (1975b).
31 Mannheim (1936), pp. 116, 168; Merton (1973), p. 270. This mainline tradition of the sociology of science is being strongly challenged today, both by sociologists, cf.
Bloor (1976) and Mulkay (1979), and by philosophers and historians of science, like Hesse, Kuhn, Laudan, Rorty, C. Taylor, Dreyfus to name a few.
32 For example, this is the case with Locke. "Our modern Western world has been made by scientists, merchants, statesmen, industrialists; Locke was the first philosopher to expound their view of life, to articulate their aspirations and justify their deeds," Cranston (1961), p. 3. See also, Mannheim (1936), pp. 165–9 for the role of the bourgeoisie in the development of science.
33 Foucault (1970), Monod (1971) and Skinner (1971), to mention just a few, have argued that science has banished objective grounds for all personal and religious values. Cf. Dreyfus (1980) for the 'nihilism' of identify theories.
34 This, for example, would resemble in some ways the extreme position adopted by Feyerabend, for which he coined the motto, "Anything goes!"; cf. Feyerabend (1975) an (1978). For science as a work of the imagination, see Holton (1978).
35 Cf. Gadamer (1975), p. 98–99, where it is argued that human play involves players and spectators in the closed world of the game, and that the perfection of play is its transformation into the structure of a work of art; similarities with scientific model building would not be hard to express. See also Holton (1978).

REFERENCES


YEHUDA ELKANA

IS THERE A DISTINCTION BETWEEN EXTERNAL AND INTERNAL SOCIOLOGY OF SCIENCE?

(Commentary on a paper of John Ziman)

John Ziman’s great contribution to sociology of science is the establishment of a serious, working physicist’s view of science as a collective enterprise. Fleck, according to Ziman, objects to “complete inadequacy of epistemological individualism”. Ziman recognizes in Fleck a kindred spirit. Fleck, like Ziman himself, was a distinguished and creative scientist and they both belong “to ‘science’ and ‘society’ as an active person”. But for Ziman these two worlds of science and society are kept apart. Not only does Ziman accept the external/internal dichotomy of studying the scientific process but he even makes use of the sophisticated notion of internal against external sociology of the scientific community. The Mertonian norms of communalism, universalism, disinterestedness and organized scepticism are thus internal sociology. The external sociology of science deals with the “actual change in the social relations of the scientist within and outside science”. This is the central theme of Ziman’s paper; and is, according to Ziman, also Fleck’s main preoccupation. We learn much about the constraints that organized research imposes on the autonomy of science as regards problem-choice, methods, career-course, and validation. It is convincingly shown that conventional academic science and scientific ideology clash with the collectivized science of the 1980’s and the normative penumbra of what Ziman brings before us is that since the science of the 1980’s has brought with it great progress, there is not that much wrong with it, and we must study its departure from its earlier course, and tune ourselves to its new character: in other words conservative modernism has the imprimatur of a leading physicist.

Though I believe that much is wrong with institutionalized research nowadays, this is not the occasion to take up such issues.

I cannot accept Professor Ziman’s sharp distinction between external and internal sociology of science and even less his identifying Fleck’s work as external sociology of science. Not only does Fleck not think in internal/external terms, he does not even distinguish between the individual epistemology of the scientist and what the collective activity imposes upon him or on his epistemology. On the contrary: Fleck’s greatness and the source of

his half-implicit influence on his early readers like Reichenbach or Thomas Kuhn is that he studies the social origin of the individual thought-style, i.e., the way the thought-collective expresses itself in the very thinking of the individual. This is the tradition of the genuine Bacon – not the vulgarized Victorian one – this was the quest of Whewell, and this was what Einstein was referring to in his few and far-between remarks on social determination of ideas. I shall refer here only to Kuhn’s reaction to Fleck because of Kuhn’s obvious impact. In those years when The Structure of Scientific Revolution was being shaped, distinctions between individual or collective epistemology or between internal and external sociology of science were not yet at the forefront of attention. Yet Kuhn read Fleck correctly. Kuhn wanted to study the individual act of discovery as a social-cognitive phenomenon, to study the way in which the Zeitgeist expresses itself in individual epistemology. It is in this sense that the Structure leans heavily on Kuhn’s own The Copernican Revolution. However, facing the enormous complexity of historical change in science, Kuhn gave up the attempt to explain historically the sources of paradigm-changes in the Weltanschauung. Having described convincingly mechanisms of the influence of paradigm-change on the process and program of science Kuhn left it there. In this sense our generation of historians of science are all Kuhnians. However, since there is no theory of the origins of paradigm-change, it is not accidental that Kuhn’s in-depth case-study of the Black-Body Theory and the Quantum Discontinuity 1894–1912 is not really a Kuhnian work. Fleck on the other hand does make a courageous attempt to account for the origins of a paradigm-change, but he does it through studying one case: the development of the Wasserman-reaction to syphilis in all its historical complexity in terms of thought-collective and thought-style.

Ludwik Fleck’s masterpiece, since its resurrection by Baldismus, Schäfer and Schnelle and Trenn and Merton is now widely known. I shall rather refer in what follows to a lesser known short paper by Fleck: ‘Zur Krise der “Wirklichkeit”’, which appeared in 1929 and constitutes the outline of a Fleckian general theory. Fleck’s paper ‘Zur Krise … ’ was written as a contribution (rather than response) to an issue raised by Kurt Riezler the previous year. Riezler raises the classical problem of three different realities: (1) Our immediate (internal and external) perceptions; (2) the objective, I-Independent, world of invariants: unchanging laws and relations deduced from our arrived at on basis of changing relations; and (3) the truly absolute reality which is our highest human intellectual aspiration. Then Riezler mentions how the sensationalist, the idealist and the genuine realist views these three worlds. The realists:
see in the first a semblance of the senses, in the second a provisionism of cognition and in the third absolute reality. 7

This realism of the philosopher is shared by naive men of all times. However, modern science in the last century has introduced some changes into this basic conception:

Science explores the first reality, from experiment to experiment advancing and looking for laws which construct relations between the variables. It considers these laws absolute, a part of the third reality, and presupposes that this third reality is a firmly closed order . . . the more we discover such laws and their inter-relationship, the provisionism of the second reality approaches the third so that at the End of Science it will be dissolved in it.8

Now, recent advances in physics have shattered some of the self-evident presuppositions, all of them relating to the means of cognitions, to the structure of the third Reality. It is being presupposed that the means of cognition (our epistemological tools, so to speak) constitute a closed system which is sufficient for grasping Reality; moreover that this absolute Reality is an order unambiguously ruled (organized) by a finite number of invariant laws: and thirdly that these means of cognition are adequate to this order.

The shaking up of these propositions is Riezler's "crisis of 'Reality'". It is an epistemological crisis. Riezler deals with the problem — an analysis of it in terms of modern physics, not its solution, in sheer philosophical terms. Fleck's great contribution is to take the crisis out of pure philosophy and translate it into social terms. The crisis is not seen as a philosophical breakdown but an historical change in the images of knowledge which show how thought-collectives constitute the socio-historical context in which the individual formulates his thought; in different terms: how the thought-collective influences the individual thought-style. I am tempted to say that Riezler stands to Popper the way Fleck stands to Kuhn.9

First of all, emphasizes Fleck, our knowledge consists more of what we learn than of what we cognize. And what we learn is not general for all learners, it is not purely external, coming in to us, but a unique individual combination of what we were taught and what we were or are independently. There is no direct road from the teacher's mouth to the student's ear: always a distortion, a change occurs. What Fleck is hinting at is that Luther was wrong: "the meaning is not in the text".10 There is no independent existence of the meaning written down or expressed by one person: what is uttered is an inseparable part of the utterer's thought-world, and what is cognized is by definition an inseparable part of the knower's (so as to avoid saying the
receiver's) thought-world. Unlike the situation in the physical world, where we can surmise what the form of the undetected energy on its way between emitter and receiver might be, there is no meaning of the text on its way between teacher and learner. However, just like in the physical world where the detection of the energy on its way is done by bringing a receiver to interact with it and thus changing it, so the meaning of the learner's text deviates from what it was for the teacher.

Fleck considers three important factors which influence the text on 'reception', all themes of a social character: (1) the weight of tradition; (2) the impact of education and (3) the influence of the order in which the acts of cognition occur.

With breathtaking pace Fleck now heaps ideas upon ideas: man is never a tabula rasa, our mind is never a clean slate, to every new situation we bring some ready knowledge and partial answers, our cognition cannot be constructed from bits and pieces of sensations; we do not perceive until we are told what to 'see' (Gestalt, Hanson's Patterns of Discovery). But only Bacon is a genuine predecessor of Fleck in so far that only Bacon saw that what the individual ends up 'seeing' is always due to what he carried within himself in addition to what he learned due to the socially formulated images of knowledge. Thus for Fleck there is no distinction between Riezler's first and second worlds of reality — between pure sense acts and elements of cognition (I-independent as they may be).

Cognition is thus formed by social factors in such a way that any distinction between external and internal disappears. The influence of the social is through tradition, education, and the very sequence of the cognitive events which is also of social origin. The two central concepts which Fleck introduces are both of this new kind; they constitute a social epistemology. These are the thought-collective and the thought-style (a phrase coined by Mannheim in 1925) and are both in themselves results of the interaction of what before their introduction would have been called the internal thought-style of the individual and the external thought-collective of scholars, intellectuals or influential laymen. Through the introduction of the new concepts, the previous dichotomy into external/internal loses its meaning. This is a case of pseudo-incommensurability between an old and a new theory just like between two theories in science, let us say, mechanics before force and energy were distinguished and after the formulation of the conservation of energy.

The thought-collective is a group of people with its images of knowledge: sources, aims, methods of knowledge. This group or groups of people influence
the thought-style of any cognition in an individual through means of tradition and education. Fleck could have named other kinds of influence as sources of knowledge: authority, force of personality, fashion, interest, the dialectical process of critical debate; all of these are somewhat different than that what is usually understood by tradition or education. In critical debate between two traditions or between a group and an individual, neither the thought-style typical of a cognition (ein Wissen) of the individual nor the thought-collective come out unchanged.

It will be worthwhile to undertake an analysis of the concepts of style and thought-style, collective and thought-collective. Meyer Schapiro opens as follows his article on style:

By style is meant the constant form – and sometimes the constant elements, qualities and expression – in the art of an individual or a group. The term is also applied to the whole activity of an individual or society . . . .

Since Fleck is talking about the thought-style of every piece of knowledge, he means all that is constant, typical of that piece of knowledge created in the process of social epistemology.

In this brilliant short paper Fleck does not develop this idea much further, although he illuminates it from many angles with new aperçus: that thinking in the natural sciences is democratic; that there is a Great Divide between scientific praxis and its official, dead image which includes a Methodology (non-existent in practice); that practical knowledge often suffers from systematic collective amnesia in the formulation of the theory; that style influences problem-choice as much as it has an impact on processes of solution; that all scientific innovation is an artistic creation which, although rooted in social factors, yet it is never a direct answer to a demand for "there is never a scientific demand for fundamental changes"; that there is no accomplished science only a becoming one; that even weighing and measuring yield different results according to the purpose of the measurement; and so on.

Into this torrent of ideas rushing at a breathless pace, a fundamental mistake has crept in, which misleads Fleck's readers. Fleck, having stated his case succinctly, actually saying that no external/internal dichotomy exists and that the thought-collective expresses itself through the thought-style of each cognition, he now says that "like all that is socially conditioned, the cognized has its own life independent of the individual". This saying, and other similar ones, has misled also John Ziman: it encourages a vague feeling that some kind of third world of I-independent pieces of cognition may exist, except that these pieces are suspended in a social aether and not in an absolute
world of ideas. Fleck wishes to emphasize that each cognition has its own thought-style and he considers now this cognition with its thought-style as a time-independent permanent entity which can float in a social space of ideas independent of the individual. As proof of this point Fleck refers to simultaneous discoveries by different people, working in full independence from each other. However, as I argued elsewhere, there are no identical discoveries made in full independence from each other: the problems posed are at least slightly different from each other and so are the solutions. What often happens is that in the framework of the thought-style of a new synthesis of several fields of knowledge, somewhat different results are conflated into simultaneous discoveries.\textsuperscript{15}

A few passages later Fleck corrects this mistake, but he is seemingly unaware of having committed it. He repeats the previous statement conditionally only to negate it:

If one wished it to be independent of the individual, one should construe it as socially conditioned, and therefore dependent on the collaboration and communication of many, as many as possible, individuals. One should construct it democratically, taking into account that it would then become much less dependent on time, because the collective (die Masse) develops much more slowly, but also more consistently. This is the way of the natural science.

There is then no time-independent constancy, neither in a Platonic/Popperian third world nor in a social thought-collective which having interacted with the individual to form the thought-style of some cognition is now, independent of the individual. A further support for this view can be seen in the fact that Fleck is aware that different thought-styles can coexist in the same person simultaneously typifying different cognitions. These thought-styles, although distinct, do influence each other by their very coexistence and frequent clashes thus showing their dependence on the individual. If anything is reminiscent here of Riezler’s third reality it is the official, ideal image of science which has nothing to do with scientific praxis and contains according to Fleck the scientists’ religion.

As remarked above, Fleck did not go beyond this. However, he completed one detailed case-study where the actual process of how the thought-collective expresses itself continuously in the thought-style through tradition and education — the whole complex changing continuously — and this is the now celebrated study of the changing images of syphilis and the tortuous ways how the Wassermann reaction became a test for this disease.

Kuhnian studies have concentrated on critical analysis of Kuhnian theory
which was modeled on Kuhn's Copernican case-study and did not suffice as theoretical framework for Kuhn's Black-Body Radiation case-study. Fleck went the other way round, and wrote a case-study which, brilliant as it is, serves as a case-study of only a small part of his theoretical infra-structure. There is much more work to be done if we wish to explicate fully the theory that is compressed into the short 'Zur Krise der "Wirklichkeit"', which should actually have been translated as 'A Short Contribution to the Issue of "Reality"'.

Allow me to conclude with one more beautiful quotation from the paper:

Natural science is the art of shaping a democratic reality and to be directed by it – thus being reshaped by it. It is an eternal, synthetic rather than analytic, never-ending labour – eternal because it resembles that of a river that is cutting its own bed.

NOTES
4 *Naturwissenschaften* 17 (1929), 425–430. Baldamus, and Trenn and Merton do not refer to it. Schäfer and Schnelle include it in their introduction. Barbara Rosenkrantz emphasizes its importance in her review of the English translation of Fleck’s book in *Isis* 72 (1981), 96–99, and acknowledges her indebtedness to Paul Forman. Professor Nathan Rotenstreich tells me that in the 1930’s the late Hugo Bergmann extolled the importance of this paper in his lectures on philosophy in Jerusalem.
6 The Popperian allusions are obvious here. The reference is to Sir K. Popper: *Objective Knowledge*. Oxford, 1972. It is relevant also to compare the view with Hilary Putnam’s theory of realism.
7 Riezler, *op. cit.*., p. 705.
9 A recent philosophical masterpiece, which is written in purely philosophical terms but actually touches on issues which Fleck could easily translate into terms like thought-collective and thought-style is Nelson Goodman: *Ways of Worldmaking*. Harvester Press, 1979.
10 See the section 'Is the Meaning in the Text?' in my 'The Distinctiveness and Univer-

11 I cannot resist repeating once more the Baconian "On waxen tablets you cannot write anything new until you rub out the old. With the mind it is not so: there you cannot rub out the old till you have written in the new". In 'Temporis Partus Masculus' in B. Farrington (ed.): *The Philosophy of Francis Bacon*, Liverpool University Press, 1970, p. 72.

12 Fleck's own definition of thought-collective is (English translation p. 39): "a community of persons mutually exchanging ideas or intellectual interaction".

13 I chose this example because this one is at least documented in a full-length monograph (Y. Elkana: *The Discovery of the Conservation of Energy*, Harvard University Press, 1975).


DIETER WITTICH

ON LUDWIK FLECK’S USE OF SOCIAL CATEGORIES IN KNOWLEDGE

A consideration of Ludwik Fleck’s ‘Contributions to Epistemology’, which is our present theme, is confronted first of all with the difficulty of having to differentiate between the philosophy of science (Wissenschaftstheorie) and epistemology (Erkenntnistheorie). In my opinion, the two disciplines differ from one another not only in so far as their statements’ claims to validity are concerned, but also in what they attempt to emphasize about the objects on which they reflect. Epistemology is understood here as a part of philosophy, as a discipline which, like philosophy, is concerned with a fundamental orientation of the totality of human action in society, and which thus, unlike philosophy of science, is concerned not only with one particular area of human activity. Although its author only uses the term ‘epistemology’ for it, Fleck’s work is primarily one of the philosophy of science. The social and historical character of a certain area of human work, i.e., of scientific work, must be made known. But in Fleck’s efforts to fulfill this purpose, he is forced — to some extent implicitly but to some extent also explicitly — to refer to characteristics that go beyond the specificity of scientific knowledge and are involved in human knowledge in general. To that extent Fleck’s epistemology is made clear by means of his philosophy of science. In reference to the latter, we must also begin with Fleck’s epistemology.

I agree with Mr. Heelan that Fleck’s ideas concerning scientific knowledge’s social and historical character are definitely to be positively evaluated in comparison to the conception of science held by the Vienna Circle. But in my opinion this is not only the case with reference to the positivism of the 1920’s and 30’s. With reference to the later ideas of Kuhne, too, Fleck not seldom proves himself to be superior, also epistemologically. Of course a comparison between Kuhne and Fleck is obvious. Kuhne was the first one to make Fleck’s Genesis and Development of a Scientific Fact accessible as a source for contemporary philosophy of science, and we also have this circumstance to thank for our colloquium in Hamburg. And still it seems to me that a remark recently made by Kuhne (in the preface to the American edition of Fleck’s work), viz., that Fleck’s work is still a “largely unexploited resource” (1979: x.), is also applicable to the former’s own philosophy of science.

In order to underscore this, I should like first to refer to two central notions in Fleck, *viz.*, to that of the 'thought-collective' and that of the 'thought-style', which are semantically and functionally related, but by no means identical, to Kuhn's concepts of the 'community of scientists' and 'paradigm' or 'disciplinary matrix'. From the very beginning, Fleck's fundamental concepts involve many more social connections in scientific understanding than do those of Kuhn, and for the following reasons:

(1) Fleck's notion of 'thought-style' is not at all bound up with the fate of individual scientific theories, as is Kuhn's concept of paradigm. Within the framework of the same style of thought, many theories can be constructed, circumstances permitting, including so-called competing ones. This becomes clear when Fleck speaks of "contemporary science as a specific, thought collective construct" (1980: 103).

(2) The thought-style of a science is not necessarily limited to a community of *scientists*, as is Kuhn's paradigm. Rather, it can be shared by social groups extending far beyond science. It can even influence "all — or many — concepts" of an 'epoch' (1980: 15): the same style of thought can find expression in a certain religion, in a certain developmental stage of science, art, morality, etc.

(3) For Fleck there is always a certain thought style dominating in a scientific thought-collective, but it is not at all the case that that style is the only one influencing it (1980: 61).

Thus for Fleck, as opposed to Kuhn, scientific knowledge is seen to be theoretically stronger as an integrated component of a more all-encompassing spiritual culture. His method is reminiscent here of Hegel's view of the history of philosophy, although Fleck could hardly have been aware of that. Still, I do not want to claim that the concept of 'thought-style' can make that of the 'paradigm' superfluous, only that Kuhn's concept of paradigm does not have nearly the theoretical possibilities possessed by Fleck's concept of thought-style when it is a question of theoretically grasping scientific thought as a part of society's spiritual life process. Kuhn's fundamental concept lessens the chance of discovering precisely this connection for the philosophy of science.

Fleck also makes a clearer distinction between the development of entire scientific disciplines, or science as a whole, and individual theories within science than is later made by Kuhn. Investigations of this, which will later be called 'theory dynamics', are, at least according to Fleck, not identical to those investigations attempting to clarify the general development structures of science. Accordingly, that which philosophy of science can figure out
about science is on Fleck's view not exhausted in reflections concerning theory dynamics. I believe this is correct, for a limitation of the philosophy of science to the latter would be similar to a doctrine of industrial economy without one of political economy. Fleck makes the aforementioned distinction clear in calling the development of the individual scientific disciplines a conglomerate of thought styles, errors, detours, etc. while allowing only one style of thought to be decisive for the formation of individual theories, the continuation of which thought-style then clearly divides every ontogenesis of theories into several phases (1980: 28ff.)

As far as theory is concerned, Fleck further favorably distinguishes himself from Kuhn in more consciously distinguishing between theoretical products themselves and the convictions, behaviors, attitudes, etc. which scientists form in relation to theoretical results. For Fleck, for example, a contradiction to the system of knowledge held by a thought collective 'appears' to it to be unthinkable, it 'feels' that the principles of a strange collective are arbitrary, it 'suspects' a 'strange thought-style' of being metaphysics (1980: 143). Kuhn on the other hand often uses concepts which belong to the so-called 'competitive theories' themselves, instead of concepts like 'feel', 'appear', 'suggest', etc., with which Fleck tries to show the relation of subjects to the products of scientific work. This is doubtlessly favorable to Kuhn's much-debated thesis on the "incommensurability of competing theories."

It is different with Fleck. All of the aforementioned differences between his theory and Kuhn's allow him a much freer rein in dealing with an actual, epistemologically speaking highly significant problem, viz., the problem of the historical continuity of theoretical contents regardless of all historically necessary and scientifically favorable discontinuity. Not only 'intra-' but also 'inter-collective communication of thought' (Denkverkehr) is very self-evident for Fleck (1980: 146). Concepts for him are usually determined by 'ancestors' from earlier thought-styles; even the results of the pre-scientific stages of a discipline, such as those of alchemy (1980: 143), or of 'common sense' (1980: 143) are according to Fleck still active in contemporary science. The 'incommensurability of competing theories' thus remains a rare exception for Fleck; at best it refers to ideas from thought-styles which are historically very far removed from us (1980: 185).

Just as Kuhn (1962), Fleck did not carry out his concepts of epistemology and philosophy of science very strictly, either logically or semantically speaking. Often he conceives of a certain thesis in one place as universal or apodictic, to considerably weaken it or partially reject it in another. For example, it openly contradicts the overall intention of Fleck's work when he
says at one point that merely "three-fourths" and only "possibly the totality of the contents of all sciences were conditioned and explainable in terms of history of ideas, psychology and sociology of ideas" (1980: 32). Such a manner of presentation unfortunately departs from a tradition in epistemology, for which Kant's Critique of Pure Reason became exemplary. In my opinion, however, it is favorable for those elements of Fleck's concept which in Marxism challenge us most strongly to disagree, viz., Fleck's epistemological relativism.

Fleck understands that all knowledge is necessarily socially and historically determined. Yet Fleck turns the insight, so eminently important with respect to the Vienna Circle's positivism, against the objectivity of the content of knowledge. In Fleck's theory, there is no place either for the 'things themselves', of which Mr. Heelan spoke, together with Martin Heidegger, or for the 'pseudo-profiles' and 'pseudo essences' which are distinguished from the former. Fleck is not a realist in this matter — in this too we can agree with Mr. Heelan. But this fact makes many of the states of affairs he has affirmed for science difficult, if not incomprehensible. I am thinking, for example, of the anomalies, which are also important for Fleck's explanation of the history of science, but which cannot be based on any observations in accordance with a thought style. I would remind the reader of the state of affairs which he has laid out, which can be transmitted above and beyond many different styles of thought. As I see it, a meaning can be found in all of this only when one admits the ability of human knowledge to achieve a content independent of the conditionedness of historical circumstances, although it remains the case that human knowledge can only be given in a historical and socially relativized form. Because Fleck argues against this, I call him an epistemological relativist. This relativism allowed him to appear interesting even to ideologists in whose approval Fleck certainly was not interested, viz., to his fascist reviewers in the Germany of that time. With his epistemological relativism, Fleck succumbs to a philosophical 'thought-style', if we want to express it in his terminology, which he obviously takes to be self-evident, although it was not actually so either in his time or in any earlier period. In this question, so epistemologically and ideologically important, Fleck could not divorce himself from either the Vienna Circle or the sociology of knowledge, in spite of the fact that he had strongly criticized both directions in other respects, a fact to which Mr. Heelan has also referred. I do not want to investigate the social background of Fleck's epistemological relativism; I would, however, like to refer to several methodological ways of procedure which allowed this relativism to be acceptable to Fleck, similar to the way it was later to Kuhn.
We see then that Fleck examines thought-styles and the theoretical results consistent with them only on certain points, to see whether there are practical circumstances which have been beneficial to their genesis and development and what these practical circumstances are. This is the case, for instance, where he introduces the “social importance of the syphilis question” (1980: 97), the “rivalry among nations” (1980: 102f.). Or a “social tension”, which “brought order to research.” (1980: 77f) in relation to the successes of syphilis research at the beginning of our century. At one point he even states the belief that such a practical-political institution as a “team of civil servants” “actually deserves the title of discoverer of the cause of syphilis” (1980: 25). But out of such a description of an individual scientific occurrence, with which Fleck is particularly well-acquainted due to his profession, no corresponding problematization, let alone theoretical position, can grow. As far as the latter is concerned, beyond the formula that ‘changes in attitude’ produce ‘changes in thought’, he does not discover what causes such a change in attitude, or why knowledge can be “the most strongly socially determined activity of man” (1980: 48), concerning which argument is certainly possible.

Fleck also limits himself, in those places where he attempts to theoretically grasp the evolution of theories, to those phases conceiving which the practical background is the most difficult to fathom. As was the case with Kuhn later, Fleck only worked out two stages of the development of theories: the stage of their unlimited functioning and the stage of their gradually coming into question due to their less and less mastering the anomalies contradicting them (1980: 42). How attempts at theory occur in the first place, however, is considered scarcely or not at all. And yet it is precisely the stronger theoretical consideration of the early phases of theory development which would have provided the opportunity of having to resolutely look for the needs which press for a new theoretical attempt.

Fleck proceeds in just as one-sided a fashion when he investigates the influence of attained styles of thought and theoretical positions on various human activities. Again it is their relation to the practical process of life which is considered least. In only a few places in his work (e.g., 1980: 138) does he briefly take this up. The influence of thought styles on empirical research, on the reception, evaluation and understanding of earlier attained knowledge, on the construction of concepts and terms, and on the activities of teaching, learning and research, however, is investigated thoroughly. I consider this rather complex view of the influence of an attained state of knowledge on the production, dissemination and appropriation of knowledge to be a definite merit of Fleck’s. Still, it cannot make up for Fleck’s stingy
treatment of the practical application of science, whether with respect to
the epistemological view of the problem or with respect to its solution. This
was precisely the objection which Ms. Masterman raised against Kuhn in the
sixties. It also applies to Fleck.

Thus Fleck always breaks off the theoretical reflection on thought-styles
and scientific activity at the point at which they should be questioned con-
cerning their connection to the practical life process of the society. Nothing
compels him, therefore, to consider or even seek out concepts by means of
which an attempt is made to overcome this doubtlessly handed-down lack of
considerations belonging to philosophy of science. Karl Marx's *Theories of
Surplus Value*, for example, which traces in great detail the genesis of a
scientific discipline and of 'theory dynamics' in connection with practical
life process, had in 1935 already been around for a long while. And as far as
the natural sciences are concerned, the notable lecture in 1931 of B. Hessen,
the Soviet philosopher of science, as well as the beginnings of the British
'Social Relations of Science Movement' (J. D. Bernal, J. B. S. Haldane,
P. M. S. Blackett, etc.) also precede Fleck's book.

All of these methodical failings provided favorable conditions for Fleck's
epistemological relativism and finally made a corresponding conception of
truth possible. In relation to it, too, we again find the contradiction existing
between various occasional ideas of Fleck and his very different theoretical
concept. His clever remark (which could have grown out of the tradition of
a Herder or Hegel) that there are "probably no total errors, just as there are
no total truths" (1980: 31) is unfortunately not put into effect conceptually.
Totally justifiably, Fleck polemizes — just as Kuhn does later — against a
conception of truth which knows only the values of 'absolutely true' and
'absolutely false': he is precisely on the mark when he raises the objection
against this conception that it is 'incredibly naive' to believe that the ancients
thought falsely and we moderns think truly (1980: 69). He is equally justified
in arguing against the division of scientists into "two classes", *viz.*, "into the
bad characters, who do not find the truth, and the good characters, who do"
(1980: 153). The theory of truth attacked by Fleck is indeed incompatible
with the historical development of thought. But because Fleck does not give
any further theoretical attention to his dialectical vision of the truth problem
cited above, he must now rigorously relativize 'being true' — if he wants to
harmonize this notion, at least externally, with the historical development
of knowledge. He can perceive "no other truth than the truth of culture to
be the truth of nature" (1980: 48).

For a Marxist, Fleck's theory leaves a decidedly two-sided impression.
Bold philosophical-scientific ideas, the further development of which promises many new insights even today, stand next to epistemological ideas for which this claim can, in my opinion, in no way be made. But the latter does not necessarily present an obstacle to the former. Fleck's epistemological relativism is neither an unavoidable presupposition nor a consequence of that which we find valuable in him.
STEVEN SHAPIN

HISTORY OF SCIENCE AND ITS SOCIOLOGICAL RECONSTRUCTIONS*

One can either debate the possibility of the historical sociology of scientific knowledge or one can do it. Ludwik Fleck took the latter course of action. In *Entstehung und Entwicklung einer wissenschaftlichen Tatsache* Fleck's overriding concern was with the interpretation of a particular episode in the history of science, and his focus never strayed from the empirical materials pertinent to that task. His more general theoretical statements always arose out of and referred to the historical particulars and circumstances of that episode. Thus, one way of characterizing Fleck's book is to regard it as the work of a practising scientist, intimately familiar with the genesis and career of the Wassermann test: and this would not be an incorrect characterization. Another way of appreciating his accomplishment would be to see it as a piece of empirical history, providing a concrete exemplification of the sociology of scientific knowledge. The only wholly misguided approach to Fleck's work would be to distill his theorizing out of the empirical concerns in which it was grounded.

As a historian I read Fleck as writing history; that is, I recognized the sort of scholarly enterprise in which he was engaged. However, there is a more particular reason why I recognized Fleck's work. Over the past decade there has appeared a body of empirical historical and sociological writing, especially in Great Britain and North America, which looks as if it were inspired by Fleck's views, but which, for obvious reasons, developed in ignorance of them. It is therefore with a certain tragic irony that one can say that this body of work is Fleck's legacy.

My job here is not to discuss Fleck's book: that has been done elsewhere in this volume; it is to give an account and appreciation of empirical work in the history and sociology of scientific knowledge in Fleck's idiom. This job is all the more necessary since it is still often said that there are as yet no examples of empirical work that show the propriety and value of a sociological approach to scientific knowledge.\(^1\) That charge needs to be answered, and the best way of doing so is to display the relevant literature and to make

* A somewhat different version of this paper appeared in *History of Science* xx (1982), 157–211. Neither the text nor the bibliography of the present paper has been revised since its final draft in 1982.

as visible as possible its significance to the sociology of knowledge generally.

