SCIENTIFIC KNOWLEDGE SOCIALIZED

Selected Proceedings of the 5th Joint International Conference on the History and Philosophy of Science

Edited by
I. Hronszky, M. Fehér, B. Dajka

AKADÉMIAI KIADÓ, BUDAPEST

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Selected Proceedings of the 5th Joint International Conference on the History and Philosophy of Science Organized by the IUHPS, Veszprém, 1984

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AKADÉMIAI KIADÓ, BUDAPEST 1988

507637

MAGYAR
TUDOMÁNYOS AKADÉMIA
KÖNYVTÁRA

This book is published by Kluwer Academic Publishers
as Vol. 108 in the series "Boston Studies in the Philosophy of Science"

ISBN 963 05 4771 6

© Akadémiai Kiadó, Budapest 1988

Joint edition published by Akadémiai Kiadó, Budapest,
Hungary and Kluwer Academic Publishers, Dordrecht,
The Netherlands

Printed in Hungary
by Akadémiai Kiadó és Nyomda Vállalat, Budapest

M. TUD. AKADÉMIA KÖNYVTÁRA
Könyvleltár 379.6./19 83-sz.

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PREFACE

The present volume contains the selected proceedings of the *5th Joint International Conference on the History and Philosophy of Science*. The Conference was held in Veszprém, Hungary, 14th to 20th August 1984, by the IUHPS through its Joint Commission. Members of the Joint Commission included Imre Hronszky (Chairman, Hungary), Ernan McMullin (U.S.A.), and Uffe J. Jensen (Denmark). Additionally appointed members were Hans Lenk (F.R.G.), Márta Fehér (Hungary), András Hajnal (Hungary), and the late László Mátrai (Hungary). The main topic of the Conference was the 'Impact of Society on Scientific Cognition'. Papers were also read on the topics of 'Exact Models of Science', 'New Philosophical Trends in Logic', and 'Linguistic Philosophy'. There were a few presentations on such problems, now classical in the philosophy of science, as incommensurability or the conflict between rationalism and historicism. The Joint Commission invited 72 speakers, 57 of whom accepted the invitation: 49 came and held their lecture, while another approximately 30 participants came from various countries, mainly from Hungary. An especially large number of Hungarian specialists, among them representatives of the younger generation, took the rare opportunity to attend a meeting of such international importance. We regretted the absence of a few prospective participants (among them M. Hesse and J. Barwise), who were prevented at the last moment. (We owe special thanks to Professor Hesse for the text of her paper which she furnished later.) The interest Hungarian colleagues took in this work gave the idea to the Joint Commission to stage three 'poster sessions' which provided young specialists with the possibility of meeting leading experts in their respective fields. Let us hereby

thank Professors G. H. von Wright, E. McMullin, and J. L. Cohen, who probably sacrificed rare opportunities of leisure in their hectic conference schedule to act as benevolent chairmen of the poster sessions. Some of the materials presented there are published in *Doxa–Philosophical Studies*, a periodical of the Hungarian Academy's Institute of Philosophy. (Requests for all *Doxa* numbers are to be addressed to the Institute of Philosophy, 1398 Budapest, Pf. 594.) The present volume of the selected proceedings is mainly composed of papers presented on the 'Impact of Society on Scientific Cognition'. Papers presented on Logic and Linguistic Philosophy are published in *Logique et Analyse.*

The Conference was sponsored by the IUHPS, as well as UNESCO, and on Hungary's part by the Hungarian Academy of Sciences and the Technical University of Budapest. Special thanks are due to the Technical University for various facilities such as its resort at Lake Balaton. We are also grateful to the Academic Committee of Veszprém who, through the kind assistance of Mrs Edit Bakács, offered us the inspiring milieu of their 18th-century three-storey palace in the Veszprém Bourg, equipped with a magnificent garden where the long conference days were extended into lively evening talks and friendly encounters. We received no less valuable help from the personnel of the Veszprém State Administration School where many of the participants were catered for and housed. Kitchen chefs and attendants did a good job in negotiating local and international ideas of culinary pleasures. Mrs Judit Kaiser, from the Technical University, applied herself with outstanding efficiency to the task of organizing all that was possible and impossible.

The Joint Commission expresses its collective thanks to Professors E. N. Hiebert (President of IUHPS) and R. Hilpinen (General Secretary of DLMPS) who excelled both as invited speakers and promoters of financial and organizational matters of the Conference.

Finally our thanks must also go to the Publishing House of the Hungarian Academy of Sciences, especially to Matild Gulyás as

copy editor, and to Kluwer Academic Publishers as well as to Professor R. S. Cohen, editor of Boston Studies in the Philosophy of Science, for their readiness to publish the conference materials.

On behalf of the organizers and editors
IMRE HRONSZKY

PART I

THE IMPACT OF SOCIETY ON SCIENTIFIC COGNITION

MARY HESSE

SOCIALIZING EPISTEMOLOGY

I. EPISTEMOLOGY OF SCIENCE

Epistemology as a normative philosophy of scientific knowledge has had a bad press recently. Criticism has come both from philosophers like Feyerabend and the neo-Popper school, who claim that there are in principle no rules of scientific research, and from sociologists of science who claim not to find any in scientific practice. This paper will consider why these charges are made from the direction of sociology, and whether they are justified.

Traditional epistemology has been parasitic upon an accepted ontology. That is to say, it has been assumed that we know there are certain kinds of objects in reality, and the question has been, how do we come to know this? This is the case for Platonic forms, Aristotelian species and demonstrative knowledge, for the two modes of knowledge recognized in the 17th century – mathematics and sensory knowledge, subsequently for Newtonian science, and then for the accepted ontology of natural science in general. Empiricist ontologies of natural science demanded an epistemology in order to explain what kinds of data there are, how they get connected to theories, and what the criteria of theory choice are. All this presupposed, in some sense, a realism of the objects of natural science. There is an objective environment to be explored by science, the question is, how do we draw correct conclusions about it?

A challenge to this programme arose from the recognition (first due to Kant), of the mental ingredient in all this, in the form of conceptual schemas organizing sensory knowledge, which could lead to underdetermination of theory by data and to the possibility of radically different internal ontologies as described by different

Imre Hronszky, Márta Fehér and Balázs Dajka (eds), Scientific Knowledge Socialized, 3–26.
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theories. This challenge, however, did not necessarily lead to an abandonment of realism, but only to a change in its locus – it could become (as most recently with Quine) an ontology of sensory inputs, not of an objective world lying behind these. Quine also removes the remaining rationalist elements in science by suggesting that there are no fixed a priori truths about logic or conceptual frameworks to which science must conform. Epistemology becomes *naturalized* – it describes how we get around in our natural environment by inductive methods, which may themselves be understood as extensions of animal learning. By methods of trial and error, aided by both biological and cultural evolution, we get better and better knowledge of the objective natural world, as tested by the accumulating success of our scientific predictions and capacity to control. The previous a priori elements identified by rationalists become contingent universals of human biology and psychology, whose success has been guaranteed, not by their a priori truth, but by the various mechanisms of selection and survival. If one wanted to put a constructivist gloss on this, one would speak of the 'objectivized' natural world as a construct of human sensory interaction with the world and the biological need to cope successfully with it. But such locutions do not conceal the underlying realism of the position – the ontology is one of interaction *within* a natural reality embracing the physical, the biological, and the human perceiver.[1]

One problem with this naturalized epistemology is that there is little detailed evidence for it. The complexities of scientific development and its technical exploitation do not look, on the face of it, like the natural extension by the human species of animal learning mechanisms. Nor is it obvious that they have much to do with the survival of the species in the biological sense, at present indeed rather the reverse. The most important objection is, however, that naturalized epistemology remains incurably *individualist*. Hints towards cultural as well as biological evolution are not sufficient to

take account of the social context of science. Epistemology needs not only to be naturalized but also *socialized*.