For obvious reasons some criteria of selection have to be imposed. I will not deal with programmatic statements and will make only brief references to some admirable, and often historically sensitive, theoretical literature in the sociology of knowledge.² It would be quite incorrect to regard empirical literature as if it were merely a ‘testing’ of some theoretical programme. Even though empirical work has an important bearing on the validity of theoretical positions, its significance may only be properly appreciated if it is understood on its own terms. In addition, I have attempted to pre-empt some obvious criticisms by discussing work that deals with scientific ideas and practice and largely excluding many admirable studies that treat, for example, images of science, the rhetoric of spokesmen of science, views of scientific method not clearly related to practice, and the sociology of scientists as opposed to the sociology of science. I shall resist any temptation to discuss the social history of science as if it all bore upon the sociology of knowledge; much of it, such as work dealing solely with institutional aspects or the career-structure of science, despite an occasional gesture towards sociology of knowledge, is not concerned with knowledge or practice.³ For my part, I see no danger in “the history of science losing its science”, but much literature in the social history of science has less of a connection with the sociology of knowledge than many apparently traditional exercises in the history of ideas. Thus not all relevant empirical studies come clearly labelled as sociology of knowledge: many of the most significant achievements give little explicit warning that sociological explanation has been perpetrated, and some lay claim to that accomplishment without evident basis. I make no apology, therefore, for attending to what some authors do, in occasional (well-meaning) disregard of what they say that have done.⁴

I propose to discuss empirical literature from the point of view of a series of related interpretive perspectives. Therefore some justification of the order in which I treat these materials may be required. For many scholars the sociology of scientific knowledge is equated with studies of the role of ‘external’ macrosociological factors such as social class. Since this is, as I shall show, an inadequate characterization, there is some point in proceeding, as it were, from the inside out. I begin by examining studies which treat structures and processes usually thought to belong to scientific culture and the scientific community. Although much of this work might be readily accepted by historians of ideas, I shall try to establish its relevance to the sociology of knowledge. Only then shall I deal with work which relates scientific knowledge to factors usually thought to belong to the wider society. And I shall
conclude with some brief remarks about what sociological explanation of scientific knowledge actually looks like in practice and how it relates to some demarcations (such as those dividing the 'internal' from the 'external' and the 'rational' from the 'irrational') conventionally deployed in present-day scholarship.

I. CONTINGENCY AND THE SOCIOLOGY OF KNOWLEDGE: OBSERVATION AND EXPERIMENT

If scientific representations were simply determined by the nature of reality, then no sociological accounts of the production and evaluation of scientific knowledge could be offered. Perhaps one might attempt to understand why certain features of reality were selectively attended to at different periods and in different social settings, but of the resulting knowledge nothing of sociological interest could be said. It would be pointless to argue against the kind of naive realism and positivism which has few, if any, philosophic proponents at present. The underdetermination of scientific accounts by reality, and the 'theory-laden' nature of fact-statements are both quite widely accepted. Nevertheless, the way forward from these basic sensibilities towards a full-blown sociology of scientific knowledge is by no means generally recognized. Even so, this is the best way to proceed: the sociology of knowledge is built upon an appreciation of the contingent circumstances affecting the production and evaluation of scientific accounts.

While it may be banal to say that statements of scientific fact may be theory-laden, it is not, apparently, banal to demonstrate this empirically and to pin down the specific network of expectations and goals affecting the production and evaluation of statements of fact. Quite simply, there are few such historical studies, and even fewer studies of observation reports. Historians act as if, after all, observed facts count as a 'hard case'; making a fact into a historical product (an artifact) is an exercise which historians of science approach with great caution (even though scientists do it routinely). The small number of historical studies we have are therefore unusually detailed and circumspect. As a foundation upon which one might build a sociology of knowledge they are worth considering.

In the 1860s the English biologist T. H. Huxley undertook a microscopical examination of a number of alcohol-preserved specimens of sea-bed mud dredged up from the North Atlantic some ten years previously [20, 23]. He discovered a particularly primitive form of naked protoplasmic Urschleim that had recently been discussed by the German biologist E. H. Haeckel. These
living forms he named *Bathybius haeckeli*, and he produced drawings of what he had seen through the microscope. Subsequently, Huxley’s observations were confirmed by Sir Charles Wyville Thomson on the *Challenger* expedition, as well as by a number of American, English and German biologists and geologists. *Bathybius* was a fact. It was also a significant fact; the existence of such a life-form served as crucial evidence supporting a number of scientific theories. It served to establish a link between the nebular hypothesis of planetary evolution and organic evolution much sought after by some Darwinians, as well as by both Huxley and Haeckel. It also served as a datum favouring abiogenesis against the views of Louis Pasteur. Thus it figured in the vitalist-mechanist debates raging at the time. To those who maintained that there was a continuity between living and non-living forms and that life might be easily and normally generated out of non-living materials *Bathybius* was not an anomaly; it was a non-contentious fact of nature. It was seen by very many observers. Unfortunately, evidence soon began to appear against the reliability of that perception. Some biologists claimed that *Bathybius* was not a fact but an artifact: it had been created by a combination of observers’ imagination and the precipitating effect of alcohol on ooze. *Bathybius* was nothing but calcium sulphate in an amorphous colloidal form, and this is the view taken by present-day scientists. Nevertheless, those who strongly supported *Bathybius*-as-fact continued to fight, disputing the critics’ observations. *Bathybius* died a gradual death, assisted by the writings of scientists who opposed the theories which its existence had been used to support.

There are several historical studies which reinforce the general lessons of the *Bathybius* episodes. Baxter and Farley [1] have produced a meticulous account of controversies over cytological observations of meiosis in the period just before and after the re-discovery of Mendel’s work.\(^8\) According to whether or not the observer subscribed to Weismann’s picture of “reducing division” different accounts were given of chromosome behaviour. There was no agreed interpretive framework within which cytological observations could be unambiguously situated. However, with Mendel’s re-discovery, sections of the biological community embraced the chromosome theory of inheritance and, as Baxter and Farley say [1, p. 172], “the cytological work was reinterpreted so as to fit into Mendel’s scheme”. The microscope has figured significantly in several other historical studies showing the interpretive nature of perception: R. C. Maulitz’s study of Theodor Schwann’s observation of cell genesis as crystal formation [13], J. V. Pickstone’s account of early nineteenth-century observations of the “globular structure” of tissues [17], Sandra Black’s work on the anatomy of the neural synapse in the
1890s [3], and L. S. Jacyna’s examination of Goodsir’s cell theory [11]. All these historical studies reject the notion that ‘erroneous’ perceptions might be sufficiently explained by defects in contemporary observational instruments.⁹

Naked-eye observation reports do not present a radically different picture. One of the finest studies in this area is M. J. S. Rudwick’s account of disputes between Darwin and other geologists about the origin of the Parallel Roads of Glen Roy in Scotland [21]. In this episode structures were labelled differently according to whether one held the theory that the Roads were formed by elevation of the land above the sea, a falling sea-level, or by lakes of glacial or non-glacial origin. In some instances, what were ‘minor roads’ in one version were no roads at all in another. Broadly similar orientations are also available in Rudwick’s more recent study of controversies during the 1830s over what came to be known as ‘The Devonian System’ [22]. This is a detailed account of negotiations among geologists over the classification of strata according to their fossil contents. What proponents of one theory regarded as crucial confirmatory evidence was treated by advocates of another classification as an intolerable anomaly and support for their alternative order of strata. Rudwick displays the fine-structure of negotiations over empirical evidence which eventually culminated in a consensually-accepted order.¹⁰ Negotiations and conflict over fossil evidence also figure in A. J. Desmond’s treatment of a nineteenth-century controversy over the morphology and physiology of the dinosaur [8]. In the 1830s it was the generally-held view that the Mesozoic saurians were “monstrous lizards” and their similarity to extant lizards was asserted. Against this position the English anatomist Richard Owen mounted a vigorous campaign. He construed the fragmentary fossil bones as evidence of reptiles so highly developed that they possessed traits associated with pachyderm mammals. On this basis Owen raised the Dinosauria to ordinal status, giving them a taxonomic distinctiveness they lacked on existing theories. Desmond shows that this was a strategy Owen adopted to establish the fact of degeneration, and thereby to argue decisively against the materialist Lamarckian transmutationism that asserted unabated increase in the complexity of fossil forms through time. Owen and the Lamarckian Robert Edmond Grant disagreed in their readings of the fossil evidence because they diverged in their views on the more fundamental matter of species change. By literally “designing” or “inventing” the dinosaurs Owen hoped to counter both materialist intellectual tendencies in the culture as a whole and transmutationism in biology.

Negotiations over the correct classification and interpretation of visual
evidence also figure in Winsor's sensitive study of nineteenth-century work on barnacle larvae [26], and in Burkhardt's excellent paper on the phenomenon of 'telegony' [2]. In the early nineteenth century it was an accepted fact that male mates might impress their character upon the offspring that a female had with subsequent mates. Careful paintings of the celebrated case of Lord Morton's mare were commissioned, definitively showing that an Arabian mare that had borne a foal to a quagga stallion produced foals with quagga-like characteristics when subsequently mated to an Arabian stallion. At the time there were a number of interpretive schemata that made sense of the phenomenon as a normal fact of nature. When theories of inheritance changed later in the century, that 'fact' became an 'anomaly', and eventually a 'non-fact'. The effects of telegony were no longer observed, and even the original paintings, that had been the exemplars for establishing other instances of telegony, were no longer deemed to show the phenomenon. Burkhardt makes some intriguing suggestions about the moral uses to which telegony beliefs were put in the nineteenth century, although this is by no means his major concern. However, we shall later discuss disputes over observed facts in cerebral anatomy in which considerations in the wider society did have an important bearing upon reports of what was seen [121].

Before one moves from disputes over facts to a full-blooded sociology of scientific knowledge there is one major obstacle to overcome. Suppose, it might be objected, that while scientists disagree over observations and interpretations of observational evidence, they may readily give assent to impersonal criteria for making observations and performing competent experiments: in the end, these non-social criteria will adjudicate disputes of this sort. Recent empirical studies of modern scientific controversies over the reality of certain phenomena do not give support to this detour around a sociology of knowledge. H. M. Collins has studied controversies in the 1970s over the existence of high fluxes of gravitational radiation [4]. The initial claim by one scientist to have built an 'antenna' which detected such radiation was soon countered by a host of criticisms. Other 'antennae' were devised and put into operation that produced no empirical support for the reality of the phenomena supposedly detected by the original. Those experimenters who were committed to the reality of the phenomena claimed that 'competent' experiments were those which reliably detected the radiation, while those which failed to do so were judged to have been 'incompetently' performed. Conversely, scientists committed to the opposite view judged that experiments which indicated high fluxes of gravitational radiation were not competent experiments. Different communities' views about what
the natural world contained were used, so to speak, to calibrate the experiments. Since ‘experimental design’ cannot be divorced from the commitments of the communities that frame and evaluate experiments, there is no possibility of avoiding a sociological account of fact-production by appealing to impersonal rules of experimental procedure. As it happens, Collins’ work was undertaken when the outcome of the controversy was not known. Since that time, the ‘fact’ of no high fluxes of gravitational radiation has been established, and Collins has followed the controversy to its resolution: “The existence of (hf) gravity waves is now [in February 1981] literally incredible . . . . Their demise was a social (and political) process” [5, p. 54]. Elsewhere, Collins and Pinch have come to a similar conclusion about competent experiments and the social construction of scientific facts from a study of parapsychological research [7, 18].

A still more detailed account of the contingency of experimental findings is Pickering’s study of the recent history of experiments designed to discover whether or not free ‘quarks’ exist in matter [16]. It has been commonly assumed that experimental results, provided the experiments are competently carried out, can compel assent from scientists. But Pickering offers a particularly striking exemplification of the Duhem-Quine thesis: that all experiments are in principle open to criticism. In the case of the quark work a series of experiments conducted in Italy produced no evidence of free quarks, while others performed at Stanford were regarded as having demonstrated their existence. The Italian physicists calibrated their experimental procedures by their production of credible results — in their case the non-existence of non-zero charge in a version of the Millikan oil-drop experiment. The experiment was deemed to have been competently performed because of its reliable production of no fractionally-charged objects. Since the existence of fractionally-charged objects would be a highly abnormal addition to physicists’ natural world, the Italian experiments were not systematically criticized and the experimental findings were accepted. This was not the case with the Stanford work, even though it was carried out with extreme rigour. The Stanford finding of charges of $\pm 1/3e$ has been subjected to intense scrutiny, albeit mainly out of the printed public forum. The Italians have been at pains to identify a number of features of the Stanford experimental system which makes its findings less than compelling. Pickering concludes in a similar vein to Collins: “. . . [O]ne cannot separate assessment of whether an experimental system is sufficiently closed from assessment of the phenomena it purports to observe: if one believes in free quarks then the Stanford experiment is sufficiently closed; if not, then it is not” [16, p. 229]. It is the
accepted knowledge of the community that adjudicates; reality is filtered through that knowledge and has no unmediated compulsory force. Similar orientations to the relationship between experimental findings and the acceptable options available to a community of practitioners may be found in Pickering's study of magnetic monopoles [15], as well as in papers by Wynne on the detection of J-rays [27, 28], Nye on N-rays [14], Pinch on solar neutrinos [19], Travis on memory-transfer experiments [24, 25], and Harvey on experimental quantum mechanics [9, 10]. (Some of these papers, to be sure, treat phenomena that the modern scientific community regards as 'pathological', but some deal with currently-accepted facts of nature. In any case, what is offered is not a sociology of error or of pseudoscience, but a sociological appreciation of the processes by which statements of fact are accredited or rejected.) Perhaps the most detailed assessment of the social construction of scientific facts is Latour and Woolgar's investigation of a modern neuroendocrinological laboratory [12]; this stresses the role of scientific apparatus and of the 'literary' processes by which facts are stabilized, although these authors do not share Collins' emphasis on the methodological priority of controversy [6].

Such studies serve to demonstrate that neither reality nor logic nor impersonal criteria of 'the experimental method' dictate the accounts that scientists produce or the judgments they make: they open the way to a sociology of scientific knowledge, and for this reason they are invaluable. However, they do not by themselves constitute such a sociology. An empirical sociology of knowledge has to do more than demonstrate the underdetermination of scientific accounts and judgments; it has to go on to show why particular accounts were produced and why particular evaluations were rendered; and it has to do this by displaying the historically contingent connections between knowledge and the concerns of various social groups in their intellectual and social settings. In this respect the empirical work discussed up to this point, however particular its historical focus, has a fundamentally philosophical character. Philosophical work sympathetic to a sociology of knowledge ends by displaying the contingency and open-ended nature of scientific knowledge; fully-developed sociology of knowledge starts with the recognition of historically contingent factors and then proceeds to array and stress their different roles in scientific action. Having said that, we are now in a better position to appreciate the sociological significance of a body of empirical studies that relate divergent bodies of knowledge to the concerns of social groups within the culture of science.
II. PROFESSIONAL VESTED INTERESTS AND SOCIOLOGICAL EXPLANATION

Within the scientific community, and within any given specialty or discipline, there will typically exist a distribution of different skills and technical competences. For example, some scientists will be more skilled than others in mathematical demonstration; some biologists will be more adept at morphological studies of animals and others will be highly skilled in biochemical analyses; within a scientific subculture there may also frequently be a division between theoreticians and experimentalists. These technical abilities and competences will have been acquired through processes of socialization; they will have represented a considerable investment on the part of the scientist, and he will naturally tend to deploy them, to show their value in scientific work and to extend the possible range of their application. Such skills and technical competences therefore represent a set of vested social interests within the scientific community. There is every reason why a scientist should wish to display the value and scope of what he can do, even to the extent of criticizing the value and scope of others' acquired skills and competences. In the process of defending these 'professional vested interests' conflicts may arise within the scientific community over the nature of phenomena. If nature is constituted in one way, then its investigation may best proceed through the application of one set of competences; if it is constituted differently, then perhaps another set of technical competences are called for. In this way, professional vested interests may form the middle link which connects, on the one hand, controversies about the nature of phenomena and, on the other, conflicts over the availability of resources or the securing of credibility for scientists' work. The analysis in terms of socially acquired technical competences may even be extended to encompass scientists' investments in the practical or interpretive line of their previous work. If a group of scientists has accomplished a body of publicly available research in which it argues for a given point of view, theory or interpretation, it may well wish to defend that position from attack and display its value and scope over other positions — even if it is technically able to work from another cognitive or practical orientation. Naturally, there is no coercive force involved and scientists may readily shift their positions, seek to acquire other competences, or see the advisability of terminating a controversy to further shared interests. What is involved is a strategy for defending and furthering interests, based on complex calculations about the consequences of various courses of action.

There is a substantial body of history of science literature that shows the
explanatory value of attending to professional vested interests. The significance of this perspective may best be shown by proceeding from the smaller to the larger scale of such interests. In November 1974 two new and unusual elementary particles (named “J-psi” and “psi-prime”) were discovered by a group of high-energy physicists. Theorists in the community were faced with the problems of explaining the new particles’ properties and of situating them within a coherent framework that also dealt with existing particles. Andrew Pickering’s study of the controversy between advocates of the “charm” and “colour” models, and the quick resolution of that controversy, is built upon sensitivity to the pre-existing distribution of interests among specialist groups within high-energy physics [40]. Without entering into the technical details of each model, charm’s proponents were so successful in vanquishing their colour model rivals that within eight or nine months a solid consensus in favour of charm had developed; within two years colour’s advocates had been effectively isolated and the charm model had been solidly established. What was the basis of charm’s success and colour’s failure? Pickering demonstrates that the charm model intersected with, and could be readily integrated into, a range of existing bodies of theoretical practice in high-energy physics. For example, charm generated a puzzle — to do with rules for interpreting the longevity of certain particles employed by hadrodynamics; it offered a solution to this puzzle which provided a programme of work for experimenters and hadron spectroscopists; and it gave support to, and generated support from, a group of important “gauge theorists” who saw ways by which the success of the charm model could give additional credibility to the “gauge theory revolution” in quantum mechanics. Moreover, the charm model used conceptual resources that were very widely distributed in physics; as Pickering says, “whenever the model encountered mismatches with reality the resources were available to essentially anyone to attempt to fix it up, and for others to appreciate such work” [40, p. 125]. By contrast, very few bodies of practice in theoretical physics incorporated resources associated with the colour model. Charm succeeded insofar as it was successfully insinuated into a range of bodies of practice; the greater and the more consequential the extent of that integration into practice, the more charm appeared as a fact of nature rather than a human contrivance.

In this particular case charm theorists could have done colour theorizing and vice versa; each group had acquired through their socialization into the high-energy physics subculture the competences to do both charm and colour theorizing. It was not a question being unable either to do or to see the point of the other’s theorizing. Nevertheless, given the pre-existing distribution of
theoretical practices, each made the evaluation that gave most promise of validating and extending the range of applicability of its practices. Again, no coercion was evidently involved.

Let us move to consider competences within a scientific community that are not so readily acquired or discarded. One example is John Dean’s examination of a series of controversies among twentieth-century botanists over the correct classification of plants [31]. One group of practitioners has maintained that species are to be delineated on the basis of their morphology while the other group has claimed that experimental techniques of various kinds (including transplantation studies, cytological and biochemical work, and measures of genetic exchange) are required for a correct classification to result. These disputes have been going on since at least the 1920s and are still unresolved today. Each group is quite capable of generating its own taxonomy employing its preferred techniques. As might be expected, sometimes the taxonomies render given bits of botanical reality differently. In the case of the *Gilia inconspicua*-complex experimentalist taxonomists, using cytological findings, discern five species, while morphological taxonomists identify just one. In another *Gilia* complex (*Tenuiflora-latiflora*) the situation is reversed; morphological criteria identify four distinct species while information about gene exchange points to just one. Each set of species criteria, so to speak, works, and each can be used to further the practical concerns of the classifying communities. So each group of scientists construes botanical reality differently. Each group is also distinguished by its members’ acquired technical competences and, to a large extent, by the institutions in which they work. The more traditional taxonomists have been trained in morphological techniques deploying the existing Linnaean system of nomenclature and identification. Many of them work in herbaria producing monographs and flora which aim to provide clear-cut means of distinguishing taxonomic groups on grounds of their gross appearance. Botanists who have vigorously criticized the herbaria taxonomists (the experimentalists or “biosystematists”) tend to have been trained in genetics, cytology, ecology and related disciplines and to work in university research departments. Thus the criteria each group advances as the basis for a proper classification act to defend and further its investments in socially acquired technical competences. The groups have on occasion competed for resources, but in the main they have worked out a *modus vivendi*, with the result that alternative techniques for classification also stably co-exist in the botanical community.

Sometimes differing evaluations of statements of fact hinge upon scientists’ differing investments in both practice and theory. A particularly clear example
is provided in Robert Kohler’s study of the reaction to the discovery of the enzyme zymase [32]. In 1897 Eduard Buchner reported the production from the intracellular juices of yeast of an extract that was capable of fermenting glucose to alcohol and carbon dioxide. At the time of this claim it was generally believed that the scope of enzymes in vital processes was very limited; so far as was known, enzymes only mediated the one simple reaction of hydrolysis, and all functioned outside of the cells. The great majority of vital reactions were thought to be under the control of living protoplasm. Moreover, Pasteur’s work had shown that the presence of actual living yeast cells was essential for fermentation to occur. The existing state of knowledge and practice was one basis for scepticism about Buchner’s findings. Bacteriologists, histologists, botanists and physiologists generally disputed the reality of Buchner’s zymase; their resistance was bound up with their investment in the protoplasmic paradigm. Even more interestingly, technologists, particularly in the brewing industry, were initially sceptical. As practical guardians of Pasteur’s legacy, brewing technologists treated fermentation as a physiological act of the intact living cell. Among the strategies critics adopted were suggestions that Buchner’s experiments had been incompetently performed: there could, for example, have been unnoticed spores or yeast cells in Buchner’s ‘cell-free’ extract; or ‘zymase’ could in fact be bits of living protoplasm. Again, scientists’ commitments to the phenomena were used to judge the competence of experiments.

However, other scientists readily credited the existence of zymase and its functions. Predominant among believers were chemists, enzymologists and many physiological chemists. For them zymase was a welcome extension of chemical understanding and practice into the explanation of life. They, like the critics, set out to replicate Buchner’s experiments, but for them initially negative results were a spur to refining the techniques: “at first we too got no positive result, while later we never failed” [32, p. 337]. Eventually, the brewing technologists began to visualize potential practical outcomes of Buchner’s work and conceived of purely enzymatic fermentation that could be easily integrated into new practice; Buchner, as Kohler says, “became the darling of the brewing industry”, and he remained a hero even after it emerged that enzymatic beer was impracticable [32, pp. 337–38]. As Kohler concludes, the evaluation of Buchner’s findings was “profoundly influenced” by “professional commitments and outlook”; “it was not experimental facts that determined attitudes toward zymase so much as previous commitments, experience, and expectations”; there “was no crucial experiment, no certain proof” [32, p. 351]. In a subsequent paper Kohler goes on to show how the
enzyme theory became the cognitive and practical foundation of the developing new specialty of biochemistry in the twentieth century [33].

The explanatory value of attending to the distribution of technical competences and conceptual skills within the scientific community is perhaps best shown in the very well studied “biometry-Mendelism” and “Darwinism-Mendelism” controversies of the late nineteenth and early twentieth centuries. Thus, Garland Allen notices that the early twentieth-century split between Darwinians and Mendelians was paralleled by a dichotomy in the biological community between naturalists and experimentalists [30, also 29]. By and large, those biologists who favoured the Darwinian account of evolution by the natural selection of small continuously-varying characters were trained in descriptive and qualitative methods; their research activities concentrated upon the field and the museum. Mendelians, by contrast, tended to have an experimentalist training; they preferred quantitative methods and worked in the laboratory and the experimental garden. In their view, evolution proceeded by large-scale discontinuous variations. In criticizing the Darwinian position, Mendelians claimed, among other things, that their opponents’ preferred small-scale variations were not inherited and that selection was of doubtful efficacy. But not all biologists with quantitative methodological preferences were to be found in the Mendelian camp. The “biometrical” school, led by Karl Pearson and W. F. R. Weldon, married statistical methods in the study of heredity to a strongly Darwinian commitment to the role of small continuous variations. MacKenzie and Barnes press the explanatory role of socially-acquired resources in their examination of the biometry-Mendelism disputes of circa 1890 to 1906 [36, 35, ch. 6]. Pearson was by training a mathematician; his colleague Weldon was a biologist, although he also studied mathematics for two years at London University. Their joint effort was to mathematize evolutionary biology; as Weldon said “...the problem of animal evolution is essentially a statistical problem” [36, p. 5]. The biometricians’ major critic, the morphologist William Bateson, embraced Mendelism as providing strong support for his view that discontinuous variations provided the stuff of evolutionary change; and he disputed the explanatory value of his rivals’ methods: “the gross statistical method is a misleading instrument” [36, p. 18]. It appeared to more traditionally trained biologists that the new statistical methods tended to devalue their skills, in particular morphological assessment of the individual case. If, as Pearson asserted, the future lay with the mathematically competent, then it certainly did not lie with traditionally trained biologists. MacKenzie and Barnes do not, however, advance the distribution of professional skills as a necessary and sufficient
explanation of scientific controversy. Teaching does not determine future career-choices or judgments. Pearson possessed the competences to adopt a Mendelian framework; too much should not be made of Weldon’s mathematical training; Bateson set aside several important aspects of his technical training [37]. As we shall later see, the controversies between biometricians and Mendelians provide opportunities for deploying other sorts of sociological explanation in addition to those concerned with factors internal to the scientific community.18

One could say that the evolutionary disputes among biometricians and Mendelians (or between Darwinians and Mendelians) occurred within the one scientific discipline of biology; alternatively, one could plausibly point to the groups concerned as nascent sub-disciplines. By the early twentieth century the differentiation of scientific specialties had proceeded a long way towards the present condition. If, however, one moves back in time, one reaches a situation in which the demarcations between what we are accustomed to call scientific “disciplines” were poorly drawn. We may see disputes between “disciplines”, because it may seem to the actors that there is not room and support enough for more than one approach to a given problem area. Something of this sort is apparent in Dov Ospovat’s excellent study of attitudes towards adaptation and teleology among British scientists from the 1830s to the 1850s [39]. Ospovat shows that an important group of British scientists had rejected the explanatory role of teleology and “final causes” prior to the publication of the Origin of Species. This group, including the biologists Richard Owen, P. M. Roget, William Carpenter, Martin Barry and Louis Agassiz, accepted the commonly-held view that organisms (both at present and in the geological past) were “perfectly adapted” to their conditions of existence, but they construed adaptation in a way significantly different from other writers. To the biologists adaptation of structure to function was the outcome of specifically biological laws or patterns; it was not the result of environmental determinism; it was not the effect of the deity’s special creation; and it was not regarded as an explanation in itself. Both the adaptation of existing organisms and their succession in the geological record were to be explained without making reference to teleology. In Ospovat’s interpretation teleology ensured the dependence of biological explanation upon geological facts; the rejection of teleology “secured the independence of biological theory from geology” [39, p. 44]. Those writers who insisted upon the explanatory role of teleology in biology tended to be geologists (including steady-state theorists like Lyell, as well as progressionists like Adam Sedgwick and William Buckland). To them changing conditions in the
inorganic realm determined the changing forms of plants and animals. In making this argument the geologists regarded external conditions as the product of specifically geological forces and laws; geological change was thought of as primary, with organic change dependent upon it. Thus, by stating a dependency relationship in their objects of study the geologists stipulated a similar dependency relationship in the scientific community of the time; by rejecting that dependency relationship in nature the biologists were making a move for an equality of cultural status.

In Osipovat's materials it is far from clear what more than cultural status might have been at stake. Of course, it is not necessary that every such dispute should be analyzed in 'vulgar' terms, and that its explanation should point to command of resources or access to jobs. But neither is there any reason for historians to be squeamish about such considerations and to avoid looking for them. Let us briefly recall the Devonian controversies alluded to in the preceding section. In these episodes actors disputed the correct ordering of certain geological strata, and, in so doing, the best method of ascertaining that order. Morrell and Thackray [38, pp. 461–65] have provided valuable background information supplementing Rudwick's account [22]. One of the protagonists, de la Beche, was suspicious of identifying strata using just their fossil content, and maintained that this was best done by observing the actual ways in which the strata were superimposed. His opponents, led by Murchison, took a different view, and set out to discredit de la Beche's findings and methods. This exposure at the 1836 meeting of the British Association for the Advancement of Science was to be public, and it was expected that the Chancellor of the Exchequer would be in attendance. At this time de la Beche, whose private fortune had been much reduced, had just been appointed first Director of the Geological Survey, and he was completely dependent upon his salary for his livelihood. The British Government, notoriously mean in its support of science, had to be persuaded that de la Beche's methods were indeed reliable. De la Beche's opponents were more fortunately circumstanced: Murchison, for example, was a gentleman-geologist of great independent wealth. So on one level the dispute was about empirical evidence and methods: those employed by de la Beche and the Geological Survey and those employed by Murchison and his allies; on another level it was about the social role of the man of science. Was the State to support scientific research (as de la Beche and others wished), or was the man of science properly to be a gentleman of independent means, not contaminated by State lucre?19
III. INTERESTS AND THE BOUNDARIES OF THE SCIENTIFIC COMMUNITY

Dov Ospovat’s study of differing evaluations of teleological explanation in nineteenth-century British science points, as we have seen, to the hierarchical relations between scientific specialties [39]. But his materials also ramify into the relations between the scientific community in Britain and theological concerns among clerics and in the wider culture generally. Theories of the structure and function of organic forms and of their succession in the geological past bore intimately upon religious proofs and demonstrations as well as upon the moral order that religion underpinned. If purpose was regarded as a necessary and sufficient explanation of the accepted fact of perfect adaptation, then the deity (as the ultimate source of purpose in the world) was implicated in scientific explanation. What happened if one rejected teleology as a satisfactory explanation of natural processes and objects? What if one insisted that the natural world followed its own self-sufficient natural laws and that scientific explanation was the search for those self-sufficient laws and patterns? The effect of such moves was to undermine historically established cultural relationships between natural knowledge and theology. Since the middle of the seventeenth century proofs of the existence and attributes of God had pointed to the evidence of nature as God’s creation. As these cultural relations built up, it became one of the accepted functions of natural knowledge to supply evidence relevant to theological concerns. The body of culture which specifically fulfilled these functions was called natural theology, and it acted as a bridge between theology and natural science. Natural theology also established a hierarchical relation between those who studied the natural world and those whose role it was to interpret God’s ways to man. Natural knowledge was widely esteemed valuable and accurate insofar as it displayed to mankind the evidence in nature of God’s existence, design, power, wisdom and providence. If the practitioners of natural knowledge performed this function, they secured the support of powerful religious institutions in society. If, however, they severed dependency relations linking the natural world to an external source of purpose and spiritual power, then they cut themselves loose from the protection and approval of the clergy.²⁰

In recent years some of the most outstanding work in the social history of science has dealt with aspects of the changing relations between the scientific community and the Church in Britain during the nineteenth century, and with concomitant changing conceptions of nature and natural knowledge. As
we have seen, the geologists discussed in Ospovat's essay were by and large happy to retain teleological explanation in natural science. Teleology performed a number of functions for them: it kept purpose in the natural world and stipulated a dependency relationship subjugating those who studied the organic world to those whose sphere was the inorganic world. But it would be a mistake to treat attitudes towards teleology solely within the scientific setting. A number of the major actors involved were themselves clerics or had strong religious commitments to defend or advance. J. H. Brooke has examined the role of natural theology, and specifically of the argument from design, among British geologists in the 1830s and 1840s [41]. For the Reverend Adam Sedgwick and the Reverend William Buckland (among others) the argument from design was a central theme in explaining the relationship between geological facts and, for example, the fossil record. To ignore the clear evidence of purpose and design in the natural world would, in their view, be unscientific.