The socialization of epistemology may be said to begin with Durkheim, whose overt aim was to replace the rationalist element in received epistemology by social categories.[2] He notices the vast variety of categorial frameworks and beliefs operative in traditional societies and wishes to explain these not by Lévy-Bruhl's 'prelogical mentality' but as symbolic elaborations of social structures – elaborations which take off from the social base and become quasi-autonomous systems of reasoning, appearing to claim the absolute allegiance of the mind. How is it, he asks, that logic, and the concepts of truth, cause, class, number, space, time, in a given society, attain consensus, stability, authority and a quasi-absolute status? It can only be, he replies, that, like the concept of God in religious societies, they have behind them social power and sanction, and these are derived from the actual force inhering in social norms and structures, such as clan divisions and hierarchies, and seasonal imperatives of hunting and agriculture. Like Lévy-Bruhl, however, Durkheim declined to apply these insights to the modern scientific mentality, and so although he has a sociology of the rational and of religion, he has no sociology of science. There are two chief reasons for this. The first is that his whole theory is predicated on an evolutionary view of human history which itself rests on a scientific theory of natural evolution, and this cannot be socialized without apparently undermining itself. The second is that his sociological method itself is always explicitly positivist. He believes that positivist scientific method is the highest product of the evolving forms of knowledge, and is therefore, in our time, effectively uncriticizable.[3]

Thus in the end Durkheim's socialized epistemology is conservative and uncritical with respect to both natural and social science. He provides tools for the study of systems claimed as cognitive by other, non-scientific cultures, but neglects the self-reflexive critiqe of his own tools, and consequently the possibility of alternative

modes of knowledge open to us. Meanwhile precisely these alternative possibilities were being explored by Dilthey and Weber. Dilthey asks the epistemological question about the study of history that parallels the one Kant had asked about the study of nature: we *have* historical knowledge (in the burgeoning studies of 19th-century Germany), *how* is it that we have it? Dilthey, Weber, Gadamer, Habermas and others have answered the question in terms of various forms of understanding *(Verstehen)* and in terms of social *interests* in a form of knowledge that is not that of natural science, but has to do with the practice of human communication and mutual interpretation, in other words, the normative knowledge of history, politics and morals becomes the subject matter of a non-positivist epistemology.

Modern Durkheimians have objected to Durkheim's exclusion of natural science from his socialized epistemology, but they have not always broken free of his positivism. For example, in their different ways both Mary Douglas and David Bloor[4] have tended to regard natural scientific methods (the search for testable laws, etc.) as the appropriate methodology for the sociology of natural science as well as for its own practice. But Bloor's 'strong programme' asks that science should be subject to the same scrutiny as every other social institution. This surely means that the sociological method adopted in studying science should also be subject to scrutiny, not just in the sense that the development of sociology of science itself needs a social explanation (though it does), but also that the method of sociology should not be presupposed to be that of the natural sciences. More recently adherents of the strong programme have explicitly acknowledged that normal historical criteria of adequacy are appropriate for sociology of scientific knowledge, and they look not only for generalizable causal laws, but also for the particular cause–effect relations and appeals to human intentionality that characterize general history.[5]

There is another point to be made about the application of the strong programme in sociology of science. Bloor proposes that ex-

planations in history of science should be 'symmetrical' as between true and false theories, and between modes of thought which may or may not reflect 20th-century Western interpretations of scientific methodology. Another way of putting this thesis is to say that scientific theory, past or present, should be investigated on the same basis as that adopted for the symbolic belief systems of cultures in general, according to the methods of sociology and social anthropology. This means, among other things, that cognitive terminology such as 'belief', 'knowledge', 'truth', 'falsity', 'rationality', will be used in describing actors' thoughts and behaviour as it is used by actors themselves, without bringing any external judgements to bear from our own standards of knowledge and truth. It also means that social causation of beliefs will be sought in the same way whether we judge the beliefs to be true or false, rational or irrational, and that when this social methodology is applied to the past or present science of our own culture, even there no such judgements are relevant.

We should note, however, that an unnecessary air of paradox may attach itself to this formulation of the strong programme. If we take the sociological analogy seriously, it is clear that the principle of symmetric explanation can be used to safeguard, rather than to reject, the distinctiveness of Western science. For if we consider, say, an anthropologist fallen among Cavendish physicists, a necessary part of his method will be to learn the distinctive standards and rules of truth, correctness, empirical test, rational argument and the rest that his actors exhibit, and he will have to try at least to understand these as they do. There is no need, therefore, for the strong programme to suppose that there is no difference between tentative scientific beliefs, serious hypotheses, theories, and accepted parts of scientific knowledge, because these things will be governed by the actors' own rules, and these are different from the overt criteria of truth and inference in more traditional cultures. The scientific rules may or may not be identical with those discussed by philosophers of science, about which there

is dispute, but they will generally include at least the requirement of empirical testability, leading to criteria for good theories which require successful predictions, more or less conforming to standard logic, etc. The adherent of the strong programme need not deny that in our culture there is some recognition of these technical and logical 'reasons' for adoption of the theories we do adopt; what he may rightly go on to point out is that the actors' accounts may contain elements of false consciousness, and that in any case adoption of the criteria themselves is subject to social causation which can be investigated socially and historically.

II. ACTORS' MODEL

The attempt to discover the explicit institutional rules of Western science is just one example of the sociologist's *actors' model*, that is, the principle of giving privilege to actors' overt points of view. But, as philosophers of Western science have discovered, it is not possible to follow this principle very far without finding it necessary to simplify, idealize and reconstruct, if only because actors' overt points of view are rarely systematic enough to provide a coherent story. Before looking at these problems directly, however, it will be useful to have before our minds two examples of social historiography of science which on the whole adopt the actors' model, while arguing that important features of theory are inexplicable by the standard 'rational' account of scientific rules and methods. I take both examples from physics during the last 100 years, because it is presumably in physics rather than in softer sciences that the thesis of social constraint will be most difficult to establish. The examples are Paul Forman's now classic discussion of 'Weimar Culture, Causality and Quantum Theory', and Brian Wynne's interpretation of ether theories in Cambridge in the late 19th century in 'Physics and Psychics: Science, Symbolic Action and Social Order in Late Victorian England'.[6]

Forman describes the introduction of acausality into quantum physics by German physicists in the aftermath of defeat in the First World War. His thesis is that this has to be explained in terms of contemporary attacks upon mechanistic science, coming from a romantically influenced intellectual milieu, and disseminated widely among physicists. Moreover, Forman argues that the rejection of causality in physics is not explicable in terms of internal scientific developments. Wynne describes the intellectual milieu in which ether theories arose, and argues that this was one of conflict between, on the one hand, utilitarian and professional exploitation of materialist science as a basis for education and morals, and on the other hand a more conservative scientific realism for which such exploitation of science was seen as a threat to social order. In the conservative view which dominated Cambridge science, metaphysical and spiritual interpretations of human nature were seen as being supported by the postulate of a nonmaterial ether, and indeed ultimately by an ether which subsumed the theory of mechanical matter itself, and thus provided a physical model of unity and continuity. Also, some of the Cambridge physicists were active in the nascent Society for Psychical Research, for which a scientifically grounded ether theory provided welcome intellectual support, and which was used to give rational backing to a less articulate spirituality of the 'lower classes'. Like Forman, Wynne claims that the transformation of physical theory involved in the ether postulate was not required by the data nor by the tradition of physics. Moreover the postulate is not found among other physicists. Those working, for example, in contemporary Germany adopted the contrary postulate of action at a distance. Therefore, Wynne concludes, the appearance of ether in a particular place and time must be related causally to non-scientific factors.

Both case studies make the historical claim that a theoretical discontinuity having important metaphysical implications cannot be explained as an outcome of purely scientific reasoning. 'Purely scientific reasoning' might of course be interpreted in such a strict

sense as to make this claim merely platitudinous, for it is generally accepted that in a complex scientific situation theories will be underdetermined by logic and evidence, and hence not explicable by purely scientific reasons in that sense. But the claim becomes substantial if we take it in the sense that internal reasons drawn from logic, evidence, normal inductive reasoning, and the local scientific tradition are not sufficient, and moreover that the remaining explanatory gap cannot be filled by reference to individual psychology ('great man' theories of explanation), and should not be filled by appeal to simple historical accident.