However, as Brooke makes clear, design arguments also served an intrinsically religious purpose. Reference to the evidence of purpose and divinity in nature served the function of uniting Christian factions which more contentious religious tenets were seriously threatening to disrupt. Anglican Broad Churchmen, like many of the Christian geologists of early Victorian England, felt that the most serious danger to the moral order was posed by divisions among believers, such as between Church and Chapel, for such cleavages would open the door to secularizing tendencies. Christian geologists were, therefore, happy to accept ambiguities in their natural theology, for the very doctrinal imprecision of natural theology was the foundation of its ieretic function. Pressures on Christian unity were particularly acute during the 1830s and 1840s. By the 1870s these pressures had been considerably relaxed, partly through the removal of civic disabilities from Dissenters and partly through the general opening up of religion to liberal opinions. Correspondingly, many of the social functions that design arguments had been called upon to perform were no longer necessary, and the arguments themselves began to disappear from scientific literature.

Thus the acceptance and propagation of design arguments within scientific culture is shown to be a feature of a society in which the role of the theologian and the role of the scientist were not distinct. In such a society the practitioner of natural knowledge was content that one of his functions should be the provision of evidence to support religion; he accepted a fundamental dependency relationship between himself and the cleric; indeed, there was no clear distinction between the role of the cleric and role of the man of
science. This arrangement did not, as we know, survive the nineteenth century. Certainly, by the middle third of the century sectors of the British scientific community were making bids for cultural and social independence from clerical concerns and clerical control. The strategy adopted to achieve these ends was scientific naturalism. F. M. Turner and L. S. Jacyna have argued in great detail that scientific naturalism is properly seen as a strategy in the professionalization of science in Victorian Britain [54, 56, 45]. The naturalists' strategy involved the rejection of the existing cosmology which linked theological concerns to natural knowledge and the role of the cleric to the role of the scientific practitioner. In that cosmology matter and spirit were distinct ontological categories; spiritual entities were the ultimate source of power, plan and activity in nature, thereby rendering the material world subservient to immaterial agencies. In its place the scientific naturalists erected a monist mechanism: there was only matter and its states of motion in the world. According to the laws of thermodynamics the world-machine contained a fixed amount of energy that was conserved in all physical transactions; no external source of power was necessary. The naturalist doctrine of psycho-physical parallelism held that mental states were the products of (or were coincident with) corporeal states; it was incorrect to regard mental states as the causes of action, as having power in themselves. Evolution by natural selection was mechanistic; no reference to purpose or design was required to understand organic change.

A natural world so constituted defined the nature of scientific inquiry just as it formed the basis for an autonomous scientific role. If nature was like that, then the old dependency relationship between the man of science and the man of the cloth could no longer be sustained. But freedom from clerical superintendence was only the first step in the attainment of professional status for the scientist. To achieve desired social support and command of resources the applicability of scientific procedures to a wide range of social questions had to be recognized and pressed. Hence scientific naturalists often adopted a more aggressive posture as cultural imperialists: man, society and mind all could be encompassed within a naturalist schema. The development of the social roles of the psychiatrist and the social planner were landmarks in the extension of scientific orientations to new cultural domains just as much as social Darwinism and eugenics [42, 43, 44, 48, 51, 53, 35, pp. 52–56]. Ultimately, the issue involved a clash of sources of expertise and credibility in society, and the command of resources which would follow from a recognized position of interpretive authority [55]. Thus studies of the naturalist cosmology rightly situate conflicting evaluations of
it in the contest over the professionalization and scope of science. It is also possible to press far beyond the cosmological level to show the strategic nature of scientists' positions on, for example, the fine anatomy of cerebrospinal ganglia, detailed embryological processes, and the nature of cause and power in physics [45, ch. 4; 46, 47].

Modern scientific representations of the natural world developed in the course of demarcation disputes with traditional sources of authority and intellectual expertise, such as religion. However, demarcation problems faced practitioners of science on many fronts. Securing credibility in society as interpreters of natural phenomena often involved making publicly visible the distinction between the cognitive claims of 'authentic' scientists and those of the general laity. Sometimes this took the form of avowals or disavowals of what sorts of objects existed in the natural world. A particularly instructive instance of this is contained in Ron Westrum's study of attitudes towards meteorites in eighteenth-century France [58]. While scientists of the Royal Academy admitted the existence of meteors (or glowing and rapidly moving celestial objects), they did not credit claims that earthly objects fell from the sky or that such alleged "thunderstones" were connected to the appearance of meteors. According to the official scientific establishment, meteorites were not natural objects; they were not facts of nature. In the eighteenth-century French social setting one of the problems with claims that meteorites had indeed fallen from the sky after a meteor display was that such reports tended to come from witnesses whose credibility as observers of nature the official scientific community was committed to denying. It is not the nature of meteorites to fall with great frequency within the precincts of established scientific institutions; more often they fall in rural districts where they are observed by peasants and attested to by local priests and assorted worthies. But it was the credit-worthiness of the laity that the French academic scientists were concerned to dispute. Interestingly, official recognition of the factual status of meteorites followed the Revolution and the changed attitudes that the Revolution encouraged towards the competence of the laity to participate in cultural pursuits. In other studies Westrum has pursued the relationship between social credibility and the reliability of reports with extremely compelling results. The approach he adopts to deal with the eighteenth-century French material does not need much modification in order to be applied to modern disputes over the existence of unidentified flying objects, sea-serpents and the like: reports of the existence of phenomena are often evaluated according to their social source [57, 59].

By the late nineteenth and early twentieth century the boundaries between
the professional scientific community and mere amateurs had been fairly well defined in most areas of science. However, there were specialties in which the amateur-professional demarcation was not yet as rigid as it had become elsewhere. One such case was observational astronomy. John Lankford has provided an excellent account of a controversy between various sectors of the astronomical community in the 1880s and 1890s [49]. This was a period in which amateurs (that is, persons not employed to do astronomical research) still routinely made significant contributions to stellar and especially to planetary astronomy. Of necessity, amateurs employed telescopes with relatively small apertures compared to the big telescopes used by professionals, particularly in the new American observatories. A controversy erupted when a leading British amateur astronomer asserted that small aperture instruments were actually superior to big telescopes for planetary observations. Specifically, it was claimed that they gave better definition even though they had less light-gathering power. Large refracting telescopes, it was said, suffered from the “glare” produced from gathering too much light. One consequence of this was that certain planetary details were more crisply seen with small telescopes than with the professionals’ large ones, for example the Great Red Spot on Jupiter, canals on Mars and certain spots on Saturn. American professionals, who used large refractors manufactured by American companies, eventually countered the British amateurs’ criticisms. As one American professional said, the sharp borders of planetary structures shown by small reflectors are not genuine features of those structures but are instead artifacts, produced by small instruments’ inability to resolve extended detail. Therefore the hazy borders revealed by large refractors are the natural appearance of the objects, not distortions due to defective optical properties. As Lankford concludes, the groups “represented opposing interests, and the scientific knowledge they produced rested on strikingly different perceptions of the natural world” [49, p. 27]. Here, then, is an interesting complement to Collins’ point relating evaluations of competency in experimentation to pre-existing views of the reality of the phenomena to be detected by the experiments. In Lankford’s material the conflicting groups had no obvious investments in the appearance of planetary spots; instead, they had pre-existing investments in the reliability of the instruments they employed. And, in this case, the type of instrument used served to distinguish two sectors of the astronomical community: professionals and amateurs. Thus, disputes over the appearance of the Great Red Spot were an episode in the professionalization of science. In an extended study of controversies in modern radio astronomy, Edge and Mulkay similarly stress the importance of investments in instrumentation [133].
In a brief and boldly conjectural article written almost thirty years ago Pannekoek weaves together an episode in planetary astronomy and aspects of the professionalizing strategy [50]. After William Herschel’s discovery of Uranus in 1781, it became widely noticed that the planet was deviating from its predicted orbit. By the 1830s it was thought that these deviations might be caused by yet another, hitherto unknown, planet lying outside the orbit of Uranus, and it was even suggested that one could find that planet from the pattern of Uranus’s perturbations. In the mid-1840s this was duly accomplished, by Adams in England and Leverrier in France. French scientists took the occasion to trumpet the discovery of Neptune to the public as unique proof of the predictive power and certainty of science: a demonstration of the value of science to the nation. This, and the relative lack of such propaganda surrounding the discovery in England, Pannekoek cites as an indication of the particular problems faced by French scientists in subduing the residual authority of the Church. But the real interest in Pannekoek’s account arises from his discussion of subsequent events. Shortly after Adam’s and Leverrier’s discovery, the American Walker calculated a precise orbit for Neptune which differed radically from that constructed by the Englishman and the Frenchman. Walker’s compatriot Peirce went so far as to say that this ‘Neptune’ was in fact a different planet from the planet whose orbit was calculated by Adams and Leverrier. So the relevant scientists were faced with a decision: were the ‘Neptunes’ the same or were they different? The Americans advanced the view that they were different; the French insisted that they were, after all, the same, pointing to wide limits of error involved in such calculations. Why? Pannekoek conjectures that the French sameness judgment was informed by an interest in saving their previous public display of the predictive power of science, while in America no such public investment had been made and scientists’ judgments proceeded on the basis of other criteria. If Pannekoek is right (and much further research would seem to be needed), one of the most fundamental acts of cognitive judgment (are natural objects the same or not the same?) was in this case structured by interests in the professional status and social standing of the scientific community.

IV. SCIENTIFIC KNOWLEDGE AND THE WIDER SOCIETY

Professionalization radically changed the ways in which concerns within the scientific community related to the concerns of the wider society. This historical shift has natural historiographic consequences. The historian of pre-professionalized science will frequently point to different sorts of social
factors from those implicated by the historian of professionalized science. However, to many historians of science it is puzzling to speak of a social history of modern scientific knowledge; to them the enhanced degree of autonomy enjoyed by professionalized science spelled an end to the explanatory role of 'social factors'. I have already discussed literature which seems to provide a solution to this puzzle: a solution which explodes an unsatisfactorily restrictive sensibility towards what 'social factors' are and how they function in explanation. (This is a topic to which I shall return later on.) Despite this, many scholars regard the social history of scientific knowledge as solely constituted of studies which relate scientific beliefs and practices to social and political concerns in the wider society. A few instances of this sort of work are very well known (or notorious) among historians and philosophers of science, and that is one reason why I shall give them less detailed attention than the studies already discussed. Nevertheless, there are features of these studies that might profitably be made more accessible.

There are several major reasons why the sociological significance of empirical studies of this type has been widely underestimated or insufficiently appreciated. One reason, undoubtedly, is the impatience of many theorists, including those sympathetic to the sociology of knowledge, when confronted with detailed empirical studies of any sort: 'historical' or modern. When this impatience is allied to a programme, such as that of Lakatos and his followers, that makes empirical history dependent upon philosophical judgments, there is added reason why sociological findings are unlikely to be credited.27 All the blame cannot, however, be put upon unsympathetic theorists. Historians of science, like most historians, tend to favour particularistic orientations. They generally define their work, not in terms of an interpretive tradition but in terms of a body of empirical materials or in terms of biographical foci. It is frequently counted as a criticism to say of a historian that he has an overriding theoretical commitment; such commitments are felt to get in the way of a properly disinterested engagement with the facts. Or it is felt that a single well-defined interpretive approach to a body of materials causes the loss of the richly textured narrative much valued by historians. Also, many matters of theoretical interest call for comparative perspectives: competence and willingness to master disparate subject matters or materials from different cultural settings. But the specialism and particularism of modern historical training makes the acquisition of these skills difficult and the insecurity of modern academic life tends to make their deployment risky. Immersion in particular empirical materials has obvious advantages (as anyone familiar with cavalier genres of sociological and philosophical theorizing can
attest); but there has also been a price to pay. That price has been a 'poverty of theory' among many historians of science. It is sometimes difficult to discern what the 'argument' or 'point' of certain empirical studies may be; in others the stated conclusions bear slight relation to the empirical body of the work; and in general there is little effort at connecting a particular study to its interpretive kin in other empirical domains. Finally, there is a marked lack of rigour in much social history of science; work is often thought to be completed when it can be concluded that 'science is not autonomous', or that 'science is an integral part of culture', or even that there are interesting parallels or homologies between scientific thought and social structures. But these are not conclusions; they are starting points for more searching analyses of scientific knowledge as a social product. All this may reduce to saying that history of science is a largely empirical discipline, and that there are certain problems attendant upon empiricist orientations. Empirical studies relating wider social factors to scientific knowledge can make important contributions to the development of the sociology of knowledge generally. If they are viewed collectively, as they rarely are, they display interesting and valuable similarities in their largely implicit sociological orientations. And, if those orientations can be made somewhat more visible than they usually are, further similarities with the work already discussed should also become apparent.

a. The Use of Cultural Resources

One of the most straightforward approaches to the connections between scientific knowledge and the wider society is found in studies that show scientists taking up intellectual resources associated with other forms of culture. Before briefly discussing some work in this vein a cautionary note is in order. There are two major techniques for addressing the boundaries between 'science' and 'other forms of culture'. One, associated with some philosophers of science and quite a few 'internalist' historians, involves a prejudgment of what counts as science and what does not. Usually that prejudgment is informed by modern scientific conditions and is implicated in a series of evaluations about what properly ought to belong within science and what ought to be excluded. Whatever apologetic functions may be performed by such an exercise, strictly speaking it is historical nonsense. The other approach involves trying to ascertain how historical actors themselves defined what belonged to science (or 'natural philosophy', or whatever term and cultural domain was indicated) and what did not. This definitely is a
historically significant project, for historical actors may well treat cultural items differently depending upon what side of their boundary they happen to place them. Thus the matter of so-called 'external influences' upon science is interesting insofar as the boundary in question is the actors' boundary and not one imposed willy-nilly upon the past.\textsuperscript{30}

There is a rich variety of historical studies convincingly demonstrating that the cultural relations of science in the past were considerably different from what they are at present. For example, the seminal work of scholars such as Alexandre Koyrè, Frances Yates and Walter Pagel, along with P. M. Rattansi, J. E. McGuire, E. M. Klaaren and others, has shown the close links between religious and general philosophical currents and developments within natural philosophy. 'Magical', 'neo-Platonic', 'hermetic' and theistic forms of culture, which now would be considered illegitimate if introduced within scientific culture, were important components of the scientific culture of the sixteenth and seventeenth centuries.\textsuperscript{29} Interesting as this sort of work is, it is not directly relevant to central questions in the sociology of knowledge. How may we move from recognizing the disparate cultural connections of science to understanding the relations between scientific knowledge and aspects of social structure?

In the making of scientific knowledge any perceived pattern or organized system in nature, in culture, or in society may be pressed into service. These patterns serve as resources for understanding the natural phenomena in question. Some of these usages are well known to historians of science and their demonstration has not been regarded as particularly contentious. For example, there are several studies of William Harvey's use of contemporary mechanical pump technology in conceptualizing the workings of the heart [61, 78].\textsuperscript{30} And there is an especially well worked-out instance of the scientific use of technological patterns in Sadi Carnot's idealization of a heat engine in the construction of his thermodynamic theory [65, ch. 7; 70]. There is little disagreement among historians that technology is not part of science proper, and, therefore, that these count as examples of the scientific use of extra-scientific resources.\textsuperscript{31}

Historians treat instances of the scientific use of resources from technological culture in a relatively matter-of-fact fashion. Structurally, the same perspective may be adopted in dealing with resources deriving from social thought or social experience. For example, Martin Rudwick has written about the geologist Poulett Scrope's use of concepts from political economy in the understanding of geological time [74], and also about Charles Lyell's use of resources from human history, demography and political economy
[75]. However, this matter-of-fact approach comes under threat when the alleged use of social resources concerns a particularly revered scientific production and especially when the social resources are regarded from present perspectives as ideologically suspect. In such cases two historiographic tendencies are in evidence: the first is to treat the use of such resources as an exposure or aspersion on the work in question; the second is to construe the matter in terms of the individual scientist’s motivation or state of mind in using these resources. Nowhere are these tendencies more evident than in studies of Darwin’s use of the patterns made available by Malthus’s social thought [52]. To many writers an ‘influence’ from Malthus (or from Paley) has not been something to describe and explain, but something to be ‘explained away’, since, from present perspectives, it would be regarded as an illegitimate inclusion in properly objective scientific thought. Of course, such an individualistic and implicitly evaluative approach is not the only possibility and R. M. Young has shown the way in a series of exemplary papers. He has demonstrated that ideas associated with the early nineteenth-century Malthusian debates over the correct distribution of wealth and power in society were also taken up by writers concerned with the scientific understanding of the distribution and succession of organic forms [79, cf. 62, 71]. Rather than identifying this as an instance of ‘extra-scientific influences’ [69] upon the culture of science, Young points to the existence in that setting of a “common context” in which cultural items routinely deployed in moral and political argumentation were also routinely brought to bear upon problems in the natural sciences [81]. The actors themselves did not regard such usages as illegitimate, although the evolutionary debates later came to involve considerable controversy about what counted as proper scientific discourse and what did not [77].

Valuable as these studies are, for present purposes they do little more than serve as an added reminder that historical actors’ conceptions of what counts as ‘internal’ to scientific culture is likely to vary from one setting to another; there is no reason to expect that present demarcations (and the evaluations they may express) will adequately describe any past context. Nor are patterns of resource-using in science, even when the resources happen to come from social thought, necessarily linked to actors’ motivations, particularly to alleged ideological intentions [80, pp. 386–87]. Neither Scrope’s uptake of banking metaphors nor Darwin’s use of Malthusian conceptions reveal anything in particular about the social purposes of the actors concerned. On the other hand, the availability and comprehensibility of given cultural items will vary for groups differently situated in the social structure.
and at different times and places. We may echo the judgment of Charles
Gillispie who observes that Darwin's use of individualistic and agonistic
models makes it "inconceivable that [the Origin of Species] could have been
written by any Frenchman or German or by an Englishman of any other
generation". 33

Young's and Rudwick's work therefore shows that scientists may draw
upon the materials provided by social thought; but their studies do not
reveal that there were any other reasons why Scope, Lyell and Darwin
should have deployed these resources than that they were familiar with them
and regarded them as valuable aids in doing scientific work. There was, that
is, no evident purpose in or relating to the wider society that informed
Darwin's or Lyell's use of resources deriving from social thought. There is,
however, a particularly well known study which relates the scientific use of
such materials to an important purpose in the wider society. Paul Forman
has written about the circumstances in which the physical and mathematical
communities in Weimar Germany came to adopt acausal modes of scientific
explanation [66, also 67]. It is difficult to summarize the arguments of this
detailed and complex paper, yet the basic contention is that Weimar physical
and mathematical scientists adopted attitudes towards determinism which
were prevalent in the wider society arising from Germany's defeat in World
War I. The development of quantum mechanics by Heisenberg, Schrödinger
and others was in part a consequence of the scientific community's accom-
modation to powerful currents in the general social and cultural milieu.
After the war it became fashionable to attribute the debacle to scientific
materialism and determinism, and enormous pressures were brought to bear
on scientists to dissociate themselves from these tendencies. One of the
most important vehicles for this general rejection of determinism was the
social philosopher Oswald Spengler's Decline of the West, a work which the
historical actors showed no sign of regarding as 'scientific' (even though it
did contain the outlines of a sociology of scientific and mathematical knowl-
edge). Yet, as Forman argues, it was the importation into physics and mathe-
ematics of attitudes to causality expressed in Spengler's writings and pervasive
in the wider society which provided one of the conditions for the production
of the quantum mechanical revolution. To adopt, and be seen to adopt,
these attitudes was to align Weimar science and its practitioners with increas-
ingly powerful social forces in the milieu, thus defending it from the very
real possibility of damaging attacks. 36 Similar processes of accommodation,
and their consequences for scientific theorizing, have been made visible in
quite different settings. For example, Brown has shown that the English
medical community's favourable response to mechanical models of bodily function and disease in the late seventeenth century was informed by an appreciation of the high prestige attached to Newtonianism in that setting, a prestige partly deriving from its development as moral and social philosophy by powerful elements in society. Prestige was a practical consideration in physicians' political disputes with other castes of medical practitioners over professional rights and privileges, and, as it happened, mechanism was abandoned when those particular political considerations were dissipated [63, 64, and for studies displaying similar processes 68, 72, 73].

b. The Social Use of Nature in the Wider Society

The work discussed in the preceding section deals with the deployment in the natural sciences of models, theories and attitudes current in social and political thought. Let us now turn to a body of historical writing that treats the deployment in society of conceptions of nature.

As we move from the professionalized and highly differentiated science of the present towards the natural knowledge of the seventeenth century we tend to move from a secularized natural order to one which was charged with moral, social and political significance [107, pp. 59–64]. Nature, that is to say, once had a constitutively normative dimension. The normative character of nature was generally thought to derive from the action of a deity who had created both natural and the social orders. This deity might use the normal or abnormal functioning of nature to signal to mankind His overall will, His pleasure or displeasure at particular events. Debauchery, regicide, or insubordination might be punished by plague; good weather might bless conformity. The natural order was, therefore, a pool of moral significances which might be drawn upon as needed to comment upon specific political events or the proper order of society. In the seventeenth and eighteenth centuries such usages were pervasive, and the scientific culture of those periods can hardly be understood without considering the institutionalized moral and social uses to which representations of the natural order were put. To speak of such practices using modern vocabulary, as the 'social use of science', runs the risk of misleading. Seventeenth- and eighteenth-century moral uses of nature were not the 'scientistic' extrapolation of esoteric natural scientific findings onto social problems; the moral and social uses of nature were essential considerations in the evaluations historical actors made of various theories, models, metaphysics and statements of fact.

A critical overview of some of the most significant recent scholarship
dealing with social uses of seventeenth- and eighteenth-century natural philosophy is available elsewhere and its findings need only be briefly summarized here [104]. For example, J. R. Jacob's studies of Robert Boyle's natural philosophy elucidate the religious and political setting in which English corpuscular philosophy was produced and evaluated [86, 87, 88]. Jacob finds that historical actors in mid-to-late-seventeenth-century England regarded matter-theory as highly relevant to social and moral concerns. Thus the correct physical explanation of the behaviour of liquids in the classic Torricellian experiment and in the air-pump experiments at Oxford and Gresham College was treated by the actors as a matter of pressing moral significance. Boyle argued strenuously in favour of explanation in terms of the "spring of the air", or, as we would now say, the differential pressures of columns of air. In so doing he identified alternative explanations in terms of "nature abhorring a vacuum" as erroneous, and, interestingly, as morally pernicious and subversive of true Christian religion [86, pp. 114–15; also 104, pp. 99–103, 135–39]. Of what possible relevance to moral concerns was the physics of liquids in a partially evacuated glass tube?

The answer proceeds from the pervasive seventeenth-century use of representations of nature to comment upon the social and moral orders. These usages became particularly intense and problematic in the 1640s and 1650s when the dissolution of traditional monarchical and ecclesiastical control in England set loose a deadly contest over the nature of moral and political authority in the state. Of particular interest was the proliferation of extreme religious sects, many of which rejected the notion of priestly intermediaries between God and the individual and abominated a hierarchical order of society. The Digger Gerrard Winstanley, for instance, developed a vigorous and coherent political programme which threatened to make away with established Church, universities, legal and medical corporations, and the private ownership of property. His argument was founded upon a vision of God's relationship to the universe in which divinity was immanent in material nature just as it was immanent within each believer. Divine power was thus accessible to all; revelation was democratized and the hierarchical order which made nature dependent upon an external spiritual deity, the believer dependent upon an external spiritual intermediary, and civil society dependent upon supervision by a divine-right monarch was collapsed and rejected. To the latitudinarian group for which Boyle spoke the radical sectarian threat had to be opposed, and one way of opposing it was to produce and disseminate a philosophy of nature and God which insisted that material entities were "brute and stupid", that God was not immanent in
nature, and that, therefore, nature, like a congregation and civil society generally, required for its activity the superintendence of external ordering and animating agencies. The notion that "nature abhorred a vacuum" was morally pernicious because it implied a hylozoism in which activity was essential to matter. It was subversive of institutionalized religion because it threatened the concept of an immortal soul: if there were no immortal soul which survived apart from the body, there could be no eschatological sanctions upon human behaviour [87, also 85]. And if there were no independent volitional soul apart from man's corporeal nature, then determinism, the ultimate liberation from man's responsibility to external moral authority, might be supported.

The overarching task of political and religious writers during the Interregnum was the reconstitution on a sure foundation of the basis of obligation in the state. After the Restoration the work of guaranteeing and securing that basis continued. The natural philosophy of Boyle and the early Royal Society was generated with a view to these social and moral uses; it was evaluated partly on the basis of how well it could be used in those contexts. The moderate and rational spiritual order and the limited monarchical order of the Restoration was soon under threat again. With the Exclusion Crisis, the Glorious Revolution, and, later, the intense uncertainty surrounding the Protestant Succession, the basis of moral and political authority in the state continued to be problematic from the 1680s until the 1710s. And in the political and theological debates inflamed by this long-lasting crisis of authority the social use of the natural order continued to be pervasive and important [89]. Margaret Jacob has studied the moral and social uses of the Newtonian philosophy of nature disseminated from the 1690s by the Boyle Lecturers [90]. Again, it is found that an insistence upon the inert character of matter and its dependence upon external animating causes had important apologetic functions. In particular such representations of the natural order served to secure the moral authority of a Church which was coming under increasing attack from libertine, Hobbitist, and freethinking deist circles in the late seventeenth and early eighteenth century. In a more finely-textured account Simon Schaffer has pinned down the particular political and ecclesiastical factions to which many Newtonians gave their allegiance and has shown the social and moral uses which informed the production and evaluation of detailed aspects of contemporary chemistry, physics and astronomy [100]. As Low Church Court Whigs the Newtonians sought simultaneously to celebrate the prerogatives of monarchical power and to show its proper natural limitations. This they did in part by displaying a natural order whose
phenomena showed the clear marks of God's supreme and unrestricted will while also manifesting God's perfect wisdom in framing natural laws in accordance with which the cosmos mainly functioned. Depending upon which source of the perceived threat the Newtonians were addressing they might elect to stress either God's will or His wisdom in His relations with the natural world and the 'world politick'. In the extended controversies with Leibniz Newtonian writers like Samuel Clarke laid especially heavy emphasis upon the sovereign power of God's will in nature, explicitly linking this to God's special providence in society and the supremacy of the monarch's will. In this context of use their preference for a voluntarist philosophy of nature proceeded from a perception that intellectualist philosophies linked Leibniz, as the Hanoverian court philosopher of the future royal house, to deistical factions in England which had been arguing vigorously against both the power of king and court and the rights of the Established Church [100, ch. 7; 105, 106]. Elsewhere, Schaffer has argued that the *dramatic* character of natural philosophy in this period is one of the keys to its moral uses [99]. For example, the public display of violent electrical phenomena produced by the Leyden jar served to make visible to every person the power latent in nature and available to God [101]. Much of Schaffer's valuable work on seventeenth- and eighteenth-century natural philosophy is concerned with the question of *access*: how could God's power be *made manifest* to everyone? Unless God's providence and potency could be made visible to all it was widely felt that the foundations of moral order were unsure. Thus it was part of the eighteenth-century natural philosopher's 'job description' to make God's power manifest.

In a setting in which representations of nature are used and evaluated as tools to further wider social interests a network of calculations is likely to be established: contingent associations between particular views of nature and specific constellations of social interest will be recognized and will then provide a basis of calculation and evaluation by other interest groups. In order to oppose the social interests of a group it may seem advisable to discredit and combat the view of nature which that group uses as a social strategy. Such a complex network of calculations involving wider social interests and the use of natural philosophy is evident in eighteenth-century Britain. Perceptions that Newtonian natural philosophy was the apologetic resource of a particular party in ecclesiastical and temporal affairs provided the basis for a series of attacks on the adequacy of that philosophy emanating from groups whose social interests conflicted with those of Low Church Court Whig Newtonians. Thus Christopher Wilde has described the Hutchinsonian
natural philosophy which many High Church clerics adhered to as a vehicle for combatting the authority and ecclesiastical dominance of the Newtonians [110, also 109]. Margaret Jacob has examined the anti-Newtonianism of the “Commonwealthmen” for whom John Toland spoke [90, ch. 6, 91, 92]; and John McEvoy has studied the anti-Newtonian rational dissent expressed by the Unitarian natural philosopher Joseph Priestley [93, 94, 95]. All these groups represented social interests which were in conflict with the Newtonian hegemony and all produced natural philosophies which sought to erode key aspects of the Newtonian world-view. And by these processes of opposition the natural orders constructed by Hutchinsonians, by the radical Commonwealthmen, and by ‘rational dissenters’ came to share a stress upon the self-sufficiency of nature: but in the cause of different interests. Hutchinsonians advocated a mechanically self-sufficient universe because it was beneath the dignity of a High Church deity to intermeddle with the material and the mundane; Commonwealthmen rejected voluntarism for Old Whig political reasons (such a God has been the legitimating resource of the Court faction); and Priestley did so in recognition that this God had been used to stifle rational dissent. Thus, shared cosmological representations did not proceed from shared ‘social backgrounds’ or even from shared social interests, but from interests in attacking positions associated with a common opposition.

The significance of the social use of nature becomes even more visible when human nature is at issue. How plastic or rigid is man’s constitution? Is there a portion of human nature (how large?) about which one can do nothing, and is there a portion over which one has volitional control or which is subject to modification by environmental forces? These disputes, of course, have a long history and need not take a specifically ‘biological’ form; thus the general shape of subsequent conflict over the moral significance of human necessity and liberty is prefigured in the celebrated Hobbes-Bramhall debates of the seventeenth century and in Priestley’s disagreements with the Scottish philosophers of Common Sense: neither of those episodes involved anything like a ‘genetic’ theory of human limits. However, by the early nineteenth century a biological theory which seemed to set boundaries on the sphere of human accountability did appear in the form of phrenology. Originally developed in Vienna and Napoleonic Paris as a reaction against Enlightenment meliorism, phrenology in Britain and the United States was forged into an extremely important naturalistic movement — the ‘forerunner’, if one wants to use the notion, of late Victorian scientific naturalism. The career of phrenology in Edinburgh has been the subject of some quite explicitly sociological study [102, 103, 121]. In these analyses phrenology appears as the strategy of
disaffected mercantile groups in early-to-mid-nineteenth-century Edinburgh society. Part of their strategy consisted in opposing the academic intellectual elites who, in their view, monopolized access to the universities and mystified proper mental philosophy through their ‘method’ of introspection. To the Enlightenment environmentalism of the academic philosophers, Edinburgh phrenologists symbolically juxtaposed the ‘hereditarianism’ of the phrenological system of human nature. But the unmodified assertion that human character was laid down by nature could scarcely further the wider interests of a group which felt itself badly served by the current distribution of rights and resources in British society. Thus phrenology in Britain had another face. Phrenologists claimed that a reliable, observation-based (and therefore ‘scientific’) system of character-diagnosis was a prerequisite to shifting human nature in a desired direction; for the size of the thirty-five cerebral organs subsuming each distinct mental faculty indicated the traits an individual would come to display other things being equal. Things could be made ‘unequal’ by a whole array of interventionist environmental techniques: education, public health, even, over generations, what later came to be called ‘eugenic’ marriages. British (and American) phrenology thus developed into one of the most important naturalistic resources deployed by bourgeois social reformers. Later on we shall see how preferences for or against such changes in the wider society featured in detailed judgments of anatomical fact in the context of cerebral anatomical research.

It was not only phrenology which displayed an interestingly plastic and dynamic view of human heredity in the first part of the nineteenth century. Theories of human heredity shared by medical practitioners and the laity held that heredity was a process, extending from the moment of conception through gestation and even weaning. It was also believed that what was inherited was not a trait but a tendency, say, a tendency to develop certain chronic diseases. What resulted was a transactional theory of disease in which practitioners could point to nature as the reason why certain of their interventions failed, while also extending their sphere of influence over an individual’s patterns of behaviour [97, 98]. Since, in this conception, ‘acquired characteristics’ could be inherited, what one did with one’s own life had a bearing on the constitution of future generations, and was therefore a legitimate area for the concern of society and its medical experts. For example, this dynamic theory of human heredity figured in arguments both for and against female emancipation [108]. Moreover, like the phrenological system, the dynamic view of heredity provided the naturalistic foundations for melioristic social reforms: within natural limits, human nature could be changed for the
better. However, by the end of the century, as Rosenberg shows, both medical men and bourgeois social thinkers came to prefer a far harder and more rigid view of human heredity: nature became more unforgiving as social reforming postures vis-à-vis the working classes began to seem a less attractive strategy. Hard hereditarianism now manifested itself in the guise of eugenics.

As MacKenzie has shown, eugenics is appropriately viewed as the strategy of the British professional middle classes [35, chs. 2, 4]. By assuming that the social order was, with some discrepancies, a natural order founded on the biological endowments of individuals, eugenists like Francis Galton found a naturalistic justification for the social claims of ‘brain workers’ over the hereditary aristocracy, plutocrats and manual labourers. Perhaps more importantly, eugenics fitted into the overall strategy of scientific naturalism, offering both a theory of society and a programme of practical action which made maximum use of the skills and competences of secular intellectuals like scientific medical men and allied professionals. Eugenic views and eugenic programmes, like the other conceptions of human nature we have briefly discussed, are best seen as the strategy of specified social groups using conceptions of human nature as a persuasive resource. The usefulness of this mode of analysis to an understanding of the contemporary debates over racial differences in IQ has been demonstrated in several perceptive papers by Harwood [82, 83, 84]. And Provine has made a related point in his examination of changing evaluations by geneticists of the effects of race-crossing [96]. Both in the case of human nature, and in examples from natural philosophy in the seventeenth and eighteenth centuries, the use of representations of nature in a wider social context formed a basis for scientific judgments.

V. FULL CIRCLE: CONTINGENCY AND WIDER SOCIAL INTERESTS

For purely conventional reasons this paper has so far considered the role of a variety of social interests as if they were distinct and manifested themselves in separate bodies of knowledge. The time has come to correct any such impression and to try to put together a number of historiographic orientations which are often seen as incompatible. One traditional source of difficulty in sustaining a sociological approach to scientific knowledge comes from the view that the power and validity attributed to science is guaranteed by its freedom from ‘social influences’. In this account social considerations can only work to corrupt proper science; the scholar convinced of the value of science and concerned to defend it from attack must therefore take great care before showing the presence of social interests in scientific activity.
Writers in this tradition tend to read sociological accounts of scientific knowledge as aspersions, however great the pains taken by sociological writers to state otherwise. By now this particular battle has been fought so many times that it is pointless to do more than reiterate: sociological accounts have no bearing upon whatever evaluations one may wish to put upon science; indeed, the major reason why such accounts are frequently self-described as 'naturalistic' is simply that they have no evaluative axe to grind.

A related source of misunderstanding seems to stem from within certain strands of sociological thinking, namely a tendency to regard bodies of knowledge as the manifestations of single types of social interest. Knowledge used, for example, to legitimate structures in the wider society is considered to be different in kind from knowledge used for the 'prediction and control' of phenomena. Since these interests are thought to be incompatible, so must the types of culture they produce. In the history of science such an impression may be reinforced by the purely conventional fact that empirical studies of particular bits of science tend to fall into distinct genres: there are studies which treat Newtonian natural philosophy as informed by technical interests in prediction and control and which situate it in a cultural tradition, and there are studies which assess the same body of culture as a legitimating resource deployed in the wider society. The historian my feel he is being asked implicitly to choose between incompatible approaches. Of course, 'choice' is not necessary, and there are already several empirical studies which explicitly make this point [104, pp. 124--31].

In a particularly concise example Lawrence has studied conceptions of the human nervous system and its functioning in eighteenth-century Scotland [117]. The major empirical concern of his paper is to show that theories of nervous 'sensibility' and 'sympathy' in that setting were evaluated according to their use in justifying the cultural and social leadership of Lowland intelligentsia and their allies. But Lawrence goes on to argue that these conceptions functioned in both 'scientific' and apologetic contexts; they were, as he says, "multifunctional", and, while he does not himself show their detailed usage in medical and physiological settings, he points to a body of historical work which does display such a role. Similarly, Wynne's account of aetherial and energetic conceptions of matter in late Victorian Cambridge argues the importance of their use as an anti-naturalist, anti-professionalizing strategy, strongly linked to psychical research, without in any way claiming that such ideas did not also inform much technical work in the study of radiation [122]. But to make the point about multifunctionality [103] totally convincing, and to advance our understanding of interests and scientific knowledge
generally, it is best to turn to empirical studies which themselves document the role of a variety of interests in the development of knowledge.

Let us return to the cluster of sub-cultures which encompassed the study of evolutionary mechanisms (biometry and Mendelism), the biological understanding and manipulation of social structure (eugenics), and the techniques thought requisite to eugenic theory and practical programmes. In briefly considering some empirical literature dealing with biometry and Mendelism I pointed to the possible explanatory role of the distribution of scientific competences and skills (see Section II above). However, some of the writers on these episodes explicitly state that such considerations are insufficient to explain the controversies; Pearson apparently possessed the competences to have embraced Mendelism if he so chose; Bateson’s attachment to Mendelism cannot satisfactorily be explained by his particular experimental skills; and proposals to reconcile the two orientations were systematically rejected or ignored for quite some time. MacKenzie and Barnes [36, 37, also 35, ch. 6] thus turn to factors operating in the general cultural, social and political milieu of late nineteenth- and early twentieth-century Britain, that is to factors generally identified as ‘external’ to the natural scientific culture of that setting. If one proceeds in a traditional historical manner and concentrates on key individual actors, one can discover interesting differences in their social and political views. Karl Pearson, the major British biometrician, came from a dissenting middle-class background [119; also 35, ch. 4], and had pronounced anti-clerical, anti-laissez-faire, social imperialist views close to those of many Fabians. His belief in biological gradualism was paralleled by his strong commitment to progressive and gradualist social change; indeed, he believed in the application of the results of scientific investigation to social problems. The display of progressive continuity in nature underwrote a commitment to progressive and continuous social change. A commitment to continuity thus ran through Pearson’s evolutionary views and his highly-developed social philosophy. By contrast, his opponent William Bateson was connected to traditional academic elites and was deeply mistrustful of the effects of industrialization, and the ideologies of utilitarianism and evolutionary social progress. As Coleman has shown [124], Bateson was, in Mannheim’s sense, an essentially conservative thinker. He thought that both science and society ought to ‘treasure exceptions’; just as evolution depended, in Bateson’s view, upon the exceptional discontinuity, so social progress depended upon the uncontrollable appearance of rare individual genius.

So preferences for biometrical versus Mendelian explanation appear
to proceed from divergent social orientations; preferences for continuity
theories versus discontinuity theories in the natural sciences were structured
in part by conflicting interests in the wider society. These divergences also
manifested themselves in attitudes towards potential courses of practical
social action, particularly towards eugenics. Pearson, like many biometricians,
was a committed eugenist, while Bateson, like other opponents of biometry,
was deeply suspicious of eugenics. This is an association which becomes
more understandable when one recognizes the extent to which biometry was
developed with a view of coping with the problems posed by a eugenic view
of society and by practical eugenic programmes of action [35, chs. 5–6, 120].
Insofar as eugenics was the strategy of a particular interest group in British
society, the biometry-Mendelism controversy was sustained by conflicting
interests in the distribution of rewards, rights and privileges in the wider
society. Of course, recognizing these features of the controversy supplements
rather than diminishes the significance of professional competences and skills.
A range of social interests, including those usually considered ‘internal’ and
‘external’ to the scientific culture, need to be considered in order satisfactorily
to explain this particular episode.

This methodological point becomes especially important when one con-
siders some of the mathematical tools developed within this cluster of sub-
cultures. For example, Ruth Cowan showed some years ago that Francis
Galton’s statistical theory was informed by his eugenic commitment [113,
also 112, 114, 115]. She took the view that the significance of eugenics was
that it provided the motivation which turned Galton towards particular
statistical questions the content of which was, presumably, not dependent
upon Galton’s eugenic purposes. Recently, however, MacKenzie has pressed
the point that “the needs of eugenics in large part determined the content
of Galton’s statistical theory” and has produced detailed demonstrations
of this in his account of the differences between Galton’s work and that of
the error theorists, and in his discussions of Galton’s work on regression,
correlation, and the bivariate normal distribution [35, ch. 3]. However, given
our present concern with the range of social interests involved in sustaining
scientific knowledge, it is MacKenzie’s study of statistical controversy be-
tween Pearson and G. U. Yule which provides better illustrative material
[35, ch. 7, 111, 118]. This was a highly esoteric controversy within early
twentieth-century British statistics dealing with the correct way to measure
the association of data arranged in contingency tables. The controversy
overlapped with the most intense phase of the biometry-Mendelism conflict
and involved some of the same major actors. By 1900 there was general
agreement on how to measure the correlation of normally-distributed interval variables, but it was uncertain how best to deal with nominal variables, i.e. those for which no unit of measurement existed such as 'alive' or 'dead', 'vaccinated' or 'unvaccinated'. From 1900 Pearson's approach was to treat nominal variables in the contingency tables as if they were produced by an underlying bivariate normal distribution. Pearson was aware that this was often an untestable assumption, but he nevertheless regarded his measure of correlation as the correct one; others were indeed possible, but these were treated as approximations to the tetrachoric coefficient. By contrast, Yule did not make the assumption of underlying normal distribution, and by 1905 he openly attacked Pearson's work, especially the assumptions underlying the tetrachoric. The controversy continued for a decide, involving a good part of the small British statistical community.

MacKenzie stresses that Pearson (and his followers) and Yule (and his followers) had different goals in statistical theory: the former wished to maximize the analogy between the treatment of interval variables and nominal variables, while the latter wanted to treat nominal data sui generis. These differing goals MacKenzie terms divergent "cognitive interests". The two positions were incommensurable by virtue of Pearson's and Yule's differing goals in statistical theory [111]. But MacKenzie goes on to try to explain why it was that differing cognitive interests were so distributed. MacKenzie's analysis is too subtle to be briefly summarized; however, he connects the two sides' conflicting views on association with their divergent positions on eugenics. Pearson, MacKenzie shows, developed his statistical theory in the manner he did because of the requirements of the eugenic programme to which he was committed. Yule, on the other hand, had no commitment to eugenics and developed his statistical work in this area differently. And, as MacKenzie has already shown, commitment to eugenics is itself to be referred to wider social interests, such as those affecting the professional middle classes, on the one side, and traditional elite groups, on the other.

Thus esoteric work in mathematical statistics is explained by referring different views to diverging purposes within the statistical community, and also to diverging goals in the wider society. Historical work of this sort therefore illustrates two points of relevance here: firstly, it shows beyond any doubt that the explanation of even the most technical and esoteric scientific activities may need to be referred to wider social interests. In this respect it has long seemed that the history of mathematics is an unusually tough nut for the sociology of knowledge to crack, but cracks have indeed
begun to appear recently.\textsuperscript{40} Secondly, MacKenzie's work, and other studies to be discussed shortly, erodes any tendency to think of wider social interests as affecting, as it were, the 'outside' of scientific knowledge (models, metaphysics and metaphors) while the esoteric core is generated solely through disinterested interrogation of reality. Any such 'two-tier' model of the sociology of knowledge gets no support here. And finally, such work reinforces the point made by studies discussed above: that institutionalized bodies of scientific knowledge may typically be sustained by a variety of social interests, and these may cross-cut historians' conventional categories of 'internal' and 'external' considerations.

By connecting interests in the wider society to judgments of the adequacy and validity of esoteric mathematical formulations we have come close to completing the methodological circle. We started by considering historical studies which showed the contingency of scientific judgments about experimental findings and matters of fact, and we have now reached the point at which we can begin to see that such judgments may well be structured by wider social interests. Let us amplify and refine this point by briefly examining two final historical studies: one dealing with the notion of a 'competent experiment' and another concerning observation reports. Farley and Geison have studied nineteenth-century French controversies over the spontaneous generation of life [116]. In the late 1850s the Rouen naturalist Felix Pouchet pronounced himself convinced of the reality of spontaneous generation and published what he took to be experimental proof of the phenomenon: microorganisms appeared in boiled hay infusions under mercury after they had been exposed to artificially generated air or oxygen. These experiments elicited immediate critical comment from Louis Pasteur: he suggested to Pouchet that his experiments had been improperly performed; contaminated air had almost certainly been introduced and this error in procedure, rather than spontaneous generation, was responsible for the appearance of life in the flasks. Immediately thereafter, Pasteur undertook his own series of experiments. He took a set of flasks high up on a glacier in the French Alps, exposed them to the rarified (and presumably uncontaminated) air and showed that only one developed signs of life. This one Pasteur regarded as an anomalous result — the series definitively proving that competently performed experiments refuted spontaneous generation. Pouchet felt obliged to replicate Pasteur's experiments (although the replication was not exact), and he went to the Pyrenees with his flasks, where all showed signs of life when briefly exposed. This Pouchet took to be proof that competently performed experiments established the fact of spontaneous generation — all
that was needed to make life appear in an organic infusion was oxygen. Since Pasteur and Pouchet could not agree between themselves about the criteria for a ‘competently performed experiment’, Pouchet issued a challenge which resulted in the appointment of an adjudicating commission by the Paris Académie des Sciences. In the event, that commission was heavily stacked in favour of those already convinced of the impossibility of spontaneous generation; some of the members announced against Pouchet even before the experiments were examined, and so Pouchet withdrew, leaving the prize to Pasteur.

Farley and Geison thus describe a situation already familiar to us from the sociological work of writers like Collins and Pickering [e.g., 4, 6, 7, 16]. There are no transcendent criteria by which the competence of experimental procedure may be judged; prior commitment to the existence or non-existence of the phenomenon in question necessarily enters into a judgment about whether or not relevant experiments have been competently performed. But Farley and Geison do not seek merely to establish the contingency of judgments about experimental procedure; they seek to identify particular contingent social considerations which structured differing judgments. As it happens, those considerations relate scientific judgments to concerns in the wider political and moral setting of mid-to-late nineteenth-century France. The issue was materialism, and the consequences perceived to flow from materialism in that context. Belief in spontaneous generation seemed to imply the self-organizing capacities of matter and (echoing seventeenth-century hylozoist themes) therefore to threaten the existence and role of the external spiritual agencies upon which the authority of the Church rested. While Pouchet stipulated the religious orthodoxy of his particular version of spontaneous generation, Pasteur’s forces insisted upon identifying their opponent’s views as heterodox and dangerous to public morality. Farley and Geison conclude that “members of the French scientific community may have chosen Pasteur over Pouchet” (and therefore have judged the competence of their respective experiments) “at least in part for socio-political reasons” [116, p. 183].

In Farley’s and Geison’s example we see how wider social interests bore upon evaluations of experimental competence, and, therefore, upon the truth-status of experimental findings of fact. The micro-sociological focus of writers like Collins and Pickering is supplemented here by a macro-sociological analysis: the relevant social factors in this case happened to include both features internal to the sub-culture of French science and considerations linking science to religious, moral and, ultimately, political discourse. It is
a contingent association: one which there is no reason to expect operates in all controversies over experimental competence. One might, for example, be surprised to find views for or against the authority of religious institutions figuring in the gravity radiation controversies (but perhaps less startled to find that similar considerations might prove relevant to explaining the present-day disputes over species-change). The appropriate methodological strategy derives from the historical circumstances, not from the level of culture the historian seeks to explain. This is best demonstrated by bringing our methodological excursion full circle; let us consider an episode in which actors disagreed about visual observations.

The setting is one which we have already introduced: the disputes over the validity of phrenology in early nineteenth-century Edinburgh [102, 103]. We have seen that phrenology was the argumentative strategy of groups in that society which were concerned to erode the authority of existing academic and spiritual elites and to substitute for it a proto-naturalist, participatory science of man as natural object. The local controversies thus tended to array disaffected and inconoclastic bourgeois groups against traditional elites and their intellectual spokesmen; it is no exaggeration therefore to see that Edinburgh phrenology disputes in terms of the macro-sociological category of social class. Nevertheless, these controversies also involved a series of esoteric issues in cerebral and neuro-anatomy; there were disputes over the exact contours of the cranial bones, the patterns of the cerebral convolutions, the exact fibrous constitution of the hemispheres, and the fine structure of the cerebellum and the fibres connecting it with other parts of the brain and the spinal cord [121]. Participants in these disputes violently disagreed about what could be seen when one looked at these structures. There might be a temptation to separate the controversies into cosmological and methodological components, on the one hand, and esoteric, technical and ‘scientific’ matters, on the other. It might be thought that the former could be referred to macro-sociological considerations, while the latter must pertain solely to concerns within the sub-culture of anatomy. This proves, on inspection, not to be the case. Anti-phrenologists’ insistence that cranial bones in the region of the frontal sinuses were not parallel was explicitly connected to their claim that phrenological character diagnosis was impossible; phrenologists’ assertion that the cerebral convolutions might show standard pattern and morphological differentiation was explicitly related to their view that mental faculties were subserved by distinct cerebral areas. Similarly, disputes over the fibrous nature of the brain mass were closely associated with conflicting views of the differentiation of cerebral organs and
of mental function. As the anatomical disputes, so to speak, pushed deeper into the brain and into esoteric anatomical issues, the participants themselves seemed less able convincingly to assign social interests to their opponents’ claims. Indeed, it was the public appearance of disinterestedness upon which rested, in that setting as in many others, the credibility of the claims advanced. Nevertheless, close observation of the disputed phenomena did not lead to a convergence of claims, for observation was, apparently, a thoroughly political process and the political ends of the parties involved differed. In this case, as in the micro-sociological studies discussed in the first section, natural reality did not possess the coercive force with which actors’ discourse often imbued it. Reality seems capable of sustaining more than one account given of it, depending upon the goals of those who engage with it; and in this instance at least those goals included considerations in the wider society, such as the redistribution of rights and resources among social classes.\textsuperscript{92}

CONCLUSIONS

If the empirical achievements of the sociology of scientific knowledge are really as impressive as made out here, why have they received so little recognition? And why does the empirical sterility of the sociology of knowledge continue to be cited as a major reason for rejecting the approach? I have already suggested that part of the explanation must reside in historians’ interpretive reticence; it is sometimes possible to miss the sociological thrust of certain exercises, and it is not unknown for authors themselves to put a radically anti-sociological label on work which can strongly support a different conclusion. Nor is it beyond the bounds of possibility that the current distribution of reward in the academic history of science affects what some writers say about their work more strongly than it affects what they do. Too much importance must not, however, be laid upon these considerations; at the end of the day it is practice that is important.

An apparently more significant problem arises from a largely informal model of the sociology of knowledge which seems to be prevalent among a number of philosophers and historians of science. For ease of reference this may be called ‘the coercive model’: its main characteristics can be briefly described: (i) it maintains that sociological explanation consists in claims of the sort: ‘all (or most) individuals in a specified social situation will believe in a specified intellectual position’; (ii) it treats the social as if one could derive it by aggregating individuals; (iii) it regards the connection between social situation and belief to be one of ‘determination’, although little is
explicitly said about the nature of determinism; (iv) it equates the social and
the 'irrational'; (v) it equates sociological explanation with the invocation
of 'external' macrosociological factors; (vi) it sets sociological explanation
against the contention that scientific knowledge is empirically grounded in
sensory input from natural reality.\textsuperscript{43} On these suppositions what would a
practical piece of sociological explanation look like? In the first place it
would be fundamentally prosopographical: one would search for statistical
correlations between the social circumstances of groups and their scientific
beliefs; one would worry about 'exceptions' and about the 'level of signifi-
cance' of the correlations; and individuals would generally be regarded as
troublesome, as they would frequently not 'fit' the causal connection being
tested. On the other hand, the connection between the social and the cogni-
tive would generally be sought through the use of individualistic orienta-
tions, particularly through the category of 'motivation'. Then one would contrast
actors' apparently volitional actions with the role of the social; those courses
of action that seemed to be purposive (or 'rational') would be excluded from
the sociological ambit and be treated as self-explanatory. One would look
exclusively to macrosociological categories for one's explanatory tools;
factors internal to the scientific community would be viewed as non-social.
And on this basis one might claim that there are interpretive and method-
ological asymmetries between sociology and history of science, or between
the study of modern professionalized science and that of past settings.
Finally, one would say as little as possible about the fact that scientists
conduct experiments, look down microscopes, go on field expeditions, and
the like, for wherever 'reality' enters in, there sociological explanation is
obliged to stop.

Of course, it should be apparent that the 'coercive model' has, from a
certain point of view, two splendid advantages. First, it is a model for the
sociology of knowledge that maximizes the chance that no successful instance
of its practice will ever be encountered. Second, it portrays the role of the
social and of sociological explanation in unpalatable normative light: as if
it were said that 'no rational person would ever allow himself to be socially
determined!' Nevertheless, there is one major problem confronting the
'coercive model': namely, that it is not an accurate picture of sociological
practice. One could establish this programmatically, or one could proceed
in a style more in keeping with the present exercise: by looking at what the
empirical literature actually does, and by trying to tease out of various
approaches represented there some common sociological sensibilities and
explanatory tactics. Naturally enough, there is a variety of sociological
perspectives available in these writings, and few historians do anything so vulgar as to advance an explicit model of explanation. However, one would not seriously misrepresent the interpretive thrust of much of this work by discerning in it what may be called an 'instrumental model' of sociological explanation. What are its main characteristics?

For many writers a sociological approach meshes with some routine practices in the history of ideas; for example, the state of knowledge at any given moment is matter-of-factly treated as the base point for cultural change. The cultural heritage is socially transmitted; no man invents his own language, and, just as the speaking of English in seventeenth-century England was socially transmitted, so the heritage of, say, natural philosophical concepts and practices was socially transmitted to individual men of science, whatever innovations they then wrought on this legacy. Thus variability in concepts and practices in different settings and among different groups is frequently referred to patterns in the social agencies that transmit knowledge: schools, universities, churches, and the like. Of course, the resources that are available to solve scientific problems may not be the monopoly of formal educational and cultural institutions. People may deploy the resources provided by the experiences of living and operating in society, although, as we have seen, it is an entirely contingent matter whether scientists do so or not. Much of this is generally regarded as uncontentious, although many historians of ideas still treat contributions to culture as if they were generated in vacuo by atomistic individuals, and some of them continue to view the source of cultural materials used in science as a matter of moral concern.

Some more problematic issues in the sociological analysis of knowledge arise from the treatment of meaning. In the traditional practice of the history of ideas there have been two dominant tactics for explicating the meaning of texts and propositions generally. The first has been enshrined in the close exegetical study of individual texts and has assumed that meaning may be unproblematically 'read off' from statements contained therein. This has amounted to the view that meaning is culturally universal and not contingent upon historical circumstances. In recent years this tactic has come in for some cogent criticism, most notably by Quentin Skinner who has argued that historians should carefully attend to the meaning that given texts had for the actors. However, for Skinner, 'meaning for the actors' is not an explanatory or descriptive terminus: it is a way station on the road to 'real' historical meaning which resides in the intentions of the individual author. Thus Hobbes's intentions, and therefore the real historical meaning of Leviathan, are to be recovered through a study of the collective historical phenomenon
of Hobbes. There is, however, an alternative approach to the meaning of texts which accepts and welcomes what may be called ‘critical Skinnerism’ while rejecting the location of meaning in individual authors’ states of mind. This would be a procedure seen at work in certain accounts of seventeenth- and eighteenth-century natural philosophy [e.g., 100, 104, 105, 109, 110]. In these studies the meaning of a natural philosophical proposition is discerned through its contexts of use in concrete historical settings rather than through the exegesis of the isolated text or through theorizing about the psychological state of the individual author. Newtonian natural philosophical propositions, for example, were not ‘essentially deistical’ and did not ‘logically imply’ deism (as Hutchinsonian and High Church critics said), nor were these same propositions ‘inherently inimical’ to proper deism (as writers like Toland said) [92, 109, 110]. Instead, their significations derived from the contexts in which they were deployed, the enemies against which they were directed in various settings, the alliances that were mobilized through their enunciation. Moreover, the historical actors themselves were ceaselessly engaged in stipulating the ‘real meaning’ of Newtonian propositions: working to ‘make out’ Newtonianism to be ‘inherently’, ‘logically’, and all along the thing it only became through the active production of meaning in different settings. Interestingly, actors’ stipulations about ‘real meaning’ often proceeded by way of statements about what ‘must have been’ Newton’s own intentions (some of which were proffered by Newton himself). But these stipulations show as much variability, and are as marked by signs of active work, as statements about the ‘inherent’ meaning or ‘logical implications’ of texts. Recognizing the active contextual nature of meaning-production does not, as might be thought, lead to historiographic chaos: it may instead lay the grounds for more historically sensitive empirical work. In this perspective meaning is a social accomplishment: it is something that actors achieve in the course of doing things with culture in concrete historical circumstances. Actors are not locked into meaning which is ‘inherent’ or ‘logically implied’ in texts, still less into the meaning intended by the authors of texts. That is to say, in the vocabulary of the ethnomethodologists, they are not “judgmental dopes”. Men make meaning.

Men also make knowledge. They do so against the background of their culture’s inherited knowledge, their collectively situated purposes, and the information they receive from natural reality. Perhaps the most puzzling charge sometimes laid against relativist sociology of knowledge is that it neglects the role played by sensory input. On the contrary, the empirical literature employing this perspective shows scientists making knowledge
'with their eyes wide open to the world'. If anything, writers such as Collins, Farley and Geison, Kohler and Pickering have been more intensely concerned with how scientists conduct experiments, focus upon reality, and come to terms with the sensory information channelled by experiment than many more 'traditional' historians and philosophers from whom such criticisms often come. Both in this empirical literature and in the theoretical sociology of knowledge corpus there is no question of denying the causal role of the unverbalized reality upon which given scientific beliefs focus. What is perhaps at issue here is whether a specific verbal formulation of reality is to be privileged in sociological and historical explanation. The historian may indeed have little choice but to 'lay a bet' on the physical reality which impinged on actors, and in 'laying that bet' he might well opt for a modern text-book account. However, he must remain on his guard against using that account as a sufficient explanation of beliefs that accord with it. If the historian succumbs to this temptation, he will indeed talk about 'natural reality' as a 'constraint' upon what is said about it. But whatever appeal this procedure may have to rationalist and realist writers, historians ought to be aware of what is involved: for it may be nothing less than the very Whiggism and 'presentism' that historians have so generally agreed to abominate. To reject privileging specific verbal formulations of reality is not to reject the role of sensory input: it is to write more sensitive history. Again, it is the opponent of relativist sociology of knowledge who would make the actor a 'judgmental dope'. In this case, it is 'reality' which is said to coerce the actor.

This leads us on to what may be the most central aspect of the largely implicit 'instrumental model': the generation and evaluation of knowledge is treated as goal-directed. Knowledge is not regarded in this literature as contemplatively produced by isolated individuals; it is produced and judged to further particular collectively sustained goals. Knowledge, in this perspective, is always tailored to doing things. It is in the course of doing things with knowledge that its meaning is produced; thus, the notions of use and meaning are intertwined. We have seen this instrumentalist perspective at work both in the study of past science and its wider social relations and in the explanation of scientific controversies in present-day science. The purposes for which knowledge is produced and according to which it is evaluated may vary very widely: they may include the legitimation or criticism of tendencies in the wider society, or they may encompass goals generated exclusively within the technical culture of science. And, as we have seen, there are many instances in which both sorts of instrumental goals bear upon the production and
evaluation of culture. Typically, usage and meaning will be embedded within a complex social network of calculations, such that possible connections always exist between considerations in all parts of the net.\textsuperscript{50} As MacKenzie and Barnes conclude from their interpretive study of the biometrician-Mendelian controversies, "The general point is not that the goal-directed character of scientific judgment implies its relationship to any particular contingency, or to external factors, or political interests; what is implied is that any such contingency may have a bearing on judgment and that contingent sociological factors of some kind must have" [37, p. 205].

Finally, this brief exposition of a working instrumentalist model in the history of science allows us to reflect back upon certain aspects of the 'coercive model' that have hindered appreciation of the actual role of the social. In that model, the social was routinely contrasted to the 'rational'. However, in the empirical literature we see no such contrast. Actors are all treated as if their 'cognitive wiring' was in proper working order: that is to say, they are all possessed of 'natural rationality'.\textsuperscript{51} That rationality is expressed in the instrumental character of their behaviour. Their calculations may, as a matter of fact, take into consideration goals pertaining to the wider society or they may not. Actors' judgments which are informed by wider social interests seem no less intelligible and competent than those which do not. Given that this is so, the only conceivable purpose to be served by equating the social with the 'irrational' is stipulating which sorts of considerations the ideal type of the modern scientist should take into account. Few historians will see this as an essential and proper part of their activity.

It is this patently normative attitude towards 'rationality' which appears to inform the 'coercive model's' view of determination and the social. We are invited to conceive of 'social determination' as if it were a sort of mugging. But which model is it really that makes out actors as "judgmental dopers"? In an instrumentalist perspective actors are seen to produce and evaluate knowledge against the background of socially transmitted knowledge and according to their goals. The role of the social, in this view, is to prestructure choice, not to preclude choice.

I have attempted to show here that the sociology of knowledge is more than a set of theoretical and programmatic reflections upon what might be done; it is also a body of practical achievements. While there is every reason for satisfaction about the state of the empirical literature, there is no justification for complacency. Empirical writings are attended with problems as well as advantages, and it is highly desirable that practically minded historians should become more aware of the interpretive purport of their achievements:
both for their own concerns with the handling of concrete materials and for the clarification of more general issues in the theory of knowledge. Given proper awareness of this work, there would also be no more talk of "the widely acknowledged failure of cognitive sociology to explain any interesting scientific episodes".  

This paper has not, admittedly, been an attempt to analyze Fleck's sociology of knowledge. However, what I have done is something quite close to the spirit of Fleck's own practice, that is, I have shown sociology-of-knowledge orientations at work in explaining particular scientific episodes. Even in doing that task, I have necessarily been obliged to select certain sociological themes at the expense of others. It must be especially obvious that I have not dealt with empirical work which bears upon 'styles of thought'. But I trust that I have, in some small way, made more visible and accessible work which reflects a major portion of Fleck's own practical concerns, for it is well to say again that Fleck wrote a book of empirical history.

NOTES


2 Especially Barry Barnes: Scientific Knowledge and Sociological Theory (London, 1974); idem, Interests and the Growth of Knowledge (London, 1977); David Bloor: Knowledge and Social Imagery (London, 1976); Michael Mulkay: Science and the Sociology of Knowledge (London, 1979); see also references to Barnes and Bloor in notes 6, 26, 48, 50, 51.

3 Neither will I treat the history of the social sciences, although I do not accept that these materials present an 'easier case' for the sociology of knowledge in anything but a persuasive sense. Perhaps the greatest losses resulting from my selective criteria are: (i) an excellent literature dealing with the cognitive foundations of research schools and disciplines; and (ii) some attempts to operationalize Mary Douglas's 'grid-group' schema. Some selected references are included in the Bibliography, Section VI(c) and (d).