Given that understanding, both authors present their case studies as standard history, open to historical debate and testing and perhaps refutation. Forman has received such a response from John Hendry,[7] who has cited evidence for a greater isolation and autonomy of the community of quantum physicists than Forman allows, and has spelled out some of the internal argumentation that went on between the founders of quantum theory, out of which rejection of deterministic causality emerged. He argues that there were after all sufficient internal reasons for change, and that in any case the rejection of causality was not in itself seen by physicists as the major conceptual shift of the new theory: it was less important, for example, than the reinterpreted relation between observer and observed, and the new space–time concepts implied by quantum theory.

Hendry concludes his paper with a series of questions about the nature of historical causation, and recognizes that in a situation as complex as Weimar science there is unlikely to be a simple set of causal influences, nor are these likely to be entirely explicit, either in the minds of the protagonists or in the historical sources available. In perusing various examples of the lively debate at present going on in history of science between social historians and their critics, it becomes increasingly clear that although first-order historical work is essential to arguments about the validity of the strong programme, yet issues of principle are not going to be con-

clusively settled by historical evidence. The issues soon become deeper ones about historical methodology and about the nature of scientific knowledge. Protagonists on the two sides of the debate no longer dispute the *possibility* of a variety of causal factors from economic, political, social, intellectual, rational and empirical sources; the dispute is rather between those who wish to *weight* these factors differently. On the one hand are the 'rationalists', who regard the history of science as primarily the arena of rational thought and action, and who wish to pursue rational explanations to breaking point if necessary, after which they may admit 'deviant' ideological, social and psychological factors as causative. At the other extreme there are adherents of the strong programme, who claim to be completely neutral about the type of causation involved in any particular case, whether of 'good' science or 'bad'. The first group tend to adopt a model of the rational agent as a theory of belief in the human sciences in general, and in history of science in particular. The second group have frequently adopted socially instrumental and symbolic interpretations from social anthropology, as illustrated by the reference to 'symbolic action' in the title of Wynne's paper.

Forman's and Wynne's papers are not, however, exercises in this more general symbolic action model, but rather in what I have called explanation by 'actors' model'. By this I mean that they are concerned with the views and beliefs more or less explicitly presented by actors and in some cases argued for by them. In his prologue and concluding footnote Wynne describes a symbolic action model, in which the historian will recognize "tacit meanings ... in a process of continual renegotiation, defence and development", and in which the elaboration of symbolic universe will depend more on what is perceived to be efficacious social weaponry than on consistency with either social practice or scientific logic. But in his actual discussion of his case, Wynne is concerned not so much with 'tacit meanings' as with asserted beliefs, and with face-to-face connections between individuals of the different groups. He

attempts to show that in some cases there was explicit recognition of the applicability and value of scientific theory as a weapon in social conflict. He is concerned rather with individuals' rationalizations than with the latent functionality of insitutions.

The distinction between the actors' model and general symbolic action models parallels the similar distinction in social anthropology between those who give 'privilege' to actors' accounts of their beliefs and actions, and those who seek further explanations of these in functional, structural and symbolic contexts. We shall look at some such examples later, but meanwhile it is worth making a general point about the prevalence in sociology of scientific knowledge of models taken from social anthropology rather than from other disciplines of the human sciences.[8] The reason seems to be that historians and sociologists have conceptual problems in the area of ideas and thought systems that do not arise from the study of human action in general. Historians and sociologists of, for example, myth, metaphysics and religion, need an epistemology of their subject matter just as historians of science do. Students of Western thought have generally been content to adopt models of rationality based on Western logic and science as the paradigm norms of thought. It is social anthropologists who have most obviously been forced to consider modes of thought very different from Western rationality, and to take them seriously as cognitive systems. Since it is now the Western tradition of objective knowledge that is itself in question, sociologists of science have sought alternative epistemologies in the study of non-Western thought.

III. COGNITIVE SYSTEMS

The question that now arises is, What are the marks of a socially recognized cognitive system? What, on this view, *is* the subject matter of epistemology? I shall suggest three possible answers to this: first, that epistemology is about sets of belief of a certain

kind; second, that it is about symbolic statements concerning the natural and social order; third, that it is about collective world models serving ritual or practical social interests.