4 I should make some formal statement concerning my own evaluation of the empirical work discussed. Obviously, I cannot and do not claim scholarly competence in all the relevant areas; therefore I cannot 'vouch for' the factual accuracy of much empirical work I treat. Nevertheless, I see no major problem in presenting empirical achievements as, so to speak, 'state of the art'. Very little of this work has been challenged in print, but, where such challenges do exist and may bear upon the adequacy of interpretive perspectives, I shall make every effort to point this out in Notes. I must also stress that summarizing empirical studies always results in loss of detail and therefore of
persuasive power. The brief sketches I provide should be regarded more as guides to reading empirical work than as substitutes for reading it.

5 A positivistic sociology of the dynamics of science and its foci of interest appeared in R. K. Merton: *Science, Technology and Society in Seventeenth-century England* (new ed., New York, 1970; orig. publ. in *Oziris* iv (1938), 360–632), and, while even this exercise is disallowed by some critics, it does not belong to the sociology of knowledge proper and will not occupy us here. For one of the few attempts to apply Merton’s collective biographical approach to scientific foci: Steven Shapin: ‘The Audience for Science in Eighteenth-Century Edinburgh’, *History of Science* xii (1974), 95–121.

6 It was Mannheim’s view that wherever a ‘historical’ account of knowledge could be given, there a ‘sociological’ perspective was also indicated. This is a useful sensibility, despite Mannheim’s well-known tendency to distinguish between those beliefs that were “situational” and those which were not, such as “2 + 2 = 4”: Karl Mannheim: *Ideology and Utopia: an Introduction to the Sociology of Knowledge* (London, 1936), pp. 271–73. Ludwig Fleck’s distinction between “active” and “passive” elements in the production of knowledge seems roughly analogous, although I believe that Fleck is properly read as saying that all knowledge is founded upon “active” human decisions. For a solution of Mannheim’s problems in conceptualizing a sociology of mathematics: David Bloor: ‘Wittgenstein and Mannheim on the Sociology of Mathematics’, *Studies in History and Philosophy of Science* iv (1973), 173–91; *idem, Knowledge and Social Imagery* (ref. 2), chs. 5–7. See also references in Bibliography, Section VI(b).


10 Rudwick is preparing a full-length study of the Devonian controversy. Some additional considerations relevant to understanding this controversy are available in Morrell and Thackray [38, pp. 462–65]; these are briefly pointed out at the end of Section II below. For a general discussion of the sociological significance of different responses to anomalies, see Bloor [125].


12 The argument establishing that in principle all experimental conclusions can be challenged was stated by P. Duhem in *The Aim and Structure of Physical Theory* (Princeton, 1954), ch. 6. His argument is that every experimental procedure rests upon
tacit assumptions. They may be the physical theories taken for granted in the construction of the apparatus, for instance, optical theory in the telescope; they may be the theories used in correcting results for the distorting effects of, say, temperature or friction; or the assumption that all relevant variables have been identified and controlled, e.g. that chemicals are pure, etc. For these reasons no hypothesis can be tested in isolation but only in conjunction with a network of other hypotheses. If an experiment produces unexpected results or appears to refute a hypothesis, it is always possible to lay the blame on a subsidiary assumption in the test procedure. Using the usual notation of symbolic logic: if \( A \rightarrow H \rightarrow O \) and \( \sim O \), then all that can be concluded is \( \sim H \) or \( \sim A \) where \( H \) = hypothesis; \( A \) = background assumption; \( O \) = observation. A decisive refutation would require a proof that there does not exist an alternative \( A \), say \( A^* \), such that \( A^* \cdot H \) produces an 'acceptable' observational outcome. Since proofs of the non-existence of a suitable \( A \) are never available in practice, neither is a decisive or crucial experiment. These themes have been taken up by W. V. O. Quine: 'Two Dogmas of Empiricism' in his *From a Logical Point of View*, 2nd ed. (Cambridge, Mass., 1964), esp. p. 43. In Pickering's usage a 'closed' experimental system would be one in which all variables were perfectly understood and controlled, and all findings deriving from such a system would command universal assent. An 'open' system would be one which was imperfectly understood, measurements upon which would be open to a variety of interpretations. Scientists sometimes behave as if their experimental findings should be incontestable, although Pickering doubts whether such a thing as a 'closed' system exists in nature [16, p. 218].

13 An interesting study of Dirac and the monopole concept, providing background to the episode discussed by Pickering, is Helge Kragh: 'The Concept of the Monopole: a Historical and Analytic Case-Study', *Studies in History and Philosophy of Science* xii (1981), 141–72. In contrast to the sociological studies discussed here Kragh refers differing evaluations of the monopole concept to 'aesthetic' considerations, even while noting disagreement over what counts as a "beautiful" theory (see esp. p. 167).


15 Quite recently there has appeared a programme devoted solely to analyzing scientists' 'discourse': Michael Mulkay; 'Action and Belief or Scientific Discourse?', *Philosophy of the Social Sciences* xi (1981), 163–71; Nigel Gilbert and Michael Mulkay: 'Contexts of Scientific Discourse: Social Accounting in Experimental Papers', in Knorr *et al.* (eds.) [9], pp. 269–94; and a series of papers by Mulkay and Gilbert. This programme is advanced as a way out of a "current analytic impasse" in the descriptive and explanatory project, viz. most of the empirical work discussed in this paper. We should try to analyze *how* scientists talk rather than *what* their talk is about: "It is simply impossible", according to Mulkay, "to produce definitive versions of scientists' actions and beliefs" (p. 169).

There are many problems with this position, not least that relating to the claim that the discourse analyst "is no longer required to go beyond the data". Still, it will be for others to judge whether the 'discourse project' is a contribution to the sociology of knowledge.

16 As Kohler notes, there are echoes in the zymase controversies of the understudied Pasteur-Liebig disputes in the 1870s. For Pasteur fermenters were living things and fermentation was correlated with life; for Liebig fermentation was a phenomenon of
death and decay and was not dependent upon living cells: see René Valéry-Radot: *The Life of Pasteur* (New York, 1960; orig. publ. 1906), pp. 215–22.

17 Recently, some aspects of Allen's work have been criticized by Jane Maienschein, Ronald Rainier and Keith Benson in *Journal of the History of Biology* xiv (1981), 83–158. Their diverse objections seem to centre upon (i) the rapidity of the shift to experimental techniques (which is not an issue in the present context), and (ii) the extent of polarization between morphological and experimental methods; the dichotomy is accepted by Allen's critics, although they wish to stress the complexity of the situation.

18 The empirical studies treated in this section tend to display scientific practitioners making large investments in fairly restricted resources, and different communities disputing the respective value of their resources in an atmosphere of 'crisis'. In a paper about the decline of "particle size analysis" in sedimentology John Law argues that there are circumstances in which scientists may invest in a number of techniques; this spread of investments helps to account for the absence of crisis involved in the abandonment of particle size analysis [34].

19 Morrell and Thackray [38, pp. 466–84] also provide important institutional background relevant to explaining early nineteenth-century British controversies over wave versus corpuscular theories of light and differing views of the adequacy of mathematical methods in physics. In this instance different evaluations were rooted in contrasted Cambridge and Edinburgh pedagogical traditions, as well as in conflicting English and Scottish conceptions of the social and cultural position of science. At the most vulgar level the disputes involved competition for students and alternative schemata for the social support of the man of science. For analyses (mostly pitched at a far less vulgar level) of these fascinating episodes: G. N. Cantor: 'The Reception of the Wave Theory of Light in Britain: a Case Study Illustrating the Role of Methodology in Scientific Debate', *Historical Studies in the Physical Sciences* vi (1975), 109–32, and David P. Miller: *The Royal Society of London, 1800–1835: a Study in the Cultural Politics of Scientific Organization* (unpubl. Ph.D. thesis, University of Pennsylvania, 1981), ch. 3.

20 These paragraphs refer to the British setting. The sparser literature dealing with France points to a significantly different pattern of cultural connections and institutionalization obtaining there; see, for example Dorinda Outram: 'Politics and Vocation: French Science, 1793–1830', *British Journal for the History of Science* xiii (1980), 27–43, also the studies cited in her note 2.

21 For excellent materials on these subjects, see Morrell and Thackray [38, esp. chs. 1, 3, and 5].

22 It is one thing to interpret professionalization as a goal sought by many men of science and as a strategy for securing relative autonomy, enhanced credibility, and greater command of resources, and quite another to posit (as some functionalist writers do) a 'need' for professional status immanent in 'science' or in a hypothesized 'scientific community'. As it happens, the empirical literature tends strongly to support the former view of professionalization and its implications for the sociology of knowledge.


25 Pannekoek’s solution shows that the “calculated” and “true” positions of the modern Neptune were indeed close together during the period of ‘discoveries’ [50, pp. 134–35]. For this historian the Neptunes are ‘the same’ object.


28 This brief discussion of ‘external’ and ‘internal’ factors overlaps with a more extended account in Barnes: *Scientific Knowledge and Sociological Theory* (ref. 2), ch. 5, but the point regarding their status as actors’ categories is still so often forgotten or missed that repetition may be justified.

29 Historians disagree whether such demonstrations may be said to show ‘external’ influences upon science. Writers like Koyré appear to regard neo-Platonic philosophy as part of rational science. Others seem to think of religion and metaphysics as ‘external’ to science, while preserving a crucial boundary around the domain of ‘the intellect’ in general. Again, we may take such boundary-placements purely as expressions of historians’ evaluations unless the issue concerns where historical actors themselves placed cultural boundaries.

30 Webster [78] generally accepts Basalla’s [61] findings while pointing out certain problems arising from the use of mechanical metaphors in Harvey’s overall vitalist orientation. There is some criticism of both Basalla and Webster in Howard B. Burchell: ‘Mechanical and Hydraulic Analogies in Harvey’s Discovery of the Circulation’, *Journal of the History of Medicine* xxxvi (1981), 260–77; Burchell says that contemporary technology played only an illustrative and expository role in Harvey’s work, not a ‘triggering’ role, but it remains unclear how a distinction is made between the language in which discovery is communicated and ‘the discovery itself’. For Harvey’s use of conceptions of the social order see Hill [85].

31 In a short note Barry Barnes has pointed out some significant analogies between how historians deal with the science-technology relationship and how they might more constructively treat the connections between science and social context: Barnes: ‘The Science-Technology Relationship: a Model and a Query’, *Social Studies of Science* xii (1982), 167–73.

32 Set alongside the voluminous historical literature on the Darwin-Malthus link it is significant that there is only one paper dealing with Darwin’s use of the ‘extra-scientific’ resources provided by the culture of pigeon-fanciers, Secord [76], even though one could argue that the patterns Darwin observed there were at least as important to his developing theory of selection as the resources of political economy and natural theology. This historiographic distortion does not escape Secord’s notice, and his paper is in every way a model of how to treat the use of cultural resources in making science.

34 Forman’s paper [66] has been widely criticized by word of mouth, but there has been only one sustained effort to reassess its arguments and the evidence for them: John Hendry: ‘Weimar Culture and Quantum Causality’, *History of Science* xviii (1980), 155–80. The gist of Hendry’s criticism appears to be that Forman neglects ‘internal influences’ on the adoption of acausal perspectives and that he exaggerates the extent to which acausality actually was taken up. Only the specialist can properly assess the weight of Hendry’s particular objections to Forman, but it would seem highly desirable that some competent scholar should recover the ground and examine the relations between purposes within the subculture of physics and purposes which connected physical thought to the wider society. However, in summarizing his overall criticisms, Hendry moves onto shakier ground. He implicitly accepts a common distinction between ‘reasons’ (internal, good) and ‘causes’ (external, bad), and appears to urge that the search for ‘reasons’ ought to have methodological priority. For a discussion of the ‘coercive model’ of sociological determinism that may underpin this historiographic evaluation, see “Conclusions” below.


38 Jürgen Habermas: *Knowledge and Human Interests* (Boston, 1971). See discussions of this perspective in Barnes: *Interests and the Growth of Knowledge* (see ref. 2), ch. 1, and Shapin [103, pp. 63–5].

39 The contrast between 'conservative' and 'natural law' styles of thought is set out in Karl Mannheim: 'Conservative Thought', in *Essays in Sociology and Social Psychology* (London, 1953), pp. 74–164. For empirical studies utilizing Mannheim's categories, see Bibliography, Section VI(a).

40 See some selected references in Bibliography, Section VI(b).

41 It is true that some of the vocabulary Farley and Geison use in their paper invites a psychological reading of their argument: the "influence" of "external factors" upon Pouchet is made to hinge upon his "sincerity" in insisting upon his orthodoxy (p. 184); we are obliged to choose whether Pasteur "allowed 'external factors'" to "influence" him "consciously" or "unconsciously" (pp. 196–97). It would seem, however, that this individualism and psychologism does not sit easily with the main strands of the paper's argument, which is pitched at a sociological level. Interestingly, a critical assessment of this paper has picked upon the psychological and exploited its weaknesses: Nils Roll-Hansen: 'Experimental Method and Spontaneous Generation: the Controversy Between Pasteur and Pouchet, 1859–64', *Journal of the History of Medicine* xxxiv (1979), 273–92.

42 For a variety of reasons (few of which relate to academic concerns) it has sometimes been claimed that relativist sociology of knowledge is at odds with Marxist orientations. One of the most basic issues here concerns the adequacy of purely micro-sociological foci and the methodological role of class analyses. However, MacKenzie has recently shown that Marxist and 'strong programme' explanations are procedurally compatible: Donald MacKenzie: 'Notes on the Science and Social Relations Debate', *Capital & Class* xiv (Summer 1981), 47–60.

43 The 'coercive model' (not so labelled) is most explicitly set forth in Laudan, *Progress and Its Problems* (see ref. 1), ch. 7, where the empirical failures of this approach are given as reasons for rejecting the sociology of knowledge.

44 For example, Quentin Skinner: 'Meaning and Understanding in the History of Ideas', *History and Theory* viii (1969), 3–53; 'Some Problems in the Analysis of Political Thought and Action', *Political Theory* ii (1974), 277–303; 'Motives, Intentions and the Interpretation of Texts', *New Literary History* iii (1972), 393–408.


46 There are many sources for this line of attack; perhaps the most explicit is A.G.N. Flew: 'Is the Scientific Enterprise Self-Refuting?', *Proceedings of the Eighth International Conference on the Unity of the Sciences: Los Angeles, 1979* (New York, 1980), vol. 1, pp. 347–60.

47 It is remarkable how little attention the 'Great Tradition' in the history of science has actually paid to experimental practice. Two recent major studies go some way to remedying this neglect; both point out how problematic is the connection between that practice and the theoretical culture that has been the major focus of historical interest: R. G. Frank, Jr.: *Harvey and the Oxford Physiologists: a Study of Scientific Ideas* (Berkeley, 1980), and, especially, John L. Heilborn: *Electricity in the 17th & 18th*
48 See, for example, Barnes: Scientific Knowledge and Sociological Theory (ref. 2), esp. ch. 1; idem, Interests and the Growth of Knowledge (ref. 2), esp. ch. 1; Bloor: Knowledge and Social Imagery (ref. 2), chs. 2, 8; Barry Barnes and David Bloor: 'Relativism, Rationalism and the Sociology of Knowledge', in M. Hollis and S. Lukes (eds.), Rationality and Relativism (Oxford, 1982), pp. 21–47; and Barnes’s papers in ref. 26.
49 See Shapin [121] for the notion of actors ‘laying bets’ on representations of perceived reality. In this episode the actors themselves privileged their preferred representations and provided psychological and sociological explanations of their opponents’ ‘erroneous’ accounts.
52 Laudan: Progress and Its Problems (ref. 1), 219.

BIBLIOGRAPHIC NOTE

This Bibliography consists almost entirely of empirical work discussed in the text. It is by no means an exhaustive list of relevant studies, but it is inclusive enough to constitute a working bibliography in the historical sociology of scientific knowledge. Doubtless, I have offended many authors, although perhaps the more profound apologies are owed to writers who will be surprised to see their work treated in a sociological context than to those who may (rightly) feel that they ought to have been included.

The Bibliography is arranged into sections closely connected to corresponding sections in the text. For the most part this is a purely conventional categorization of empirical work. Many studies contain material that relates to more than one sociological theme, and some perfunctory indication of these overlaps is given at the foot of several sections in the Bibliography. I have also endeavoured to cite work of possible sociological interest in a few of the Notes. And some wholesale omissions of sociological foci and interpretive themes is pointed out in Section VI of the Bibliography. I have attempted to make this list as current as possible, but, given the healthy state of the empirical sociology of knowledge, I fully expect (and hope) that it will very soon be out of date.

BIBLIOGRAPHY

I. Contingency and the Sociology of Knowledge: Observation and Experiment


Also relevant are Farley and Geison [116]; Kohler [32]; Lankford [49]; MacKenzie [35, pp. 120–25]; Shapin [121].

II. Professional Vested Interests and Sociological Explanation


Also relevant are: Barnes and MacKenzie [111]; MacKenzie [118].

III. Interests and the Boundaries of the Scientific Community


52. Shapin, Steven and Barnes, Barry: 'Darwin and Social Darwinism: Purity and History', in Barnes and Shapin (eds.) [31], pp. 125–42.


Also: Edge and Mulkay [133].

IV(a). The Use of Cultural Resources


IV(b). *The Social Use of Nature in the Wider Society*

97. Rosenberg, Charles E.: 'The Bitter Fruit: Heredity, Disease and Social Thought', in Rosenberg [51], ch. 1.
104. Shapin, Steven: 'Social Uses of Science', in Rousseau and Porter (eds.) [99], pp. 93–139.
108. Smith-Rosenberg, Carroll and Rosenberg, Charles E.: 'The Female Animal: Medical and Biological Views of Women', in Rosenberg [51], ch. 2.

V. Full Circle: Contingency and Wider Social Interests


VI. Other Sociological Perspectives not Discussed in Text

VI(a). 'Conservative Thought'


Also: Harwood [83]; MacKenzie [35, pp. 142–50].

VI(b). Towards a Sociology of Mathematics


Also: MacKenzie [35, ch. 7; 118].
VI(c). \textit{Discipline Formation and Research Schools}


Also: Kohler [33]; MacKenzie [35]; Rosenberg [51, ch. 12].

VI(d). \textit{'Grid and Group': Cultural Bias in the Sciences}


Also: Bloor [125].
I want to examine the difficult and rather obscure idea of a style of thought (Denkstil) as it occurs in Fleck's book on the *Genesis and Development of a Scientific Fact*¹ and his paper 'Zur Krise der "Wirklichkeit"'². I shall try to break down this idea and isolate some of its components. I can see at least some ways of building a bridge between what Fleck says about thought-styles and the detailed work in the history of science that Shapin has outlined for us. If I am right then it should prove possible to define some of the causes of thought-styles and to relate them, in the way that Fleck would have wanted, to facts about the social structure of the *Denkkollektiv*.

I

Fleck wanted us to join him in building a *comparative* and *social* theory of knowledge.³ Let me begin by examining the requirement that our theory be a social one, and then the demand that it be comparative.

One of Fleck's central claims is that in science complexity and trouble are endless. By means of a single example with which he had first-hand experience — the development of the Wassermann test — he illustrates the remorseless need to impose a pattern on the complexity of experience. He confronts us with the perpetual possibility of the breakdown of cognitive order. Every classification that we accomplish is precarious, essentially incomplete and under threat.⁴ The world is too complex for us. "Out of the almost infinite multitude of possibilities" says Fleck "every way of knowing selects different questions, connects them according to different rules and to different purposes".⁵ That there are *stable* conceptions of reality with *stable* styles of thought is therefore deeply problematic. It means that the illusion of simplicity that frequently possesses us must be something that we artfully accomplish. Order must be the result of ceaseless effort. It depends on what Fleck calls 'work' — "serious, continual work by large groups and great men".⁶

II

If this explains (at least in part) what Fleck meant by a social epistemology, what of its comparative aspects? Here, it seems to me, Fleck's ideas were less

developed. Recall what he offers on this question. Basically he gives three examples which point to a comparative treatment of knowledge. First, he contrasts ancient with more modern conceptions of an illness such as syphilis. The ancient conception stressed the moral significance of disease, the modern conception tends to be morally neutral. Nevertheless the modern understanding is still based on a whole variety of assumptions that could in principle be challenged. Thus the boundaries of the organism are conceived in a certain way and metaphors of invasion and defence are rife. Secondly, Fleck contrasts the inner and outer circle of knowers — the esoteric and exoteric regions and forms of knowledge. The certainty and rigidity of knowledge, he suggests, is a function of social distance from the confused circumstances of its creation. Thirdly, and perhaps most interesting of all, Fleck referred to what he called the 'classical period' in the development of a theory. He defined this as the period when its limitations tend to be ignored and when its adherents most confidently proclaim its truth and finality. Fleck treats this as a typical phase in the life of a scientific theory, before it becomes overwhelmed with anomalies.

These three ideas certainly represent the beginnings of a comparative approach. But they are only beginnings. We must unfold and develop these hints. Fleck himself provides a clue which shows how this may be done. Recall that he describes certain factors that are present in every cognition. There is tradition, and there is education and finally there is what he calls 'the sequence of acts of cognition'. I want to concentrate on this sequence of acts of cognition. What are these acts? Fleck said that they are things like the following:

(i) acts of judgement in which 'small divergences are not taken into account'.

(ii) judgements that a rule or law still holds despite 'exceptions' or 'restrictions'.

(iii) judgements of importance and relevance as when a troublesome experimental outcome or observation is dismissed as 'accidental' or 'unessential'.

(iv) judgements that a law should be 'supplemented' by means of more or less ad hoc complications or additions.

There is a lot that could be said about this interesting list of cognitive acts. The exercise of discretion to which they refer has been explored in detail by a number of writers. For my purposes, however, the thing that stands out is that this sequence of acts is a sequence of responses to anomaly. They are ways of responding to potential counter-examples to the established or
‘traditional’ body of knowledge. The ‘work’ of sustaining cognitive order is broken down by Fleck into an endless sequence of responses to anomaly.

Here I think we have isolated at least one of the determinants of cognitive style. The style of a body of knowledge (if it has a determinate style) might be created by the systematic application of a particular strategy for dealing with counter-examples and anomalies. If all the judgements that constitute Fleck’s ‘sequence of cognitive acts’ are biased in a particular way, then the cumulative effect would be to give the resulting system of belief a recognisable physiognomy or style. I do not say that this is the whole story. It isn’t, and later I shall explain why it isn’t the whole story — but it may be a crucial part of it. This is certainly how Fleck construed the ‘classical’ phase of a theory. Here a simple, useful picture of nature is protected by a ‘thousand cunning devices’.16

A very interesting question can now be posed. Are there an infinite number of ways of responding to anomaly? Or are there perhaps a finite, or even a very small and limited, number of possible responses? If there are an infinite number of ways then a comparative exercise becomes too complicated. At best we can merely describe one style after another. Suppose, however, that we can discern a small number of recurring strategies. Then we can put some order into cultural complexity.17 The same kinds of response will occur again and again in the historical record and illuminating regularities and patterns might be found. We will have a ‘space’ of different strategies and a space of different styles of thought — with the hope of discerning their underlying causes and connections. I speak of the underlying cause here because it is important to notice that on the present approach style is the outcome of Fleck’s ‘sequence of acts’. It is not itself the explanation of that sequence. For the explanation we must look elsewhere.

III

I think that a good case can be made out for saying that the number of strategies for responding to anomaly is not indefinitely large, but is in fact very small. Indeed I think that there are only about four or five different strategies. So there ought to be only about four or five different styles of thought — at least as far as this mechanism of generation is concerned.

I cannot argue this claim fully here, but two considerations may help to make it plausible. Most of you will be familiar with Imre Lakatos’s brilliant book Proofs and Refutations.18 You will recall that he studied a mathematical debate that lasted for about one hundred and fifty years. The debate
concerned the validity of a theorem of Euler’s about the relation between the number of edges, faces and vertices of a polyhedron. He found that just four or five methods of responding to counter-examples had been used in its entire course. Lakatos gave these strategies amusing but revealing names: ‘monster-barring’, ‘exception-barring’, ‘monster-adjusting’, ‘primitive exception barring’, and finally the ‘dialectical’ strategy — this being the one that he favoured. The important point, though, is that the same moves appeared time after time and generated recognisable kinds of mathematical work.\textsuperscript{19}

Another clue which shows that there may only be a small number of strategies is this. Think of the number of ways that a social group might react to a stranger. There might be outright rejection, or strangers may be made welcome. Again, they might be subjected to various kinds of assimilation so that they are treated not as strangers (whether welcome or unwelcome) but as members of the group. They could be slotted into existing categories or given a special status within the society. Again, notice how limited are the possibilities — just four basic kinds of response are available.

Drawing an analogy between strangers and potential anomalies is not as far-fetched as it may sound. Strangers, like anomalies, occasion both opportunities and threats. They need to have a meaning assigned to them and some act is called for to articulate their relation to established practices and social categories. The very components that Fleck described (tradition, education and cognitive ‘work’) must be brought into play in these cases too. Furthermore, as anthropologists have noted, the response to classificatory anomalies in the natural world is often a symbolic way of responding to strangers in the social world.\textsuperscript{20}

Every strategy, whether applied to the natural or the social world, has its dangers. It has its costs as well as its benefits. Too much consistency (that is, too much rejection) causes what Fleck called ‘one-sidedness’. An overly protective attitude can produce a narrowly defined body of knowledge that misses opportunities that others may exploit. On the other hand too little effort to protect existing accomplishments (that is, too much criticism or too much welcome to strangers) leads to what Fleck calls ‘sterility’.\textsuperscript{21} He might have better called this state one of ‘chaos’ or ‘anarchy’. But if every strategy has its dangers, they are not dangers that can be avoided. These strategies are not merely open to us, the choice of one or the other is a necessity imposed upon us at every moment in scientific work. There is always work to be done because, as Fleck put it, the ‘horizon’ always moves away from us. There is, says Fleck, “no law without exceptions”.\textsuperscript{22}
We must now connect together the comparative and social considerations in order to get a theory that is truly comparative and social. The connection is this. We must look at the social meaning of anomalies. We must follow the anthropologist by looking at the various ways in which anomalies may be put to work, exploited or resisted by the members of a Denkkollektiv. Do they pose a threat and, if so, to whom are they a threat? Do they provide welcome opportunities, and, if so, by whom are they welcomed? What are the purposes, goals and interests that impinge on any given cognitive act? The crucial thing to realise, however, is that the use and the meaning of an anomaly are not matters of individual volition or choice. They are subject to social constraints. An anomaly will only have a certain meaning if the collectivity endow it with credibility. An anomaly can only be put to a certain use if the social circumstances make that use possible and sustain it.

Let me illustrate and justify this claim by a simple example. Suppose that you have a closed and bounded group all committed to a single theory. For example, suppose that there is only a single recognised academy or centre of learning — the community of men of knowledge is clearly bounded, and dissidents can be banished. Under these circumstances it is possible to sustain a theory or a law by casting out potential counter-examples as ‘irrelevant’ or spurious or the result of incompetence. By a suitable definition of terms or by an expedient piece of reclassification a counter-example can be cast out and declared to be an abomination. Here, I think, we have the social circumstances necessary for Lakatos’s strategy of monster-barring to find a successful application. The counter-example is not a real counter-example. It is something unnatural and we should turn our backs on it. “No”, said the defenders of Euler’s theorem: “two tetrahedra joined at a vertex do not refute the theorem that, for all polyhedra the vertices (V), edges (E) and faces (F) are related by the formula $V - E + F = 2$”. And why not? Because the twin tetrahedron is not a real or true polyhedron. It is a monster. The defenders then produced an ingenious new definition of a polyhedron in order to prove their point.

Clearly this method can only work if it is possible to expel any supporters of the potential counter-example. It is not the world itself that makes trouble for our system of belief, it is other believers. If there is nowhere for the supporters of a counter-example to seek refuge then those able to muster support have a very effective threat available to them. Conversely, for a group
with the kind of structure that I am imagining it is easy to see why an anomaly should be deemed monstrous. There is a clear sense in which it may pose a monstrous threat. It may be seized upon by dissident, disaffected or peripheral members of the group and made into an excuse for a scientific revolution, or a radical change in theory and practice.23

The picture that I have painted is obviously very simple and idealised. But something not too unlike this occurred in some of the cases from the history of science that Shapin mentioned. Consider for example the initial outright rejection of the wave theory of light by the Paris Academy, and then the palace revolution that brought the work of Fresnel, the outsider, into the foreground.24 Or again, consider the rejection by Pasteur of the ‘incompetent’ experiments by Pouchet which seemed to contradict the orthodox separation of living and non-living matter.25 Or recall the analysis of the debate between Pearson and Yule over the measure of association of nominal variables.26 When Yule produced a measure of association that did not serve the Eugenic interests of Pearson’s dominant group it was met with hostility and rejection. It was said to be ‘dangerous’.27 If Pearson’s influence in the statistical community had been absolute rather than merely very great, then Yule’s Q-coefficient would have been relegated to the status of a mere arithmetical oddity.

Now let us imagine slightly more complicated circumstances. Rather than the relevant community being a single, bounded group let us suppose that the supporters of a counter-example have a power-base of their own. Suppose there are rival academies. Anomalies and complicating factors cannot be so easily dismissed because their supporters cannot be banished. Compromise will be necessary. For example, an anomaly might be accepted as a ‘restriction’ on the ‘scope’ of a theory or law. Alternatively the anomaly may have to be taken seriously but redescribed and reinterpreted so that it leaves existing accomplishments intact.

This is what happened in another of the cases that Shapin mentioned in passing — the reception of non-Euclidean geometry in late nineteenth-century England.28 By reinterpreting non-Euclidean geometry as the projective geometry of curved surfaces within a Euclidean space a threat was removed. What was that threat? It was a social threat posed by the supporters of non-Euclidean geometry. They were using it to make propaganda for a radical, empiricist theory of knowledge. Its supporters were to be found amongst a group known as the ‘scientific naturalists’ of whom the mathematician W. K. Clifford was a prominent member. The naturalists quite explicitly claimed that empirical science was the only form of knowledge and that scientists
where the only genuine men of knowledge. This propaganda by the militant wing of the scientific profession deeply worried more conservative scientists who were willing to compromise with the Church. For them Euclidean geometry was a symbol of non-empirical, transcendental certainty; hence the importance attached to neutralising and re-interpreting non-Euclidean geometry.

V

You will have seen that what I am doing is distributing Fleck's sequence of cognitive acts across a space of social structures. By imagining simple models for the structure of a Denkkollektiv I am trying to locate the circumstances under which different strategies of responding to counter-examples will be credible and attractive. What historical work shows is how important it is to look at the precise determinants operating upon each of the sequence of cognitive acts that make up the work of science. If we are to take Fleck's model seriously we must resist the temptation to assume that a tradition (or style) of scientific work will unfold itself. It contains no inherent implications that 'determine our Reason' or guide our understanding. To think of a theory, a tradition or a style, in science as if it contains an immanent line of development is to forget the importance of Fleck's 'sequence of cognitive acts'.29 For an adherent to a truly comparative and social epistemology each act is something to be explained. At each point we must ask: what interests are at stake? — whether these be broadly conceived interests or narrowly professional interests. We must ask: what purposes are being served and who is benefitting? We must ask: what is being risked, and who will be blamed?

These may sound simple-minded questions, but they are not. It may be granted that they are not elevated and uplifting in tone, but they get us a surprisingly long way in answering the question of why specific acts of cognition take the form they do.30

VI

I do not wish to pretend that strategies of responding to anomalies are the only determinants of scientific style. Even though the concept of style awaits a more precise definition it has a sufficiently clear, intuitive meaning to see that there are other determinants. One obvious candidate for explaining different styles is the use of different models and metaphors in the original formulation of the theory. In particular we may notice that just as there are recognisably different styles of life in different social structures, so recognis-
ably different styles of knowledge would result if the basic picture of nature were modelled on a social form.31

Historical work that has been done on the corpuscular philosophy of Boyle and Newton suggests that metaphors of hierarchy derived from the social structure have played a surprisingly important role in its development. For Boyle and Newton it was vital to stress that matter was passive not active. Matter depended for its motion on active principles and spiritual forces. It appears that a crucial consideration behind this principle was that it allowed nature to be given a certain social meaning. It made it useful as an object lesson because of the universal assumption of the time that the ‘world politic’ and the ‘world natural’ would be closely analogous.32 For ‘matter’ read ‘people’; for ‘active principle’ read ‘church’. Properly understood this historical example is of profound interest to the sociologist of knowledge.33 Notice, however, that it complements rather than contradicts what I have said about anomalies. Both the choice of basic metaphor and the response to anomaly can be illuminated in terms of the idea that nature is being put to a social use. But whatever the ultimate relationship between the different determinants of scientific style the claim that I want to draw attention to is this: studying the structures of power and interest behind responses to anomaly provides one important method for clarifying the intriguing phenomenon of Denkstile.

NOTES

3 Genesis, pp. 43, 51, 64.
4 Genesis, especially pp. 18 and 19; also p. 95. Science “has no demonstrable beginning and is open ended”. For an account of the interconnected character of factual statements and the limitless effects of the changes that may be wrought on any particular part of the system, see p. 102. See also the beautiful description on p. 114 of the circularities that attend practical reasoning in science cf. pp. 18, 19 and 114. These attest to the subtlety and depth of the constructive work which goes into apparently simple matters of fact.
5 ‘Crisis’, p. 49 in this volume.
6 ‘Crisis’, p. 50.
7 Genesis, pp. 59–62.
9 Genesis, pp. 93–94.
DETERMINANTS OF COGNITIVE STYLE IN SCIENCE

10 'Crisis', p. 47.
11 'Crisis', p. 51.
12 'Crisis', p. 51.
13 'Crisis', p. 51.
14 'Crisis', pp. 51–52.
16 'Crisis', p. 54.
17 The reader who is familiar with the work of the anthropologist Mary Douglas will recognise at once the extent to which my approach is based on her work. Indeed the thesis of my paper could be expressed in the following terms: that one valuable development of Fleck's position is to be found by equating what Douglas calls 'cultural bias' with Fleck's 'styles of thought'. It is perhaps significant that Douglas's work belongs to the tradition of Emile Durkheim, and Durkheim is one of the relatively small number of sources quoted by Fleck. The works of Mary Douglas that are most relevant to the present discussion are:
20 See the works of Douglas referred to in note 17.
21 'Crisis', p. 52.
23 This approach provides an answer to a long-standing problem in Kuhn's theory of paradigm change. What is it that turns an anomaly into a crisis-provoking anomaly? On the present approach it would not be an intrinsic property but an imputed property. It would be a comment on the use to which some or all members of the scientific community chose to put it. The circumstances behind this use then become the subject matter for particular case studies.
29 Karl Mannheim spoke of the inner logic of a theory or system of belief and was content to see the sociology of knowledge deal with *deviations*. See for example *Ideology and Utopia*, Routledge & Kegan Paul, London, 1936, pp. 239–40. A similar position is to be found more recently in the later work of Lakatos on the relation between internal and external history: I. Lakatos: 'History of Science and Its Rational Reconstructions', *Boston Studies in the Philosophy of Science* 8 (1971), 91–135. Fleck's stress on the sequence of cognitive acts makes him much more radical. For this is precisely a device for fragmenting the flow of 'natural' or 'logical' implications or the smooth guidance of 'meanings'. In fact Fleck's theory is much more like that of Wittgenstein in the *Philosophical Investigations*, Blackwell, Oxford, 1953. He shares with Wittgenstein a commitment to what may be called a 'finitist' theory of meaning. Meanings are created in the specific, local context of use and are strictly confined to that context. Each and every extension of usage is problematic. Other similarities with the later Wittgenstein are, for example, their sense of the fluid relation of symptom and criteria, and something like a family resemblance theory of all classificatory predicates.
30 See particularly the items in Shapin's bibliography under the headings 'Professional vested interests and sociological explanation' and 'Interests and the boundaries of the scientific community'. For a discussion of how responses to anomaly and scientific styles can be related to the socially determined concepts of 'mistake' and 'blame' see C. Bloor and D. Bloor: 'Twenty Industrial Scientists: a Preliminary Report', in M. Douglas (ed.): *Essays in the Sociology of Perception*, Routledge & Kegan Paul, London, 1982.
31 A particularly simple example would be, say, theories which appeal to metaphors of fragmentation and atomisation in contrast to theories which appeal to metaphors of organic unity. See for instance:
S. Shapin: 'Phrenological Knowledge and the Social Structure of Early Nineteenth-Century Edinburgh', *Annals of Science* 32 (1975), 219–43; and
Not only specific theories within science but also general and philosophical theories of knowledge can be based on social metaphors. It is, for instance, revealing to look at the
Kuhn-Popper debate in the light of their two underlying models of society. This stylistic conflict in the theory of knowledge then becomes another symptom of a clash between two long-standing social ideologies — the Romantic (or Conservative) stance and Enlightenment individualism: cf. D. Bloor: *Knowledge and Social Imagery*, Routledge & Kegan Paul, London, 1976, Ch. 4.

32 See particularly the work by J. R. and M. C. Jacob in Section IV(b) of Shapin's bibliography.

BERNARD ZALC

SOME COMMENTS ON FLECK’S INTERPRETATION OF THE BORDET-WASSERMANN REACTION IN VIEW OF PRESENT BIOCHEMICAL KNOWLEDGE

The core example chosen by Fleck to develop his concepts of the “genesis and development of scientific facts” is the serodiagnosis of syphilis known as the Wassermann reaction (in France and Belgium, this serological test is called the Bordet-Wassermann reaction to make the fact that Wassermann’s contribution to this serodiagnosis was to apply to syphilis the experimental procedure of complement deviation set up a few years earlier by Bordet and Gengou) [1, 2]. This immunological reaction, which is quite simple in its principles, turned out, when applied to the serodiagnosis of syphilis, to be extremely complex from the point of view of both its practical realization and its theoretical interpretation. Due to the socio-medical importance of the subject, this complexity had led to a tremendous volume of literature, which following Fleck’s own estimation amounted in 1934 to about ten thousand papers. Fleck concluded rightly: “There certainly cannot be many similar specialized problems which have had so many papers devoted to them” [3].

Thus, for a better understanding of Fleck’s demonstration, it is certainly of importance to well understand the ins and outs arising from this reaction. In the following paragraphs, I will try to develop the immunological and biochemical insights of the Bordet-Wassermann reaction in view of our knowledge and interpretation of today on how this serodiagnosis test was realized. I will first briefly state the principles of the Bordet-Gengou reaction (known also as the complement fixation test). In a second part, I will analyze how the chemical nature of the antigen involved in the serodiagnosis reaction of syphilis, i.e., a lipid, has led to so many technical problems, which in turn have biased the theoretical interpretation of the Bordet-Wassermann reaction.

1. THE COMPLEMENT FIXATION REACTION

Bordet and Gengou set up their reaction in 1901 [1], after the following observations had been reported by several authors:

1.1. If one injects an animal (a rabbit for instance) with red blood cells from another species, such as sheep red blood cells (SRBC), the serum of this

rabbit has the property to lyse SRBC. We shall refer to this serum as RaSRBC. The reaction can be written as:

\[ \text{SRBC} + \text{RaSRBC} \rightarrow \text{hemolysis} \]

1.2. If one heats the RaSRBC at 56 °C for 30 min (hRaSRBC) hemolysis no longer takes place:

\[ \text{SRBC} + \text{hRaSRBC} \rightarrow \text{no hemolysis} \]

1.3. Addition to the reaction mixture of serum from a non-immunized animal (guinea pig serum (GPS) is the most efficient) restores the hemolytic properties of the hRaSRBC:

\[ \text{SRBC} + \text{hRaSRBC} + \text{GPS} \rightarrow \text{hemolysis} \]

1.4. If GPS is heated at 56 °C for 30 min (hGPS), hemolysis no longer takes place:

\[ \text{SRBC} + \text{hRaSRBC} + \text{hGPS} \rightarrow \text{no hemolysis} \]

These experiments demonstrated the presence in the normal serum of a component, heat unstable, which is responsible for hemolysis of RBC by an anti-RBC serum. Bordet had also demonstrated that this component (which he called alexine but which is now uniformly called, after Ehrlich, complement (C')), was also capable of destroying some microbes when incubated with an anti-microbe serum. At the turn of this century, Ehrlich postulated that the C' responsible for the lysis of RBC was different from the one responsible for the lysis of germs. The French school following Bordet favoured the hypothesis of the unicity of C'. To demonstrate the correctness of their hypothesis, Bordet and Gengou devised one of the first, if not the first, competition experiment in a biological system. They showed that if a microbe-anti-microbe serum system is preincubated with C', there will not be RBC lysis after addition of RBC-αRBC serum to the mixture. The reverse experiment was also performed, that is after reacting RBC with αRBC serum in the presence of C', the anti-microbe serum will not have a microbicidal power on the germ, if added in the same mixture. Of course, both the αRBC and the anti-microbe serum had been heated at 56 °C for half an hour prior to the incubations in order to inactivate the C' present in these sera. These experiments demonstrated that the first system involved (either microbe-anti-microbe or RBC-αRBC) had consumed all the available C' which was then not available for the second system (RBC-αRBC or microbe-anti-microbe, respectively) and thus, that it was the same C' which was active in both systems.
If we consider that a germ is an antigen (Ag), and that the anti microbe serum is an antiserum (As), we know that when mixed together the Ag and the antibodies (Ab) present in the As form a complex which fixes C'. This C' is present in the As or if this latter has been heat inactivated (hAs) the Ag-hAs mixture can be supplemented in C' by adding GPS. The reaction can be written:

\[ \text{Ag} + \text{Ab} + \text{C'} \rightarrow (\text{Ag-Ab-C'}) \]

Fixation of C' on the Ag-Ab complex is difficult to estimate. On the contrary, fixation of C' on the SRBC-hRaSRBC complex is easily visualized, as it results from a hemolysis of the SRBC. Thus the idea of Bordet and Gengou was to use this competing system to prove the presence of an Ab in a serum. If we call the SRBC-RaSRBC complex the hemolytic system (Hs), we can write the two following possible reactions.

1.5. If in the serum studied there is an Ab specific for the Ag, the Ag-Ab complex will fix the C', and when adding the (Hs) no hemolysis will be observed:

\[ \text{Ag} + \text{Ab} + \text{C'} \rightarrow (\text{Ag-Ab-C'}) \\
(\text{Ag-Ab-C'}) + (\text{Hs}) \rightarrow \text{no hemolysis} \]

1.6. If in the serum studied there is no Ab specific for the Ag, then there will still be free C' to hemolyze the (Hs):

\[ \text{Ag} + \text{serum} + \text{C'} \rightarrow \text{Ag} + \text{serum} + \text{C'} \\
\text{Ag} + \text{serum} + \text{C'} + (\text{Hs}) \rightarrow \text{Ag} + \text{serum} + (\text{C'} - (\text{Hs})) \rightarrow \text{hemolysis} \]

The complement fixation reaction is an extremely useful immunological test which is still used nowadays. It is sensitive, specific and reproducible. Furthermore it can be performed with a soluble antigen as well as with an Ag not solubilized and present on a membrane. This reaction is extremely reliable provided proper controls are performed. These are the following:

- **Negative Controls**

  - the Ag alone should not bind C'; binding of C' to the Ag frequently occurred when dealing with lipidic Ag. This feature explained many of the false positives observed in the Bordet-Wassermann reaction.
  - the serum studied should not, in the absence of Ag, fix C'. Some serum unfortunately does present this so-called anticomplementary side-effect. It is due either to the presence of soluble immune complex in the serum, or to
other factors such as a too high concentration of lipids in the serum. Thus it
is important to take blood from a fasting subject. I do not know whether or
not this condition was respected when the Bordet-Wassermann reaction was
first described, but this might also have been a source of false positives.
• the C' should not be destroyed or damaged. This is checked by measur-
ing its capability of lysis of the (Hs) in the absence of Ag and serum.
• the (Hs) should be of good quality, i.e., there should not be a spon-
taneous lysis in the absence of C'

– Positive Control

The completion of the reaction with the serum to be tested should be com-
pared with a reference positive serum known to contain specific Ab directed
towards the Ag.

The importance of running the proper controls was known at the time
when the Bordet-Wassermann reaction was first described as shown by Fleck’s
quotation of Julius Citron’s book, published in 1910 [4, 5].

2. HOW HAS THE REACTION BEEN CONDUCTED FROM
WASSERMANN’S PAPER TILL NOWADAYS?

2.1. Source of Serum

In his original paper, Wassermann used two different sources of syphilitic
sera. One came from human syphilitic patients, the other came from monkeys
which had been experimentally infected with syphilis (positive control).

2.2. Source of Antigen

When described by Bordet and Gengou, the Ag was the germ studied, and not
a purified molecule. As it was not possible to cultivate treponema pallidum
(the causative agent for syphilis), Wassermann used as a source of treponema
the homogenate of syphilitic new-borns’ livers, which were improperly called
syphilitic extracts. One year later, Marie and Levaditi [6] reported that
positive deviation of complement was possible using homogenate from liver
absolutely devoid of treponema [7]. Furthermore, Landsteiner et al., Porges
and Maier, and Levaditi and Yamanouchi [8, 9, 10] reported that an alcoholic
extract of non-infected liver gave also a positive complement fixation with
syphilitic serum. This led these authors to question the specificity of the
Bordet-Wassermann reaction. The fact that the active substance present in the
liver was soluble in alcohol was not an argument that this substance could not be an antigen. The main argument against the specificity of the Bordet-Wassermann reaction was that homogenates or lipidic extracts from organs devoid of treponema were able to give a positive reaction with syphilitic serum. Indeed, in 1908 already, brain, red blood cells, heart or kidney alcoholic (i.e., lipidic) extracts from either human or other species were also reported to be effective as "antigen source" for the reaction [11]. All these observations had been reported during the three years following Wassermann’s initial paper. Since then, and till 1942, when Pangborn [12] isolated, in alcoholic extracts from beef heart, the factor responsible for the reaction with serum from syphilitic patients, no progress in the comprehension of the reaction had been made. Pangborn isolated a phospholipid which she called cardiolipin. This lipid has the structure of a diphasphatidyl glycerol. When used pure in complement fixation reactions, it is anticomplementary; i.e., the lipid binds C' in absence of Ab. This is probably due to the fact that cardiolipin in an aqueous medium forms hexagonal structures [13] (inverted micelles) which are extremely hydrophobic (Fig. 1a). Very soon after isolation of cardiolipin, several authors found that to circumvent this anticomplementary interference one should combine pure cardiolipin with a suitable quantity of lecithin. The sensitivity of the mixture, in the serological reaction, could then be increased substantially by adding the proper amount of cholesterol [14]. From a biophysical standpoint, the effect of adding these so-called "auxiliary lipids" (i.e., lecithin and cholesterol) is the following: addition of lecithin to cardiolipin suspension transforms the hexagonal structure in a micellar structure (mixed micelles) where the polar head group

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**Fig. 1.** Schematic representation of structures formed by cardiolipin in water solution. (a) Cardiolipin alone. (b) Cardiolipin + lecithin. (c) Cardiolipin + lecithin + cholesterol.
of the lipid is more readily exposed and accessible to the Ab (Fig. 1b). Addition of cholesterol to these mixed micelles, creates vesicles (or liposomes) consisting of a bimolecular layer, where the lipidic antigen is presented in a manner comparable to its presentation in a natural membrane (Fig. 1c).

Cardiolipin is a lipid which is ubiquitously distributed in mitochondria of animal and plant tissues and bacteria. It is also a constituent of treponema. When injected intravenously in rabbits (provided it is mixed with lecithin, cholesterol and methylated albumin) it produces a high-titer antiserum serologically very similar to syphilitic serum [15]. As stressed above, complement fixation tests can be performed on biological membranes, without extracting the antigen. As cardiolipin is present on treponema as well as on liver, this explains why the reaction was possible on liver homogenate, whether or not infected with treponema. Alcoholic treatment of tissue homogenates, extracts mainly phospholipids and cholesterol. Thus what was used by Levaditi in 1907 as "antigen source" was very similar to a mixture of cardiolipin-lecithin-cholesterol [10]. The difficulties encountered by all the different users of the reaction came probably from the variability in the composition of their lipidic extracts. This variability came either from the organs used for the extraction, the temperature of extraction, the ratio of water to alcohol used, and so on. Standardization of the extraction procedure had allowed a better reproducibility of the technique already before the isolation of cardiolipin. Even after the Pangborn publication, it took several years to find the proper ratio of cardiolipin-lecithin and cholesterol to perform the reaction with the best reproducibility and sensitivity [14].

It is interesting to analyse why it took so many years to give a correct interpretation of the Bordet-Wassermann reaction. The value of the reaction for the diagnosis of syphilis, despite some drawbacks due to technical difficulties in its practical realization, was not questioned. Nevertheless, it is surprising to note that the reaction had been misinterpreted for so many years. The rationale behind this misinterpretation lies in the confrontation of experimental facts with several interpenetrating dogmas. First is the axiom of specificity of the Ab-Ag reaction. In other words a serum containing antitreponema Ab should react only with the treponema. Thus the obvious reciprocal was that the reactivity of syphilitic sera with various noninfected organs could not be due to an Ab or to treponema. Second is the notion that an Ag should be a protein, as only proteins were supposed to have a sufficient molecular diversity to bear species' and organs' specificity. Indeed, at that time, the few lipids whose structure had been established (lecithin, cholesterol or sphingomyelin) were found in most of the tissue analyzed, and thus it was
hard to imagine that lipids could be the support of immunological specificity. Now we know that lipids are immunogenic. These are mainly steroids and glycolipids. Except for cardiolipin, phospholipids are less immunogenic. The interesting thing about lipids is that if the same lipid is present in two different organs in the same individual, or in the same organ in two different species, they will be recognized by the same Ab. The two most famous examples of these cross-species reactivity are Forssman Ag (first described in 1911) [16] and the brain lipid specific Ag (described in 1926) [17] which has been demonstrated by Joffe et al. to be galactosyleramide [18]. Thus by the time Fleck wrote his book, it was known that lipids could be responsible for cross-species reactivity. Furthermore, in 1909, Levadić and Mutermilch [19] had already reported that choleric vibrio specific Ag was alcohol soluble (and could thus presumably be a lipid). It is then quite surprising that none of the researchers involved in the field of syphilis diagnosis had imagined that in the case of the Bordet-Wassermann serological test the antigen could be shared by the treponema and normal organ. Even in 1939, Bordet [20] still questioned the existence of anti-treponema antibodies in the sera of syphilitic patients. According to the hypothesis that complement fixation serodiagnosis of syphilis would assay a true Ag-Ab complex, Bordet supported Weil’s suggestion following which auto-antibodies appeared to be directed toward lipodic antigens liberated by normal cells which had deteriorated during the treponema infection [21]. In the same paper, Bordet also mentioned the alternative hypothesis that the Bordet-Wassermann reaction would not detect an antibody, but reflect a chemical alteration of syphilitic blood. It is quite surprising to realize that one or two years before the discovery of both the chemical structure of the antigen involved in the Bordet-Wassermann reaction, and of penicillin which would nearly eradicate the disease, the old concept of ‘syphilitic blood’ was still vivid. We have here a characteristic example of how an established dogma (the concept of syphilitic blood which was not even based on any scientific observation) can bias the reasoning of nearly two generations of scientists, among which were the most famous founders of modern immunology.

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FLECK'S STYLE

THE CONCEPT OF STYLE

As Fleck himself encouraged us to do, I shall attempt a survey of his main work through one of his own methodological concepts. This concept which he applies in a sometimes loose but always stimulating way is the concept of style (Denkstil).

With this, he intends to point to something that looks like a vision of the world within one's field. People who choose (or think that they freely choose) a style look at the phenomena in a particular way and expect a certain structure in nature.

This vision of the world implies the choice of certain nouns and verbs and an emphasis on certain modes and aspects of action; it suggests what is likely to happen and what is not likely to happen and so on. . . . Style has something to do with certain elliptic formulas that easily speak to the reader's imagination. Fleck comments on the vividness of style (Lebendigkeit des Stil) and, in doing so, he refers to some selected elements that can captivate the audience and make some types of approach popular. Such short statements as 'One cell, an antibody', or 'One gene, one enzyme' (the so-called central dogmas of biology) are obviously such elements of style.

This 'style' is not natural, but acquired; it is most unconsciously learnt. That is Fleck's discovery, from my point of view: in the biologist's style two unconscious worlds interfere: the social unconscious and the individual unconscious. The style of thought is acquired through society's influence (through education for instance) but not exclusively. The individual ego can achieve a compromise with the socially determined part. This analysis can be applied to Fleck's own style of thought.

FLECK'S STYLE

Fleck's thesis, allegedly supported through the example of Wassermann's reaction, is that a scientific fact is not a percept, but a construct, and results from scientific theories which are themselves broadly social constructs. Fleck's book tells briefly how medical bacteriology, or the science founded

on the germ theory of disease, has spread its influence (over the two 'circles' of society) and progressively induced people to believe in the motto 'One germ, one disease' to which the French biologist Paul Bert added 'One vaccine'.

The main trend of this bacteriology is specificity. Fleck analyses this medical style which is founded on specificity, and suggests one alternative, another style of thought that would not be so committed to specificity.

According to most historians of science, the triumph of bacteriology was linked with a twofold acknowledgement:

- first, many diseases are associated with a specific bacterium;¹
- secondly, bacterial cultures, when available, provide a nice in vitro model, useful for the understanding and management of the disease. The so-called serological reaction refers specifically to the bacteria-serum reaction on the slide, but it is meaningful as a 'serodiagnosis' when pointing to the event of the disease.

As a consequence of the latter, the physician could rely upon the laboratory for the diagnosis of a pathological state. No matter whether he read the report in a clever professional and concerned manner (if he had got a bit of bacteriology) or in a rather mystical way (if he was at a loss to decipher it).

Fleck's idea is that the diagnosis of a disease cannot be reduced to the simple discovery of the germ. In doing so, he is not only challenging philosophical positivism; he challenges the choice that medical scientists made once, that is to say bacteriology versus immunology.

BACTERIOLOGY VERSUS IMMUNOLOGY

The idea of a germ theory of disease was victoriously waved against the old hygiene, a discipline based mainly on the famous 'vital statistics'. It maintained that disease was the product of a complex mixture where poverty and filthy air, bad food, exhausting work and hereditary factors, including acquired characteristics like various forms of degeneration, were acting together with bacterial agents, when they began to be suspected (either miasma or moulds or fungi . . .).

If bacteriology were to acquire supremacy, the definition of species in bacteria was indeed a crucial problem. If one disease could be ascribed to a particular sort of bacteria, the latter was expected to have a stable form and to be recognizable in every patient. If nosology was actually modified by bacteriology (some new categories merging, others becoming outdated), the most important part of the theoretical construct still stood up, but inspired increased confidence.
FLECK'S STYLE

For Koch's school, the main criteria of bacterial species were morphological, thanks to the extensive use of stains (bacteriology was 'finger-stained'). For Pasteur's school, they were rather physiological, relying on the discovery of ferments. Numerous authors went on to ascribe a great variety of forms to bacteria, following the meanders of a complex cycle that looked like the protozoa's cycle. One good example is Ferrán's description of the cholera bacillus:

The bacillus being cultivated in the same dejections, there appears... an infinity of bacilli. If cultivated in soup or in gelatine, the spirilla then appear. But if it is transferred to the alkalinated soup, flexible filaments... One of the spirilla has a small globe at one of its extremities... This sphere or oögon is formed by a protoplasm of refraction... Later this protoplasm contracts and leaves a void spherical casket... In this part of the sphere we notice a work of segmentation that ends in converting it into highly visible granulations... The granulations being placed in a suitable medium... change into muciform bodies... (Conference on the cholera parasite, translated from the Spanish report of C. Soli on the work of Dr Ferrán: Edinburgh Medical Journal 31 (1885–1886), 142–143.)

At the same time, Pasteur wrote a paper about his vaccine against fowl cholera. The idea of a spontaneously attenuated virus was not new, but the device of attenuating viruses was. The culture of fowl cholera had lost its virulence and Pasteur demonstrated that this transformed culture did transform inoculated animals into immune ones. The principle of a double series of modifications was coming through. What was the extent of these modifications? Pasteur was very anxious about his discovery. If the germ could get through numerous transformations thanks to the medium, in conformity with a Lamarckian point of view, this basic condition of immunity, (the so-called transformation of the organism) could lead to a very difficult position: any germ could happen to be once a saprophyte, once a pathogen, and the laws of disease would have to cope with permanent metamorphoses. He wrote in 1880 in an unpublished note:

Progressive modification of an organism showing itself in continuous change in virulence that could equally well be explained as an effort of multiplication of the organism in the animal body, is so much outside all we know... that I have resisted accepting its implications for more than one year. A completely different hypothesis is consistent with the facts. Could there not be two cholera viruses, one virulent and always fatal and one very much attenuated, the mixture of which in different proportions would give all possible virulences? (L. Pasteur: Oeuvres, Tome VII, Masson, Paris, 1922–1939, p. 52.)

To reinforce the bacteriological side of specificity, scientists had to restrict the spectrum of changes on the side of the pathogens to the changes one needed to endow man with the power of vaccination. On the other hand,
they had to restrict the spectrum of possibilities on the host’s side. They
avoided emphasizing the variety of the host’s response to infection from
latent forms to mild attacks and acute cases, and stressed the importance
of positive cultures that were the link between all accounts of the disease, so
that a clear-cut meaning could appear. The idea of the extreme variability of
the host’s response was pushed away and so prevailed the idea of a parallelism
between the disease in vivo and bacterial cultures in vitro. A System of
Bacteriology in Relation to Medicine was the title of a collective book,
sponsored by the English Medical Research Council in London, three years
before Fleck’s book was published.6

THE MEANING OF SEROLOGY

When the first serologies began to be investigated, it meant that the events of
disease were supposed to be grossly reproduced on the slide. But there were
still two available models: one was non-specific, it was phagocytosis;5 the
second one was specific, it was antibodies who played the leading part, and
the disease could be compared to a battle raging between bacteria and anti-
obodies Clemens Von Pirquet illustrates the parallelism:

Genau so wie der Niederschlag auftritt, wenn wir in vitro zu dem antikörperhaltigen
Serum einen Tropfen Pferdeserum zufliessen lassen, so tritt das spezifische Oedem, die
sofortige Reaktion dann ein, wenn ein Antikörperhaltiges Individuum mit Pferdeserum
beschiickt wird.

Die Reaktion in vivo ist ebenso spezifisch, wie die Reaktion in vitro. (Die Serum-
Krankheit, Franz Deuticke, Wien, 1905, p. 113.)

The first test of 'complement-fixation' was performed by Bordet and Gengou,
without any reference to its diagnostic usefulness, but very quickly this sort of
test was tried out for various diseases: tuberculosis, typhoid fever, and
even rabies, varicella and smallpox. This procedure could be considered more
reliable than clinical assessments. Numerous authors emphasized the safety of
such specific tests.

What happened in fact was that, when the Wassermann test was performed
as a routine test, it did appear immediately that the rate of actual cases of
syphilis was that much higher than foreseen, and this worrying discovery was
not immediately questioned, since tradition had long ascribed all sorts of
puzzling symptoms to syphilis.

The ideal of specific answers to specific questions was however challenged
by some people. Even before Wassermann’s paper, Bordet had suspected the
specificity of his own complement-fixation reaction. After the first two papers by Wassermann and his collaborators, Landsteiner himself promptly suggested the existence of cross-reactions with trypanosoma infections. And the same was suggested for typhus, fever, piroplasmosis and even, though in a controversial manner, some forms of cancer.6

The ideal of specific serology as a tool for diagnosis and adequate treatment had been clearly put forth by Wassermann himself:

It would be of the greatest diagnostic and therapeutic value if it would be possible to prove regularly the presence of syphilitic material or antibodies in the circulating blood of syphilitic patients.

[Von der grössten diagnostischen und therapeutischen Bedeutung wäre es, wenn es auch gelänge, regelmäßig den Nachweis syphilitischer Stoffe oder Antikörper im kreisenden Blute Lueskranker zu führen. (Deutsche Medizinische Wochenschrift 32 (1906), 746.)]

According to Fleck, Wassermann’s first research stood only on the bacteriological side: Wassermann would have exclusively searched for syphilitic antigens in the blood or the cerebrospinal fluid of the infected apes. But Fleck was unfair to Wassermann: the reaction was, from the beginning, two-sided. Wassermann tried to detect either syphilitic antigens with antisyphilitic sera or antisyphilitic sera with syphilitic antigens. Only this second reaction stood the test of time, and the success of the complement-fixation test can mark the turning-point from bacteriology to immunology; but its heralds were not aware of this fact, nor of the switch to a fresh point of view.

But this switch was hindered by the inability of immunology to demonstrate its object, namely antibodies. The concept of antibodies that was gaining ground was widely inconsistent. Besides, some peculiarities of Wassermann’s reaction were very annoying: the unknown nature of both substances interacting, antigen and antibody. Several theories were competing, suggesting contradictory images as guides for the understanding of biological phenomena:

— the well-known theory of lateral chains (Ehrlich’s Seitenketten theory) attributed strict specificity, namely chemical-like specificity, to antigens and antibodies,

— an alternative theory claimed that antibodies were never so specific and had always a very wide spectrum of affinities with various substances.

Maybe under Fleck’s influence, one frequently talks about two styles in the first phase of immunology.7 It has become classical to oppose Ehrlich to Metchnikoff, Landsteiner to Ehrlich, Ehrlich to Gruber. But things were not so simple. In a first sketch, one can choose to oppose ‘two antithetic styles’, as far as immunological specificity is concerned (antigen or bacteria-antibody
interaction). Ehrlich and Bordet are fairly good representatives of both extreme positions.

**Ehrlich**

*Strict specificity.*

Specificity is a *chemical* matter (stereochemical approach).

**Bordet**

Specificity is better viewed as a *continuum* (all possible degrees).

Antigen-antibody interaction is a phenomenon of *adsorption*.

But if one attempts a more realistic picture, one has very quickly to draw a more complex sketch. It is better to try to pinpoint everybody on a continuous line: Ehrlich’s friend, Arrhenius, is more sensitive to physical laws ruling antigens and antibodies and not so keen on strict specificity, Landsteiner, after his chemical studies of haptons, studied all kinds of cross-reacting chemical specificities. ... Ehrlich Arrhenius Landsteiner Bordet Fleck. ... From *strict specificity* to *Non-specificity*. Fleck is at one end, because he did not believe that chemical specificity could be demonstrated in the future, differing from Bordet in this respect.

Among people who pleaded strongly for a widened specificity there were people who explained interaction between antigens and antibodies referring to the theories of the ‘colloidal state’. There were supposed to be special chemical laws ruling macromolecules which explain immunological reactions. Landsteiner was once strongly attracted by these theories, and so was Fleck, ten years later.