(1) The most obvious definition, suggested by Western philosophical tradition, is that a cognitive system is that which commands the *belief* of most, or at least a significant number of, members of a society, together with institutional structures, performances and actions that are associated with that belief. Such a definition would include religious systems grounded in communal creeds and rituals, legitimated by institutional hierarchies and theologies, and issuing in the religious ordering of social life. It would also include the institutions of science, based on a generally uncriticized consensus in our society that information about natural reality is directly available in sense perception, and indirectly available by means of increasingly elaborate scientific theorizing and experimental techniques.

In this definition of cognitive system, belief is usually construed as an individual state of mind. It is related to a theory of *rational action*,[9] according to which complexes of human actions, desires, values, goals and beliefs may be combined with facts to give explanations of action. The explanations are like scientific theories, in the sense that they rest on publically ascertainable empirical data and observable human behaviour, and use specified 'goals', 'values', and 'beliefs' as theoretical concepts of the explanatory theory. Thus an action is rationally explicable if it can be shown that it would be an effective and efficient means of attempting to reach an assumed goal, given the factual circumstances, and assuming the truth of the beliefs of the agent. There may be a payoff between possible unobserved goals and beliefs in this schema: for example a person seen avoiding sitting on a seat marked '13' may believe in bad luck and wish to avoid it, or may believe he sees a friend in seat '30' and wish to sit next to her. The most acceptable explanation of action is the one that postulates the

most appropriate unobserved beliefs and goals to provide best fit with all the facts.

In this theory actions may be rational even though the beliefs postulated to explain them are not based on 'scientific rationality'. If the Hopi dances because he believes the dance brings rain, his action is rational given his belief and his desire for rain. Thus the theory of rational action is neutral with regard to the content of 'correct belief', although it is usually assumed that the most 'charitable' explanations of people's actions should impute to them scientifically grounded beliefs if possible. In other words, the theory of rational action is usually harnessed to a scientific account of what it is to be a 'cognitive system'. On the other hand, some authors have used the concept of cognitive system in ways that do not necessarily depend either on scientifically grounded beliefs or on any consciously worked out relation between individual belief, desire and action. Religions may be maintained as social systems by habitual or imposed recitation of creeds and practice of rituals. Religious hierarchies may persist in use of their power without having any personal belief in the creedal formulas. Moreover, as Rodney Needham has shown in a detailed comparative study,[10] the Western concept of 'belief' itself, as an identifiable psychological state, is not closely translatable into the concepts of other cultures, although there is no doubt that they do have cognitive systems in the sense of institutionalized religious mythologies and world views.

The concept of 'belief' as an unobservable mental state has also been criticized in the Western philosophical tradition. Where beliefs are not scientifically grounded and do not appear to issue in scientifically reasonable actions, even the treatment of beliefs as theoretical postulates comes into question, because there may be no check on what these explanatory beliefs are other than circular checks within the whole observable complex of ritual acts and affirmations. Even the assumption within the rational action model that there will be an intention to use effective and efficient means

to a desired end may not be applicable. For example, as Weber recognized, some actions are 'for their own sake', but no less 'rational' for that.

(2) In the light of such objections, some anthropologists suggest definitions of 'cognitive systems' in terms simply of the kinds of social institution that are typically exhibited by a religion. This will include cultural orthodoxies and dogmas: "symbolic statements about the social order", [11] as Leach puts it, which issue in ritual utterances and actions.

I mention these anthropological discussions of what counts as a cognitive system because there is a sense in which the social constructivist view of Western science is an attempt to construe science as more like the myths and religions of other societies than positivists would have countenanced, and to seek to describe and explain it in similar terms. In regard to Leach's type of description, however, it is not immediately obvious how the analogy is to be drawn. The 'scientific world view' is the taken-for-granted consensus of modern Western society, to be wheeled out for purposes of pedagogy, and to give factual grounding for social and political action in relation to medicine, the environment, nuclear power, etc. But it has not (except in maverick cases like Auguste Comte) issued in regular public ritual celebrations which structure the daily life and actions of social individuals. It constitutes one great complex of social institutions among others, and makes no *official* claims to all-pervasive competence or authority. Rather the reverse, because we are used to hearing pleas from its practitioners for more study of the moral aspects of life in order to supplement the factual information in terms of which decisions have to be made. Official science is in fact generally *resistent* to the view that science is a 'symbolic statement about reality' which in principle mirrors and directs every aspect of social and individual life.