Boris Zalé stresses the point that the complement-fixation test may involve *either cell antigens* or soluble antigens, which is unusual with serologies (agglutination concerns cell antigens, precipitation concerns soluble antigens, and so on ...). The point is important because, nearly at the end of his career, Landsteiner suggested* that these two sorts of antigens behaved in a different way: results with agglutinins suggested the idea of clear-cut specificities, while results with precipitins favoured strongly the adverse principle that the reaction varies according to a *continuum* (*Quantitative Abstufung*). The complement-fixation test is therefore a crucial issue, because it bridges both aspects of immunity, chemical and cellular and summons holders of contradictory theories to compromise.

Specificity means that some effects are strictly restricted. It is biological causality related to species. But species belong to the tree of life. It means that two conceptions of specificity are possible:
— specificity is absolute, but how did it happen that species grew apart from the main trunk, without keeping some characteristics in common?
— specificity is relative: it is correct to interpret evolution, but it leaves other issues doubtful. For instance, infection by selected bacteria will provide man with an overlapping immunity (argument of economy, contradicted by the multiplicity of substances in Ehrlich's theory) but all trials of specific immunization will hardly be able to avoid negative side-effects.

The choice of a style of thought, as far as specificity is concerned, does not merely determine the truth-value of statements, for it yields some hints of the world itself. Specificity is the key to descriptive language and to action as well. It determines logical truth and efficiency. This mixed nature was well perceived by Fleck.

There was a major difficulty in trying to close the debate on specificity at the time. Neither theory of immunity could present substances to strengthen its point of view. Were antibodies proteins or lipoids plus proteins (Forssmann) or small radicals on proteins (Wells), globulins or not-globulins? The chief materialization of antibodies lay in a system of related images (key and lock, according to Fischer and Ehrlich's suggestive way of depicting antigen and antibody, dye for Bordet, changing patterns of electric charges for Wells). Antibodies displayed a puzzling variety of names: agglutinins, immune bodies, amboceptors, sensibilisatrices . . .).

THE WASSERMANN CLUE

The Wassermann's reaction is often described in a glorious triad:
1905: Schaudinn evidences Treponema pallidum, once Spirochaeta pallidum;
1906: Wassermann first describes his test;
1907: Ehrlich begins to investigate 'Salvarsan' that will be tested on humans after 1909.

But we can just as well describe a negative triad:
(1) 1905: trials of culture, except Noguchi's, were a failure;
(2) All attempts at vaccination were a serious disappointment.
(3) 1905: it was still discussed whether syphilis could not be self-cured. Boeck was undertaking an 'experimental study of nature' in Oslo. Serology for syphilis was being investigated in this negative context. As cultures were impossible, some ersatz had to be found.

The Wassermann reaction was quickly tested after 1909–1910 in England and in the United States. Though some physicians had condemned it as
'rubbish', the serology provided country-wide investigations of the scourge. Fleck rightly emphasized the importance of technical devices and of lab skill. An enormous amount of papers were published, suggesting slight changes in the procedure: temperature, animal source of complement, time of incubation. 

.... The principle was laid down *that it must work*. Detre, for instance, who described the reaction with human sera (before Wassermann) only obtained two positive results out of six! One consequence of this state of mind is that, still today, we say 'false positives' when speaking about positive reactions (and well positive!) that perhaps do not belong to syphilitic patients. And conversely, 'false negatives,' which suggests a sort of disappointment for the physician. That is a first notice of two different medical styles, in Wassermann's time and nowadays. Wassermann's contemporaries discovered a great number of syphilitic cases which probably were not actually syphilis: it all depended on the choice of rate-limits for serology and of control-subjects whose importance was stressed by Fleck. One of the most famous specialists for syphilis, Kolmer, said in 1956:

... I have always been dedicated to the principle that an occasional falsely negative reaction in syphilis is to be preferred to unnecessary falsely positive reactions in normal nonsyphilitic persons.11

Fifty years before, they clearly preferred to choose the positive side of it. Let us compare this with a text of 1909:

Bien entendu, le sérodiagnostic n'est démonstratif que s'il est nettement positif. L'absence de sér-o-réaction positive n'a aucune valeur. (C. Levaditi et J. Roche: *La syphilis*, Paris, 1909, p. 129.)

Results were clearly improved by the tenacious social wish to ensure the detection of socially dangerous subjects.12

The contrast between a non-specific reaction and the would-be specificity of its use was made obvious by Fleck:

- there was no more specific antigen; an antigenic function was attributed to a pseudoantigen, a lipoid, which was contradictory to the belief: *Kein Antigen ohne Biiweiss.*

- there was no more specific antibody, maybe no antibody at all; the reacting substance was called a *reagin* and it was discussed whether reagins were antibodies.

Some biologists thought that the reaction was characterized by its *instability* and its variability and involved substances whose specificity was
highly dubious. Influenced by colloidal chemistry, these biologists came to state that the reaction was

liée au plus haut point aux dimensions des particules en suspension. . . . L'antigène lipoidique actif semble consister en un mélange de particules dont les dimensions varient de celles d'une suspension grossière à celle d'une dispersion colloïdale vraie, avec une zone intermédiaire relativement étendue qui confère à ces antigènes leur manque de stabilité caractéristique et apparemment essentielle. (M. G. Wells: Les aspects chimiques de l'immunité, Doin, Paris, 1928, p. 265; first English edition Chemical Catalog Co., New York, 1924.)

Thus syphilitic sera were assumed to be wholly modified and easily flocculated when mixed with a great number of lipoid substances. Fleck emphasized two points and amalgamated them:

- the claim of wide specificity versus strict specificity (slightly obsolete in his time) either in physical or chemical terms;
- the claim of the holistic approach of pathological facts versus the elementary [analytic] one.

The fact that the myth of specificity remained so strong in medicine indicated to Fleck the overwhelming influence of social interests and the persistence of outdated theories.

FLECK'S VIEW OF MEDICINE

Fleck challenges this oversimplified way of making diagnoses and reminds us that diagnosis is never safely founded on a single piece of evidence however specific it may be, but on a sum of elements and probably a pattern of elements (but he does not use this word). This type of approach could be named structuralist and reminds the reader of Goldstein's Der Aufbau des Organismus (though Fleck, by the way, was most likely unaware of his work). The physician's mind and the patient's body are two interacting continua or two series of phenomena whose interferences creates various sorts of diagnosis, like conceptual and phonemic series in Saussure's work. But Fleck's attempt is more empirical and reminds us more closely of a famous author whom Fleck read and deeply enjoyed, Landsteiner.

Fleck gave up the idea of specific elements of diagnosis and only attributed specificity to the whole; his theory of diagnosis is quite similar to the tenets of the 'colloidal theory'; those who supported the theory of the colloidal state dismissed the idea of specific antibodies in Wassermann's reaction. For Landsteiner, immunological specificity never belonged to a single chemical element, but to a continuum of chemical molecules, a family of bodies and
finally to a sum of elements (haptene plus carrier) none of which could be strictly said to be specific.

Fleck actually emphasized that medical language cannot be a mere translation of biological phenomena and showed that biological and medical dialects exchange meanings to strengthen their mutual points of view. It is clear in Bordet’s work that if serology demonstrates the presence of the disease, conversely the realization of serology makes a case for the germ theory of disease:


Fleck strongly suggests, more than he properly demonstrates, that medical discourse is an ‘impure one’, with various interferences from intersecting worlds, the religious one, the political one, and so on. . . . There is a gap between medicine and the ideal of a specific cause, exactly as Tiselius said that there is a gap between biology and chemistry, for biology is essentially the study of impure substances, that means functional substances. Medicine is never adequate to its object, because its knowledge is, most of the time, an addition of more or less relevant data. Modern authors still emphasize that there are so many factors of disease that synthesis is very problematic. A non-exhaustive list of such items would include
discoveries relating to viruses . . . research on the immune system . . . genetic factors . . . the role of individual idiosyncracy . . . the plain stubbornness of certain human beings . . . ecological data . . . . . . .13

For Fleck, all previously appreciated specificities, clinical specificity (the so-called pathognomonic symptom), bacteriological specificity, are open to criticism, but they remain all the same legitimately part of medical discourse, even if serum reactions should never be regarded as substitutes for clinical skill. The epistemologist of medicine can make out successive layers of historical meaning in medical discourse.

**FLECK’S MISTAKE**

Fleck eagerly chose the example of the Wassermann reaction and was mistaken. He chose this example because he thought that it would show the inadequacy of a medical language based only on specificity. And the case of
Wassermann's reaction seemed to him to show convincingly that a reaction between two non-specific substances could be wrongly interpreted in a frame of reference focused on specificity and for the sake of society. He thought that, besides this model of specific interactions, there was room for an alternative model of a wide spectrum of affinities.

Thus he assessed an interesting thesis, formulated along the lines of analogy with an outdated theory, and a wrong one; the Wassermann test was going to be interpreted in the context of chemical specificity, and the physical laws of lipids were going to explain its 'essential' variability.

CONCLUSION: WHICH 'STYLE' WAS FLECK ADVOCATING?

Fleck's weak point is that he did not clearly explain much about his holistic approach to medicine. The switch from the reductionist bacteriological point of view could suggest the switch of bacteriology to cellular immunology and of course not to the neo-reductionist immunochemistry of the thirties, but rather to the science of the functional immune system that was still to come. Stressing the function rather than the material substratum and the holistic prospect more than the atomistic one, the modern concept of immune system would have appealed to Fleck. It tries to syncretize personal history and external events, heredity and hazard. It may be viewed as a complex scheme, composed of heterogeneous elements.

But it is not clear whether the notion of style of thought can be clearly applied to the debate between two types of interpretation of phenomena:

- the first is an attempt to build the unknown with the known;
- the second is an attempt to build the known with unknown elements.

Fleck's notion of style of thought is ambiguous and heterogeneous like the models he preferred. It is altogether an historical and an epistemological concept. The two types of thought he described may be complementary, and they are exclusive only because of the resistance of people to changing their points of view. These two approaches are uneasily compared because they involve different prerequisites and different systems of references.

Fleck was unable to safely choose a point of view from which to compare both types of approach, and from a methodological point of view of non-specificity could not seriously discuss specificity without a logical 'tour de force'.
NOTES AND REFERENCES

1 So far, the word 'specificity' was frequently used by some authors for 'disease': for instance, "La seconde observation concerne une femme qui niait toute spécificité. . . . On désire être fixé sur la spécificité du rejeton." (C. Levaditi et J. Roche: La syphilis, Paris, 1909, pp. 127–129.)

2 The Lamarckian point of view on evolution was very strong at the time among Pasteur's colleagues. On attempts to present the attenuation of virus as a Lamarckian procedure, see A.-M. Moulin: Pasteurisme et Lamarckisme, Bull. Institut Pasteur, Paris, 1985, in press.

3 To compare with the bold text of the Comptes rendus de l'Académie des Sciences: "J'ai passé sous silence une question ardue dont l'étude m'a pris un temps considérable. Je m'étais persuadé, à vrai dire je ne sais pourquoi, que tous les faits d'atténuation que j'observais s'expliqueraient d'une manière plus conforme aux lois naturelles dans l'hypothèse de mélanges en proportions variables et déterminées, de deux virus, l'un très virulent, l'autre très atténué, que par l'existence d'un virus à virulence progressivement variable. Après m'être, pour ainsi dire, acharné à la recherche d'une démonstration expérimentale de cette hypothèse de seuls virus, j'ai fini par acquérir la conviction que ce n'était pas la vérité." (L. Pasteur: C. R. Acad. Sci. 1880, in Oeuvres, Tome VI, p. 331.) For the debate on attenuated virus, cf. P. Mazumdar: Landsteiner and the Problem of Species, Johns Hopkins University Press, Baltimore, 1976, part I.

4 A System of Bacteriology in Relation to Medicine, Medical Research Council, London, 1931, Vol. VI.

5 Phagocytosis referred to the ability of white blood cells to engulf any sort of particle.


Pauline Mazumdar has very nicely focused her main work on Landsteiner and specificity, cf. op. cit.


10 The first paper written in the USA was an editorial in the JAMA of 1907. (H. Hecht: 'Half a Century of a Serodiagnosis of Syphilis', Archives of Dermatology 4 (1956), 433.) In the latter part of 1909, there were only three English papers on the subject of the Wassermann test (L. W. Harrison: 'Half a Life-time in the Management of Venereal Diseases, op. cit., p. 444.)


12 The famous syphilitologist Alfred Fournier suggested the term 'parasyphilis' for late manifestations known as neurosyphilis: for him, they were not caused by the syphilitic 'virus' since they were resistant to mercury. The Wassermann reaction linked parasyphilis, primary lesions and latent syphilis in the newborn, before Noguchi demonstrated in 1913 active treponema in the brains of general paralytics.


THE EPistemology of the science of an
Epistemologist of the sciences: LUDwIK FLECK's
Professional outlook and its relationship to
HIS PHILOSOPHICAL WORKS

The book *Genesis and Development of a Scientific Fact*, written by Ludwik Fleck in the early thirties and which remained practically unknown for a long period, is considered today, following its recent rediscovery (mainly in relationship with T. S. Kuhn's book *The Structure of Scientific Revolutions*) to be an important pioneering work in the sociology and in the epistemology of the natural sciences. Fleck was the first to developed in detail the view that in the natural science facts do not derive automatically from the observation of nature, but are socially constructed and, as such, dependent on the sociocultural context, the 'thought-collectives' in which they evolved.

How did a microbiologist and immunologist, living in the relatively provincial city of Lwów (which belonged to Poland in the period Fleck's book was written) develop a highly original approach to the philosophy of the natural sciences and a strongly relativistic attitude towards 'scientific facts'?

In his Ph.D. thesis¹ and in subsequent works,² Thomas Schnelle maintained the view that Fleck's theories, and his relativism, were developed in the context of the Lwów philosophical school and were related to the thinking of its major representatives, Adjakiewicz, Twardowski and Chwistek.³

In this article, we want to propose a different, ultimately complementary, explanation of the origins of Fleck's relativist approach: his vision of science and his attitude toward nature, both deriving from his professional experience. We will try to demonstrate the existence of a relationship between Fleck's vision of medicine and more particularly of his own subject-matter, medical microbiology and immunology, and his more general views concerning the origins of scientific knowledge. Our analysis will be mainly based on study of examples which are abundant in Fleck's writings, referring to his scientific discipline.⁴

Fleck was trained as a physician. He specialized in laboratory work in medical bacteriology and immunology, and his principal professional preoccupation was the application of these two disciplines to a medical practice.⁵

The first article written by Fleck dealing with an epistemological problem was published in 1928. His book *Genesis and Development of a Scientific

Fact was published in 1935. Fleck also wrote several articles in the period 1934–1939: those develop similar ideas to the ones found in his book, and represent, together with it, the essential core of his epistemological reflections. Thus we can assume that the period in which Fleck’s theories matured is situated between the mid-20’s and the mid-30’s.

The period in which Fleck’s epistemological research evolved was characterized by a certain crisis in his profession of medical immunology. Immunology was from its beginning a discipline with strong practical implications, but with some theoretical aspirations as well. Its birth was nearly simultaneous with the birth of modern medical microbiology. (It is not by chance that the two disciplines were often, as was the case for Fleck, practiced together.) By using techniques that were later named immunological, like serotherapy and vaccination (and later on vaccinotherapy and proteinotherapy), microbiology, at its beginnings mainly abstract knowledge, was transformed into an efficient therapeutic practice. Before the arrival of chemotherapy, immunologically derived procedures were considered as the most potent tools available to the physician to fight infectious diseases. During the first enthusiastic years that followed the great discoveries of Pasteur, Koch and von Behring, many believed that for every disease a microorganism responsible for it would be found; and subsequently, a way to fight the spread of the disease by vaccination and a cure by serotherapy.

The enthusiasm of the late 19th century was rapidly moderated by several disappointing findings: the efficiency of most of the vaccines and many of the immune sera was low. Serotherapy produced many accidents, some of them fatal. In parallel to the studies of the beneficial effects of serotherapy (prophylaxis), the studies of its potentially harmful effects (anaphylaxis) developed rapidly. The accessory immunological therapeutic techniques like vaccinotherapy and proteinotherapy although, in absence of other more efficient therapeutic means they were widely diffused, were of doubtful clinical benefit. Attempts to treat cancer by either non-specifically stimulating the defenses of the organism or by raising specific cytotoxic sera, supposedly toxic to the cancer cells, were all unsuccessful.

The disappointment concerning the eventual therapeutic applications of immunology affected the general orientation of this discipline and was probably among the reasons for the cleavage that appeared between its theoretical developments and practical applications.

Immunology at its beginnings was agitated by an important controversy: its protagonists were the ‘cellular’ school (headed by E. Metchnikoff) and the ‘humoral’ school (headed by P. Ehrlich). The names given to these schools and their usual description may be misleading. Their controversy was often
presented only as a discussion between those who were in favor of cellular mechanisms of immunity (mediated by macrophages) and those who believed in predominance of humoral immunity (mediated by antibodies in the serum). In fact this polemic not only was about the mechanism by which immunity is obtained but dealt also with the problem of specificity. While the 'cellular' school was interested in non-specific mechanisms of defense against bacterial infection, the 'humoral' school was interested in specific anti-infectious protection obtained by the mediation of humoral antibodies.

The issue of this controversy was usually presented as a victory of the 'humoral' approach and as a failure of the 'cellular' school. It is true that with the development of serology (studies of the antibodies in the serum, usually for diagnostic purposes), the importance of the humoral antibodies was clearly recognized. But this was only one aspect of the problem. The controversy between the 'cellular' and the 'humoral' approach reflected profound divergences in the general approach to immune phenomena. These divergences were finally crystallized in the two attitudes towards immunology that prevailed in the first half of the 20th century. The immunochemical approach which started with works of K. Landsteiner and S. Arrhenius was mainly interested in quantitative in vitro studies of the specific antibodies which appeared in the serum following an immunization with a well defined antigen. It concentrated upon the study of the solutions of antibodies outside the body, using classical chemical and biochemical methods. When it became more and more evident that specific antibodies are proteins, the preoccupations of immunochemists grew closer to those of biochemists and protein chemists. Although some of the principal ideas of immunochemists derived from those of Ehrlich, they retained the biochemical aspects of his work but neglected Ehrlich's concepts (sometimes quite confused, but rich in important intuitions) concerning the cells which produced antibodies. Ehrlich's theories were also influenced by his constant preoccupation with the medical utilizations of immunology, but the subsequent immunochemical studies were often more and more distant from any kind of clinical preoccupation.

The separation between the fundamental aspects of immunology and its clinical preoccupation was encouraged by the discovery that specific antibodies can be formed not only against bacteria and viruses, but also against so-called 'haptens', artificial chemical structures developed in the laboratory. This capacity of the organism to form antibodies against artificial structures could not be explained by any one of the existing theories which viewed immunity as a defense mechanism. The haptens became one of the favorite tools of immunochemists, contributing to the relative separation
between their preoccupations and those of the physicians; the immuno-
chemists gradually abandoned the vision of immunity as a defense system,
destined to protect the body against infections, but for a long time no alter-
native idea was proposed.

While the immunochemical approach evolved towards protein chemistry,
medical applications of immunology continued to rely on the theory and
practice developed at the end of the 19th century. No new ideas and no
new theories were developed, although many of the experimental data,
accumulated from the time those theories were formulated, could not be
explained by them. Immunology, in its medical applications, was viewed as
an auxiliary medical discipline which helps pathology in improving diagnosis
and which offers some possibilities of treatment. From a medical point
of view, immunity continued to be perceived, as in the 19th century, as
the whole complex of mechanisms, specific and nonspecific, humoral and
cellular, which together were responsible for the resistance of the body to
infectious disease\textsuperscript{19} (thus encompassing a much larger range of phenomena
than the immunochemical approach, interested only in specific humoral
antibodies accessible to an analysis in a test tube). This approach has the
undoubted merit of being closer to real pathological situations. But the
attempts at a global evaluation of the immune phenomena in the context of
a given disease were rarely successful, because of the great complexity of
these phenomena. As a consequence, medical immunology and immuno-
pathology remained, during the 20's and the 30's, mostly on a purely descrip-
tive level, succeeding however in developing, often empirically, some very
useful applications of serology. Among these, probably the most important
was the Wassermann reaction which was the subject of Fleck's book. The
physicians engaged in research in immunology remained in their great majority
attached to the old methodology and terminology, to empirical descriptions
and qualitative, rather than quantitative, evaluations. Faced with concrete
problems posed by concrete pathological situations, the clinically oriented
immunologists, unable to find adequate theoretical responses, often felt
alien to the reductionist quantitative approaches that predominated in
immunochemistry and, as a consequence, some lost interest in contemporary
advances of this discipline and were not aware of the progress made by it.
While the clinical immunologists could consider the research of the immuno-
chemists as irrelevant to real understanding of pathological phenomena, the
immunochemists looked often with a certain contempt on the research
carried out by the physicians. They considered its theoretical basis and its
methodology outdated, its experimental systems too complex to analyse
and its results either irreproducible or very difficult to analyse and to interpret in quantitative terms. Serologists were given credit for some practical achievements, like development of improved vaccines or diagnostic procedures, but they were denied the status of 'true' scientists.20

Ludwik Fleck was a physician and a serologist.21 During all his life, he remained faithful to an orientation which refused to give priority to the reductionist approach to infection and immunity. His vision remained always the vision of a medical practitioner to whom the subject-matter of immunology was the study of all existent defense mechanisms of the organism involved in its reaction to infectious disease or playing a role in other pathological situations.22 He preferred the direct observation of pathological phenomena as a method of study of immunological reactions, this being reflected in the subjects of his early scientific works, published in the 20's and 30's.23

In both his general attitude toward biological sciences and in his personal research work, Fleck remained faithful to a global (i.e. for him medical) way of thinking, refusing the reductionist (i.e. chemical) approaches. He was convinced that these two approaches were mutually exclusive and fundamentally incompatible:

The conflict between closely allied thought styles makes their coexistence within the individual impossible . . . It happens more frequently that a physician simultaneously pursues studies of a disease from a clinical-medical viewpoint together with that of history of civilization than from clinical-medical or bacteriological one together with a purely chemical one.24 (p. 111)

We will try to demonstrate that while Fleck never questioned his faithfulness to the clinical-medical approach to immunology in the period he developed his epistemological views, he was sharply aware of the limitations of the official knowledge accepted in his time as the theoretical basis of this discipline. He felt the inadequacy of the knowledge inherited from Pasteur and Koch to explain more recent findings and to account for the multiple new phenomena described since the end of the 19th century. Fleck thus contested the validity of an important portion of this 'official' knowledge in immunology and in medical bacteriology, considering it as oversimplified and unable to account for the complexity of the observed phenomena. He felt that this inadequacy of the accepted knowledge could not last and that the fundamental concepts of immunology and of bacteriology, considered as absolute and general truths, should be (and probably would be) soon profoundly modified.25 He also tried to figure out what the future trends would
be. We propose that the particular vision that Fleck developed concerning his own discipline affected his global vision of scientific research. The personal confrontation with ‘established facts’ of bacteriology and immunology eventually led him to broader epistemological reflections about the social mechanisms leading to general acceptance of ‘scientific truth’. Fleck’s conviction that many important ‘facts’ in his discipline were bound to change in the near future also contributed to encouraging his general relativistic approach to science.

The first known epistemological work of Fleck is his article ‘O niektórych swoistych cechach myslenia lekarskiego’ (‘On Some Particular Characteristics of Medical Thinking’)26

In this article, Fleck polemicizes, sometimes quite violently, with those who believe in purely ‘scientific’, quantitative and logical medicine. He starts his argument with the affirmation that diseases do not represent a reality but a model; they are “ideal, fictitious pictures which are called disease units”. Moreover, his perception of disease is holistic; he is clearly opposed to a purely etiological vision of diseases.27 “Even the typhoid bacillus cultivated from feces does not prove that the given individual suffers from typhoid fever; the individual may be only the germ carrier. It is only the combinations of symptoms, the habitus, the entire status praesens of the patient that is conclusive, why even the best diagnosticians are most frequently unable to give a specific basis for their diagnosis.”

Medicine, aspiring to a scientific status, tries to organize and to classify its knowledge; this process increases the distance between practice and theoretical knowledge: in medicine, “the fictitiousness of the theoretical knowledge is much greater than in any other scientific discipline. The result is the divergence between theory and practice, so characteristic of medical science.” Fleck believes that attempts at purely logical motivations of medical acts are meaningless: “It is nowhere easier to get such a pseudologial explanation than in medicine because the more complex the set of phenomena the easier it is to get a law verifiable for a short term.”

In medicine, “the worse the physician the ‘more logical’ his therapy. The point is that, in medicine, one is able to simulate almost everything, which proves that, up to now, we have indeed failed to explain anything.” Fleck refused to accept simple causality as a valid principle in medical practice: “The result is never proportional to the cause of is it always the same.” The general result is the impossibility of a purely rational approach to pathological phenomena, their incommensurability and the inability of any single theory to explain pathological states. “Neither cellular nor humoral theory, nor
functional understanding of deseases alone . . . will ever exhaust the entire wealth of morbid phenomena.”

Fleck’s conclusion is that the medical way of thinking cannot find any kind of consistent and rational approach toward a global understanding of pathological phenomena.27b

In a later article, Nauka i Środowisko (‘Science and Environment’ (1939))28, Fleck pursued his ideas about the superiority of the empirical attitudes over the supposedly more scientific theoretical generalizations.

Science became a product which globally is not by any means simpler than nature; moreover, it is definitively less accessible. It is easier to find one’s way in a forest than in botany. It is easier to cure a patient than to really know what his disease is.

In his later works, Fleck generalized some of his relativistic ideas concerning the impossibility of objective, socially unconditioned independent scientific observations, ideas already present in his first epistemological articles and developed in detail in his second epistemological article ‘On the Crisis of “Reality”’ (1929)29. He continued to develop this idea in his further works. But, in parallel, in practically all his philosophical writings, he also continued his attacks (begun with the article cited above on medical thinking) on the adepts of the purely ‘scientific’ (for him, reductionist) approach to medicine.

His opposition to the exaggerated attempts to use purely quantitative evaluation in medical practice and the contradiction between such an approach and common sense in treating disease is illustrated by Fleck in an example quoted in his second article ‘On the Crisis of “Reality”’. In this article, he explains that, in a medical practice, it would be meaningless to stick to the same stringent criteria, in this case of classification of pathogenic bacteria (streptococci), according to their bacteriological properties (capacity of hemolysis) that are used successfully in scientific research. In the same article, Fleck also advances the idea (prefiguring the principal theme of his book) that “even the Wassermann reaction, so clearly worked out and frequently applied, is ultimately an art”.

In his magnum opus, the book Genesis and Development of a Scientific Fact and in the articles written in the same period and expressing similar ideas, Fleck’s general argumentation is supported with detailed studies of several examples, in their majority taken from his discipline, medicine, and related directly to his specialization, medical immunology.

His book is centered on the history of a disease, syphilis, and on the history of the blood test designed to detect it, the Wassermann reaction.
Fleck's comment on the history of syphilis is strongly influenced by his refusal to accept uncritically the predominant 'scientific truth': the etiological definition of this disease (syphilis equals "disease provoked by the microorganism Treponema pallidum") 30 as the definitive solution of the problem and as the only possible and valid approach to the study of this disease. In showing the varying views of syphilis through history, and in refusing to recognize one specific view as being more 'true' than the others, Fleck repeats the argument already present in his first article about the fundamental impossibility of reducing the pathological phenomena to one definite approach, thus again contesting the reductionist approach of 'scientific medicine'.

In his book, Fleck makes explicit his non-reductionist views: thus, on the infectious diseases:

the idea of the causative agent has lost the overriding importance it enjoyed during the classical period of bacteriology . . . Today it can be claimed almost with impunity that the 'causative agent' is but one symptom, and not even the more important among several indicative of a disease. (p. 18). It has been explained that the etiological concept of disease is not the only possible one . . . Nevertheless contemporary scientists, or most of them, are constrained by this concept and cannot think in any different way (p. 122)
The so-called diagnosis -- the fitting of a result into a system of distinct disease entities -- is the goal, and this assumes that such entities actually exist, and that they are accessible to the analytical method (p. 64) . . . there is often no convergence between pathology and bacteriology . . . (p. 18).

Fleck discusses the origins of the Wassermann reaction, considered to be one of the most important achievements of the application of immunological knowledge to medical practice. "The Wassermann reaction also created and developed a discipline of its own: serology as a science in its own right . . . The Wassermann reaction is often referred to simply as the 'serological test' " (p. 14). Fleck's argument is that it was the popular belief in the existence of 'syphilitic blood' that was the most important stimulus to the development of a blood test to detect syphilis. This argument is intended to demystify one of the principal claims of 'scientific medicine', its demarcation from 'superstition' and its intellectual superiority over popular medicine and popular beliefs in general 31. "The discovery -- or the invention -- of the Wassermann reaction occurred during a unique historical process which can be neither reproduced by experiment nor confirmed by logic" (p. 97). "The old idea about blood and the new idea of complement-fixation merge in a convergent development with chemical ideas and with habits they induce to create a fixed point." (p. 79)

Fleck's argument concerning the relativity of 'scientific facts' largely rests on his analysis of two phenomena (the examples are taken again from his
professional experience): variability of bacteria and specificity of antibodies.

In order to understand his position on these questions, we should situate them in the context of Fleck’s general outlook concerning biology. Fleck denied the ‘old’ mechanistic approach to life phenomena; he considered the reductionist approach outdated and condemned to disappear soon, to be replaced by a holistic view. According to Fleck,

An organism can no longer be construed as a self-contained independent unit with fixed boundaries . . . that concept became much more abstract and fictitious . . . For the morphologist, it has changed into the concept of genotype as the abstract and fictitious result of hereditary factors (p. 60).

The rigid concept of organism should therefore be replaced by the concept of ‘harmonious life-unit’. His rejection of the rigid, strictly etiological concept of an infectious disease can be integrated in the context of this general outlook; Fleck refuses to view the disease solely as the result of the invasion of the body by a microorganism and proposes to define it instead as “a complicated revolution within the complex life-unit . . .” (p. 61)

Fleck’s preoccupation with the problem of bacterial variability is directly connected with his rejection of etiological definition of infectious disease. If the bacteria are highly variable and do not form fixed species, it is of course senseless to classify the infectious diseases according to the classification of their respective etiological agents, the bacteria. Thus for Fleck the discovery of bacterial variability was an important argument supporting his view of pathological phenomena as complex events, not reducible to one single type of explanation.

The phenomenon of bacterial variability was not noticed by the founders of microbiology while in Fleck’s time this phenomenon was readily observable in similar conditions by the bacteriologists. Fleck uses this example to demonstrate that an absolutely objective observation independent of existing theories and preconceived ideas is impossible. At the same time he uses this example also to affirm and amplify his doubts about the existence of true species among the bacteria. “Species were fixed because a fixed and restricted method was applied to the investigation . . . with a less fixed method, phenomena of variability were perceived, which were not noticed before” (p. 19). And if the bacterial species are not fixed, the definition of infectious disease should be accordingly modified:

Unpredictable fluctuations in virulence, such as transformations of the saprophytes into parasites and vice-versa, altogether destroy the relation which initially appeared simple between a given type of bacterium and its associated disease (p. 19).
What are the 'unpredictable fluctuations' mentioned by Fleck? In the 30's, under the general headline 'bacterial variability', many unrelated phenomena were gathered, some of them involving permanent hereditary changes such as mutations and clonal selection, some, like the integration of a bacteriophage (bacterial virus), were reversible hereditary modifications and some of them did not at all imply hereditary changes and were the expression of temporary modification of the individual bacterium reacting to its environment (this was the case of induction of adaptable enzymes by growth in different nutritive media) 36.

Among the various types of bacterial variability, Fleck was probably particularly interested by the 'bacterial transformation'; an artificially induced change from the virulent form of a given bacterium to its non-virulent form, and vice-versa, 37 "transformation of saprophytes into parasites". It was for him one of the most convincing indications that the concept of species is not valid for bacteria 38. Fleck believed that bacteriology and immunology are on the verge of revolutionary changes in concepts. This revolution which, according to Fleck, had already started although many scientists do not yet realize it, will derive from the concept of bacterial variability. Fleck considered the future changes in his discipline as important as those brought to modern physics by the theory of relativity; he drew a direct parallel between the abolition of the old mechanistic concepts in physics and the abolition, foreseen by him, of the 'old', 'mechanistic' concepts in biology.

The transformation in physics and in its thought style brought about by relativity theory represents such a mutation, as does the adjustment in bacteriology resulting from the theories of variability and cyclogeny. Suddenly we no longer see clearly what is species and what is individual, or how broadly the concept of life cycle is to be taken. What just a few years ago was regarded as a natural event appears to us today as a complex of artefacts. Soon we shall no longer be able to say even whether Koch's theory is correct or not 39, because new concepts incongruent with Koch's will arise from the present confusion. (p. 26)

In order to understand more clearly the ideas expressed by Fleck in this paragraph, we can refer to an earlier article written by him for a Polish medical review and designed to review the subject of bacterial variability. In his article 'O pojęciu gatunku w bacteriologii' ('On the Concept of Species in Bacteriology') 40, Fleck affirms that "bacteriology does not have, until now, a clear-cut definition of species. There are variants, forms, types . . . , the optimists speak about genotypes, mutations, in fact abusing those definitions." After a detailed survey of the known types of bacterial variations,
Fleck expounds his preferred explanations of these phenomena. He prefers those approaches that considered the bacteria observed in pathological situations and in the laboratory either as highly modified, probably degenerated, forms of “wild type” bacteria or as a single stage in a complicated life cycle. (This is the cyclogenic theory to which he makes allusion in his book. This theory supposed that bacteria are in fact complex fungi-like organisms, able to form multicellular structures in some life stages. The simple forms that are usually observed are, according to the cyclogenic theory, the result of adaptation to parasitic life, adaptation which is temporary and can be reversed if the conditions of life of the microorganism change). Fleck tends to agree with many of the suppositions of the cyclogenic theory (as its representatives he quotes Alquist, Löhnis and Enderlein) and particularly with the aspect stressing the artefactual character of all present bacteriological knowledge. Fleck believes however that the cyclogenic theory does not account sufficiently for the variability and plasticity of the bacteria (the cyclogenic theory postulates relatively fixed and rigid life-cycles): and he proposes to enrich this theory with the notion (inspired by theoretical works of the Polish bacteriologist Kuczyński) of the extremely large possibilities of variability of bacteria under the influence of their interactions with the host organism, completely changing characteristics when grown on an artificial medium in a laboratory. “Cultivating a bacterium from a patient organism is not, as it is explained in the classical bacteriology, an isolation obtaining a pure culture, but an artificial birth of an organism, a transformation of a bacterium into a saprophytic variant.”

This vision of bacteria as complex organisms, passing through complicated stages of their life-cycles and able to change completely their morphological and physiological characteristics within a rich and complex interaction with the host organism, is compatible with Fleck’s general views concerning the nature of life.

The anti-reductionist concepts of Fleck and his general outlook concerning the phenomena of life are also reflected in his attitude to the second scientific problem discussed in his book: the specificity of antibodies. The theme of his principal work was the history of the Wassermann reaction. The Wassermann reaction could be chosen as an example because of its great popularity, and because Fleck as a practicing serologist was well acquainted with it; moreover he himself conducted research designed to technically approve this reaction.41

We consider probable however that the example of the Wassermann reaction was chosen mainly because its particularities were well adapted
to fit Fleck’s general views concerning serological reactions. Usually the serological reactions are designed to detect the presence of specific antibodies in the serum, and in medical practice the presence of specific antibodies in a patient’s serum helps to establish the exact diagnosis or, as is the case for syphilis, to reveal hidden illness. In order to establish the presence of specific antibodies in the patient’s serum, this serum has to react with the relevant specific antigen, which in the case of an infectious disease, is the microorganism which provoked this disease. In syphilis, the situation is slightly complicated by the fact that the etiological agent of this disease, the Treponema pallidum, cannot be cultivated outside a living organism (and only man and some primates are susceptible to it). Wassermann, in the first versions of his test, used as antigen the extracts of Treponema-infected monkey tissues. As Fleck related with many details in his book, those first experiments were more or less successful: the serum from a patient suffering from syphilis usually reacted positively with this extract. Subsequently however, it was found that if, instead of the extracts of the Treponema-infected tissues, one used extracts of normal, non-infected tissues, those extracts were as efficient as the ones prepared from infected tissues in order to distinguish between normal and syphilitic serum: only the syphilitic serum reacted with both types of tissue extracts. Finally, after further elaboration of the standardized conditions of the test, it was the easily available normal tissue extract that was retained for the massive routine testing. Thus a paradoxical situation was created in which, although the test was specific for the disease, the antigen was not.

Fleck believed that this peculiarity of the Wassermann test can be seen as a proof of the inexactitude of the predominant theories concerning the specificity of the antibodies. He took this reaction as an example to illustrate the inadequacy of the widely accepted notion that an immunization (or an infection) will induce the appearance, in the serum, of chemically defined substances which will react specifically with the inducing antigen (e.g. infectious agent), and only with it. Fleck refused the accepted view of an antibody as a specific defined substance.

After all, the Wassermann reaction proves only a special change in the syphilitic blood, and even today we do not know much more than this. (p. 74)

If an animal, for instance a rabbit, is inoculated, that is immunized with killed bacteria or with the blood of a different species, the serum of the animal in question (the immune serum) acquires the property of decomposing such bacteria or blood corpuscles. Serologists have, so to speak, materialized this property by giving the hypothetical, even ‘symbolic’ substance in the immune serum the name ‘bacteriolyin’ or ‘hemolysin’ (p. 65).
Fleck firmly rejected the accepted view that the antibodies are defined chemical substances (probably proteins) and preferred to consider the capacity to react with the antigen after immunization to be a global property of the whole serum rather than of some single type of molecule that can be isolated and studied separately; thus reminding us also of his view of the nature of antibodies as faithful to his general holistic and anti-reductionist outlook.46

In criticizing the 'old-fashioned' presentation of immunity, made by Wassermann's collaborator Citron in 1910, Fleck explained that many classical concepts of immunology were evolved during the period when under the influence of great chemical successes in physiology, misguided attempts were made to explain the whole, or almost the whole, of biology in terms of effects produced by chemically defined substances. Toxins, amboceptors and complements were treated as chemical entities, with such adversaries as antitoxins and anticomplements. This primitive scheme based upon activating and inhibitory substances is being progressively discarded in accordance with current physico-chemical and colloidal theories in other fields. (p. 62)

What was the 'new' theory Fleck opposed to the 'primitive scheme' that considered the observed biological effects as resulting from activity of chemically defined substances?

Fleck refers in this paragraph explicitly to 'colloidal chemistry'. This theory was largely accepted by biologists and biochemists in the beginning of the 20th century (and particularly in the teens and the early 20's). Colloidal chemistry asserted that biological macromolecules behave in a way that is fundamentally different from the one described for simple small molecules studies by chemists: the macromolecules obey special 'colloidal laws' and not the ordinary laws of chemistry. The previous vision of antigens and antibodies considered them as molecules with complementary chemical structures enabling them to interact specifically. In a famous graphic metaphor, Ehrlich compared the antigen-antibody reaction to 'key and lock'.47 This metaphor was rejected later as a gross simplification. For example, in Bordet's 1920 edition of Traité de l'immunité dans les maladies infectieuses48, Bordet (who was one of the first to adapt the claims of the 'colloidal theory' to immune phenomena) gave the following evaluation of Ehrlich's theory:

Elle a entravé la diffusion des idées concernant l'importance, pourtant considérable, des phénomènes d'adsorption dans les réactions d'immunité.49 Par l'abus qu'elle a fait des représentations graphiques assez puériles qui traduisent clairement l'aspect extérieur des phénomènes sans aucunement pénétrer dans leur intimité, elle a répandu le goût décivant des explications faciles mais illusoires. (p. 504)
The followers of this theory did not usually define themselves as vitalists; vitalism had, at the beginning of the 20th century, the reputation of a rather mystical theory, and was opposed to more materialistic approaches, while the colloidal chemistry claimed that living matter did obey concrete laws, but that these laws were different from the laws that govern inanimate matter. But a careful reading of the views of at least some of the supporters of the colloidal theory reveals an affirmation of the specificity and uniqueness of living matter, and stresses the differences between normal and 'colloidal' chemistry, in a way that can closely remind us of the vitalist views.

In the period of its maximal popularity, the colloidal theory was even adopted (although briefly) by immunologists like Landsteiner. The influence of 'colloidal chemistry' on biochemists was ended in the early twenties: one of the crucial events that influenced them (as simultaneously the immunologists who followed the major developments in biochemistry) was the publication of Jacques Loeb's *Proteins and the Theory of Colloidal Behaviour*, in which he clearly demonstrated that proteins underwent normal chemical reactions and thus need no special set of laws to describe their behaviour.

In contrast to the relatively prompt rejection of the 'colloidal chemistry' by the immunologists in the 20's, this theory survived much longer among the medically oriented immunologists. One can easily understand the attraction of 'colloidal chemistry' for a physician like Fleck, faced with complex pathological situations, and reluctant to accept simplified reductionist chemical approaches. In his book Fleck clearly stated his preference for explanations based on colloidal chemistry. He affirmed that "colloidal reactions vastly predominate in nature over classical chemical reactions." (p. 129) Ehrlich's model of the chemical bond between the antigen and the antibody was considered by Fleck as an example of the outdated, 'old-fashioned' approach: "the lock-and-key symbols became the theory of specificity and for a long time dominated the very depths of the the specialized science of serology." (p. 117) Instead of the chemically-based theories of specificity of the antibodies, Fleck proposed a new approach to the problem of the specificity of antibodies, in agreement with 'colloidal chemistry':

We now speak of states or structures rather than substances, to express the possibility that a complex chemico-physico-morphological state is responsible for the changed mode of reaction, instead of chemically defined substances or their mixtures being the cause. (p. 63)

We can see how Fleck's rejection of the most fundamental notions of his discipline such as the etiological concept of disease, the fixity of the bacterial
species (and the validity of Koch’s postulates), and the specificity of antibodies could stimulate his general reflections about the validity of generally accepted ‘scientific facts’ and lead him to accept a relativistic approach to natural science.

In addition, the way in which Fleck viewed biological phenomena is reflected in his description of disease as a “complicated revolution within the complex life-unit”; of bacteria as liable to “unpredictable fluctuations”; of a serological reaction as “a complex physico-chemical-morphological state”; and these point towards a general view of life process as an uninterupted chain of complex dynamic interactions in which isolation and separated study of one element is senseless because each one of these elements is at the same time influenced by and influencing the others. Such a vision of life phenomena could have an influence on Fleck’s vision of the social interactions conducive to the ‘genesis and development of a scientific fact’.

ACKNOWLEDGMENTS

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NOTES


2 Fleck’s work anticipated in many points the book of T. S. Kuhn The Structure of Scientific Revolutions but it should not be reduced only to the aspects which were later developed by Kuhn. An evaluation of the original contribution of Fleck’s epistemological studies was made by Thomas Schnelle in the article ‘Ludwik Fleck and the Influence of the Philosophy of Lwów in this volume.

3 Thomas Schnelle, Ph.D. thesis; T. Schnelle, op. cit.


4 His principal epistemological work, the book Genesis and Development of a Scientific Fact is an analysis of the history of the modern view of syphilis (defined actually as a disease induced by a specific bacterium) and the history of the Wassermann serological reaction widely used to detect syphilis. In his other epistemological works also, the majority of concrete examples designed to demonstrate how science really works, are taken from his own scientific discipline.

5 A detailed biography of Fleck can be found in the article ‘Microbiology and Philosophy of Science, Lwów and the German Holocaust: Stations of a Life – Ludwik Fleck 1895–1961’ by T. Schnelle (this volume).
Fleck was a physician by training, and a scientist by vocation and ambition. He started his professional career working in Lwów University. But later, following two years at the university, he was unable to secure an academic position, and in the years 1924–1939 he had a series of jobs involving routine laboratory analyses. During this long period, Fleck did not give up his scientific interests, and he did some research in his own private laboratory. But this research, made in his free time, was not basic, but mainly goal-oriented. His scientific articles published between 1925 and 1939 were closely related to his daily professional occupations (improvement of laboratory tests, description of clinically unusual cases, etc.).

Fleck continued to aspire to full time teaching and research work, and to recognition by his peers. He succeeded in realising these aspirations when the changed socio-political context permitted it: he worked in the microbiology department of Lwów University in 1939–1941, during the Soviet occupation of this city and held several important academic positions in post-war Poland.

In the period in which his epistemological views matured, Fleck was an outsider to the academic world. It is possible that his socio-professional position at this time influenced his rather unusual choice to address himself to a general public, rather than to the scientific community, in order to defend his views concerning his scientific discipline and natural sciences in general. His position as an outsider to scientific institutions probably also enabled him to have a particularly acute vision of the social aspects of formation of scientific knowledge, and of the consolidation of scientific facts.

Fleck’s scientific work was in this period centered on problems of vaccination, but he also published articles on proteinotherapy: ‘O stosunku proteinoterapii do anafilaksji’, *Polska Gazeta Lekarska* 2 52. 830–832; and with Dr. Ginielwicz: ‘Proby Proteinoterapii przy użyciu mleka kobiecego i szczepionek bakterijnych’, *Polska Gazeta Lekarska* 44 3, 662.

With the exception of vaccines and sera prepared against bacterial toxins rather than the bacterium itself, as is the case for diphtheria and tetanus, against which the first efficient antiserum were prepared in the 19th century.

Vaccinotherapy was the reinjection in the patient of small quantities of the same bacteria which were at the origin of the treated infection. Proteinotherapy was the injection of a minute amount of foreign protein: it was designed to stimulate the defense mechanisms of the body, in the spirit of I. Metchnikoff and A. Wright.

W. D. Foster: *A History of Medical Bacteriology and Immunology*, William Heinemann Medical Books, London, 1979, pp. 141–148. The attempts to treat cancer by either non-specific stimulation of the body defense mechanisms or by raising ‘cytotoxic’ (cell-killing) sera against the cancer cells were all unsuccessful.


Both (cellular and humoral) schools agree that the purpose of immune mechanism is the defense of the organism against infectious agents. Both agreed also that cellular mechanisms and the circulating antibody play some role in this defense. The divergences between the two schools concerned the relative importance of each component in this defense. Metchnikoff’s school, more attached to the traditional pathological approach,
considered the phagocytic cell, acting in a non-specific way, by engulfing every strange
body it finds in its way to be the crucial anti-infectious defense element. Ehrlich’s school
was more chemically oriented: it gave absolute priority to the specific circulating anti-
odies which appear in the serum as a consequence of immunization and which are
pecific to the immunizing antigen.
13 A. M. Silverstein, op. cit.
14 A compromise was achieved about 1905. The humoral antibodies were recognized
as important in neutralizing toxic bacterial products and as having an auxiliary function
helping the phagocytes to engulf the invading microorganisms (opsonization)
while the phagocytes continued to be considered as important as ‘first line’ defence against
fection and concerned with destruction of the bacteria. This compromise is for
example reflected in the presentation of the works of Metchnikoff and Ehrlich; during
the ceremony of the award of the Nobel prize in Physiology to both of them in 1908
(presentation speech by K. A. H. Morner: Nobel Lectures: Physiology or Medicine,
15 S. Arrhenius: Immunochemistry, the Application of Physical Chemistry to the
Spezifität der serologischen Reaktionen, Springer, Berlin, 1933, 1st ed.; The Specificity
subtle distinction can be made, inside the immunochemical school, between Ehrlich’s
view of antigens, reacting with antibodies in an irreversible, highly specific manner and
forming a stable chemical bond; Arrhenius’s vision of the antigen-antibody bond as
an equilibrium state; and finally Landsteiner’s concepts of the existence of a continuum
of antigenic specificities, without any sharp delimitation between them.
66 (1900), 424.
17 His quantitative studies of the antigen-antibody bond were made while he worked on
the standardization of anti-diphtheric sera for clinical uses. He viewed the antibody as a
‘magic bullet’ going directly and specifically to its target, the antigen, and neutralizing
it, thus as the perfect weapon in fighting infectious disease, W. D. Foster, op. cit.,
pp. 113–123.
18 Haptens are small molecules that when fixed on proteins (so-called ‘carriers’) elicit
the formation of specific antibodies directed against the hapten. The haptenes were
among the favourite tools of the immunochemists permitting detailed studies of specific
antibodies directed against simple and well-defined chemical structures, instead of
complex organic compounds or even worse, whole microorganisms (as was the case for
‘classical antibodies’). K. Landsteiner and H. Lambl: Bioch. Zs. 86 (1918) 343; K.
Landsteiner, The Specificity of Serological Reactions, op. cit.
19 Terms like ‘immunity’ had quite different meanings in the thirties, according to
the general orientation of the article in which they appeared, a medical context or an
immunochemical one; this makes more difficult the contemporary reading of articles
from this period.
20 “From 1900 to 1930, Immunology was composed from false empiricism and
cursed terminology, a mixture of vaccines, antisera and cutaneous tests and nothing
else.” Peter Medawar, Interview with J. Goodfield in Cancer under Siege, Hutchinson,
His vision of immunology is expounded in his article ‘Czym jest i czym powinna byc immunołógia’ (What Is Immunology Now and What Should It Be?), Medycyna Doswiadczenalna i Microbiologia 8 (1956), 397–402, starting with the definition “Immunology is the science of anti-infectious resistance” and further on explaining that “Immunology is as a matter of fact the physiology of anti-infectious defense”.

In his later research he also used chemical and quantitative methods, but only as auxiliary methods intended to support pathological observation (while “chemical” approaches, in contrast, view observation as the first stage conducive to quantitative analysis). For example L. Fleck and I. Lile-Szyszkiwasz: “Leucocytag and the Glycogen Content in Leucocytes”, Bulletin de l’Academie polonaise des Sciences, serie des Sciences biologiques 3 (1955), 137–143.

L. Fleck: Genesis . . . op. cit. p. 111.

Using T. S. Kuhn’s terminology, he considered in microbiology the growing accumulation of facts to be incompatible with the dominant paradigms and thereby to be signaling an approaching change of those paradigms.

Lecture given in the IV meeting of the Society of Friends of History of Medicine in Lwów, Archiwum Historii i Filozofii Medycyny oraz Historii Nauk Przyrodniczych 52, 298.

An ‘etiological vision’ of a disease is a definition of a specific disease by the means of its (presumed) etiological cause; in the case of an infectious disease, the microorganism that provoked it. For example syphilis is defined as the disease provoked by Treponema pallidum, tuberculosis as infection by Mycobacterium tuberculosis, etc. Two diseases presenting similar symptoms (like gonorrhea and primary syphilis) are distinguished on the basis of identification of different etiological agents.

Fleck’s perception of the difficulty in defining disease in purely rational terms, and in reducing pathology to natural sciences such as chemistry, was shared by other microbiologists with a strong clinical orientation. For example Charles Nicolle in Destin des maladies infectieuses (Librairie Felix Alcan, Paris, 1933) explained that “Parce que la maladie est complexe et qu’elle change sans cesse, nous devons nous mener, dans notre étude des figurer de langage ordinaires aux sciences exactes. ( . . . ) Jamais nous ne devons oublier que les faits dont nous nous occupons sont mouvants, qu’aucune formule ne peut les fixer. ( . . . ) Il faut dans notre étude se mener même de la raison, de la logique” (p. 35).

Fleck’s originality was to advance one step further than Nicolle and others, by affirming not only the difficulty (largely admitted), but the absolute impossibility of the reduction of the medical way of thinking to any other, thus the incommensurability of the biological or chemical way of thinking with the medical way of thinking. This view probably led him to the general idea of the co-existence of multiple incommensurable ‘thought styles’ in natural sciences.

Die Naturwissenschaften 17, 23 (1929), 425–430.

Syphilis was, for a long time, confused with a profusion of other diseases such as other venereal diseases, skin diseases, (for example leprosy), and even with cancer.

In explaining the origins of the Wassermann reaction by its connection with the popular notion of ‘syphilitic blood’ Fleck partially amalgamated two distinct phenomena. The popularity of the test, its rapid acceptance and diffusion, and eventually the ease of finding financing for research designed to develop a blood test for syphilis...
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— all these could, and probably were influenced by popular feelings about syphilis. In contrast, the motivations of the scientists themselves could be quite different. In the beginning of the 20th century, with the acceptance by the scientific community of the humoral theory of antibody production, the immunologists were in their great majority firmly convinced that every infectious disease (and syphilis is a highly infectious disease with a generalized outcome), is necessarily accompanied by the appearance in the patient’s serum of specific antibodies. The immunologists therefore did not need any supplementary motivation in looking for the presence of specific anti-Treponema antibodies in syphilitic blood, beyond their deep certainty that those antibodies should be there. Only later, when the unusual features of the Wassermann reaction were found, some scientists put forth the possibility that this test is not measuring an ordinary antigen-antibody reaction, but some unknown property of the serum of syphilitic patients, a concept that could have been influenced by a preconceived idea of the existence of a ‘syphilitic blood’.

32 The choice of these problems was certainly not accidental. Specificity was a key problem in the development of modern biology. The studies of variability of bacteria greatly contributed to the birth of molecular genetics. Research on specificity of antibodies and theoretical reflections about the origins of this specificity were of fundamental importance to the actual developments in Immunology. Fleck rightly pointed to some of the most crucial problems of biology of his time. His feeling that bacteriology and immunology are on the eve of a paradigm shift was amply justified by subsequent events: but he was completely mistaken as to the direction the future ‘scientific revolution’ in biology would take. Seen retrospectively, his sympathies, as far as the ultimate developments in fundamental biology are concerned, were on the ‘loser’s’ side.

33 Actually, one can say more ‘ecologically’ oriented.

34 A holistic approach to pathological phenomena, and the vision of disease as a result of complex host-parasite interactions, became one of the important orientations in contemporary medicine. It was popularized in particular from the 60’s on. This tendency is represented in R. Dubos’s book, *The Mirages of Health*, Anchor Books, New York, 1957. Dubos was one of the pioneers of the ‘ecological’ approach to diseases.

35 Pasteur’s and Koch’s demonstration that definite bacteria induced a specific disease rested on their demonstration that bacteria do form stable species and that from every animal, anywhere, which suffers from anthrax, exactly the same anthrax bacillus can be isolated.

36 It was only much later, in the 50’s and the 60’s, as a result of progress in molecular biology and of better understanding of bacterial heredity, that the various causes of ‘bacterial variability’ could be better understood.

37 Bacterial transformation was the capacity to change (permanently and in a hereditary manner), a virulent variety of bacteria (S for smooth) into non-virulent variants (R for rough). The most striking of these transformations was the transformation of the pneumococci, (the bacteria inducing pneumonia), first observed by F. Griffith in 1928 (F. Griffith: ‘The Significance of Pneumococcal Types’, *J. Hyg.* 27 (1928), 113–157). Griffith was able to show that if a rabbit is injected with living avirulent (R) pneumococci of type II, together with killed virulent pneumococci of type III, he was able to isolate from the animal living virulent (S) pneumococci of type III; i.e. the dead bacteria somehow “transformed” the living ones. We do not know if Fleck was acquainted with Griffith’s work, although it is probable that he read the confirmation of this work by
German microbiologists, published in the leading German immunological journal *Z. Immunitätsforsch.* (F. Neufeld and W. Levinthal: ‘Beiträge zur Variabilität der Pneumokokken’, *Z. Immunitätsforsch.* 55, 324–340), a journal in which Fleck also published several articles.) In another article ‘On the Concept of Species in Bacteriology’, published in 1931, Fleck, discussing the phenomenon of bacterial transformation referred to an earlier work of Arkwright who gave the first description of the S/R transformation in bacteria. J. A. Arkwright: ‘Variation in Bacteria in Relation to Agglutination Both by Salt and by Specific Serum’, *J. Pathol. and Bacteriol.* 24 (1921), 36–60. At about the same time, in the early 30’s, O. T. Avery, who was, like Fleck, trained as a physician and specialized in microbiology and immunology, but who had, contrary to Fleck, a strictly chemically-oriented approach, had an exactly opposite reaction to the description of the phenomenon of bacterial transformation. Avery, who believed in the stability of bacteria species, assumed that some definite chemical substance must be responsible for the observed transformation, and set up, together with his collaborators, a program intended to isolate this substance. The result, as is widely known today, was the first demonstration that genetic information is carried by molecules of DNA. (R. Dubos: *The Professor, the Institute and DNA*, Rockefeller University Press, New York, 1976.) Fleck is probably referring to so-called Koch’s postulates. The main criticism against the theory of bacterial etiology of the infectious diseases was, at the time of Pasteur and Koch, the assertion of their opponents that the presence of bacteria has no special meaning since many different types of bacteria could be isolated from both healthy and sick individuals. In order to answer this criticism Koch established his ‘postulates’, restrictive criteria needed to prove that a given bacterium is really the etiological agent of a given disease. Those included the isolation of the bacterium from the typical lesions of the patient, reproduction of a similar disease in an experimental animal with the isolated bacterium, and reisolation of the same bacterium from the lesions appearing in the experimental animal. (W. D. Foster, *op. cit.*, ch. 3.)

A lecture given during the meeting of the circle of scientific personnel of the Medical Sick Fund in Lwów, *Polska Gazeta Lekarska* 10 26 (1931), 522–539. The original idea about identity between the bacterium Proteus X-19 and *Rickettsia prowazekii*, was not due to Kuczyński. It was advanced already in the early 20’s by some of the most well-known specialists on typhus such as F. Breinl and R. Weigl (Fleck was Weigl’s assistant in the years 1920–1922). It must be stressed that in the 20’s and 30’s, before the development of the electron microscope, and with virology at its beginnings, very little was known about sub-microscopic infectious agents; the viruses and the rickettsiae. Considering viruses or rickettsiae as life stages of bacteria was certainly not the most widely accepted contemporary opinion, but, in the 30’s it was still an acceptable one, within the framework of the existing knowledge.

Kuczyński thought that *Rickettsia prowazekii* was the simplified form and Proteus X the saprophytic form of the same typhus-inducing organism: Fleck in 1931 seems to accept it as a probable explanation for the fact that blood tests for *Rickettsia*-induced typhus can be done with bacterial (Proteus X) extracts.


I.e. if it is suspected that a patient suffers from a staphylococcal infection, he is
expected to have the specific anti-staphylococcal antibodies in his serum. If, following
serological tests, such antibodies are found in his blood, this will confirm the diagnosis
of a staphylococcal infection even without actual isolation of the bacterium.
43 Most of the research workers of this period tried to find a 'conservative' explanation
of this peculiarity of the Wassermann test, i.e. an explanation in agreement with the
prevalent views concerning the specificity of the antibodies. Fleck quoted some of these
explanations in his book (pp. 74–75). The two most popular ones were that either the
antibodies found in syphilitic blood were auto-antibodies; antibodies directed against the
tissues of the patient himself (and therefore able to react with extracts of normal tissue)
or that those antibodies are 'true' antisyphilitic antibodies, directed against Treponema
pallidum, that cross-react with alcoholic extracts of certain normal tissues, probably
because the bacterium and those extracts share some antigens. (It is the last explanation
that was finally proved correct.)
44 The specificity theory was set forth by J. Citron (a collaborator of Wassermann) in
a text dating from 1910 and quoted by Fleck as a typical example of the "old" outdated
vision. · · · by antibodies we mean all the specific reaction products formed by the
organism against disease germs and their products · · · I would ask you to commit firmly
to your memory the law that every true antibody is specific and that all non-specific
substances are not antibodies. The law of specificity is the precondition of serodi-
agnostics" (p. 58).
45 'Bacteriolyins' or 'hemolysins' are antibodies capable of lysis (usually measured
in the test tube) of bacteria or red blood cells.
46 Fleck's opinions concerning the nature of antibodies can seem strange from the
current point of view. But one should take into consideration that the purification of
specific antibodies and a formal proof that specific antibodies are proteins was made
only after the publication of Fleck's book, in the late 30's, with the development of such
techniques as ultracentrifuging and electrophoresis, which permitted purification of
50 Years Ago – Experiences, Perspectives and Problems of the First 21 Years', Ann.
Rev. Immun. (1983), 1–32). For before a substance is isolated and purified, its chemical
nature cannot be affirmed (for example, in the 30's it was generally believed that anti-
bodies were proteins but at the same time it was assumed that the genes were proteins
too). Fleck's position, although extreme, was not in contradiction with any contem-
porary clear-cut experimental data.
47 This metaphor was borrowed from Ernst Fischer's description of enzyme and its
substratum.
49 One of the principal supports of colloidal chemistry applied to immunological reac-
tions was that antigen and antibody do not form quantifiable chemical bonds but obey
more complex and less quantifiable principles of adsorption. The diffusion and the
impact of colloidal chemistry on immunology were described by P. M. H. Mazumdar:
50 Fleck classified 'vitalism' together with 'bacterial transformation' and 'specificity
in immunology' as the words that in modern biology became "slogans endowed with
magic powers" (p. 43).
51 An example can be seen in the statement of the Viennese chemist W. Pauli
"unquestionably they [the methods of colloid chemistry] enlarge that territory which the organic and the inorganic world have in common. The last barriers between the two cannot as yet be broken down. There always remains an unsolved problem, the kernel, as it were, of vital phenomena. The cause of the final failure of the new instruments can rest only in their origin. They have all evolved from the study of lifeless matter. For a complete understanding of the living, the words of a great physiologist will probably hold: "Life can perhaps only be understood through life itself." W. Pauli: 'Über physikalisch-chemische Methoden und Probleme in der Medizin,' Vienna, 1900, quoted by P. Mazumdar, _op. cit._


53 J. Bordet expounded it, although in much more attenuated form, in the second edition of his _Traité d'Immunologie dans les maladies infectieuses_, Masson, Paris, 1939, p. 599. In Fleck's case this survival was particularly long. In the mid-30's it was rather rare to continue to consider 'colloid chemistry' as the coming revolution in biology.

In general, Fleck's views of biology, microbiology and immunology, as expressed in his book (1935), can strike us as 'anachronistic'. In the 30's, it was also rather unusual to stick to views such as cyclogeny in bacteria or negation of the chemical nature of antibodies. Such views were much more popular in the period 1910–1920. Fleck, when discussing general topics in immunology and bacteriology, was nearly always referring to works written at the latest in the early 20's. It is probable that his 'anachronistic' views reflect his difficulties in following the latest developments in his scientific discipline, far away as he was from the important scientific centers and outside of scientific institutions. Fleck (as revealed in his scientific papers) was able to follow, at least to some extent the new findings concerning his immediate field of specialization. But, he probably developed his basic ideas about fundamental problems of biology and microbiology during his formation years (till 1924). The opinions expressed in his book reflected his further solitary elaborations and reflections about his scientific discipline, made on the basis of knowledge acquired much earlier. His epistemological writings show little awareness of the basic conceptual or theoretical innovations in his discipline after the period he left the university. This can account for the presence in these writings of speculations about the future of his scientific discipline which are an unusual mixture of sharp intuitions and outdated theoretical considerations.
PART IV
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PREPARED BY THOMAS SCHNELLE

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