Such modesty on the part of official science is questioned, however, from two directions. There are first the scientific humanists,

and recently the sociobiologists, who put forward the view that science *ought* to constitute such an all-pervasive symbolic statement, but these are by no means a high proportion of the spokesmen of official science. Secondly, there are the critics who argue that *in fact* science in our society does have this pervasive ideological, though latent, role; that science is the institution that in fact legitimates industrial-technocratic-capitalist politics and provides the modern opium of the people. Both these points of view, as well as the general parallel between science and symbolism, serve to highlight the social characteristics that science shares with symbolic systems as identified by social anthropologists. At the same time attention is bound to be drawn to features that differentiate science and symbolism. These are not only the traditional 'rational' criteria such as falsifiability and accumulation of pragmatic success, but also *social* differentiae, such as the openendedness of scientific norms in modern societies emphasized by Horton, or the atomistic division of labour within science and within society to which Gellner draws attention.[12] The sociological approach to science does not entail neglect of its special cognitive features.

(3) A third approach, which can make room for both the parallels and the differences between science and symbolism, draws on the cognitive sciences themselves. Studies of mental modelling of the world are clearly relevant to the ways in which both science and symbolism enable humans and animals to survive and attempt to realize their purposes. Such mental modelling is not mere copying. It is already evident from work on elementary perception that no one-to-one correspondence theory of truth provides an adequate theory of modelling even of the natural environment. 'Mental modelling' must be taken to include structures having various analogical and morphic relations to 'the world' which may require elaborate transformations and mediations through action before 'the world as experienced' can be reconstructed. Moreover, models may be hypotheses, or 'stories' about the world, which issue in

testable action. The 'success' of a model may be tested by comparison of the expectations induced by it with further experiences, or by longer-term methods of biological selection, or by acceptability by social consensus and by the realization by societies of overall purposes both manifest and latent.[13]

So far little is known about how mental schemas which are social rather than individual (Durkheim's 'collective representations' or Leach's 'symbolic statements') work as cognitive systems. Functionalism, structuralism, and sociobiology are all theories of their operation, and none has been found generally adequate. But one might venture a definition of 'cognitive system' which is more satisfactory than either of the first two suggestions, along the following lines:

A *cognitive system* is a collective mental schema modelling some aspects of the world, issuing in mimetic ritual and/or appropriate action to realize human purposes.

Such definition has the merit of including those cognitive systems that are studied as such by both social anthropologists and cognitive scientists. It does not presuppose functionalism, structuralism or sociobiology, because it does not assume that societies or the human species as such have latent purposes or common psychological structure or goal-directed fitness for survival. It does however place the cognitive squarely within the domain of human institutions and human purposes, that is, within the domain of human intentionality. Does reference to 'modelling of the world' do enough to demarcate the cognitive from other human institutions? It seems, for example, to cover some aspects of the 'aesthetic' that are not usually regarded as cognitive. Deciding what the limits of the cognitive are would require a lengthy discussion of whether 'the world' that is modelled presupposes some form of scientific or other realism, or whether world models are rightly regarded as cognitive if they answer in a more general sense to

pragmatic social needs. These are questions I cannot go into here, but let us suppose that the institutions that are studied in 'sociology of science' are cognitive in this wider sense, and concentrate attention on how they realize generalized criteria of pragmatic success. This means that scientific 'modelling of the world' may be realized either by some form of natural realism, or by the *construction* of world models for a variety of other social purposes. Within this framework we can even consider the extreme constructivist claim that "the natural world has a *small or non-existent* role in the construction of scientific knowledge".[14]

IV. SYMBOLIC ACTION MODELS

If science is approached as a generalized cognitive system in the way just described, we need to be more explicit about the kinds of explanation of science there are, other than those of 'rational action and belief'. I shall speak of the *symbolic action model* of sociology of knowledge so as to include various explanatory strategies satisfying the following two conditions:

(1) The subject matter of sociology of knowledge is taken to be cognitive in the generalized sense described.

(2) The methodology of the study is intentional, in the sense that it presupposes some recognition of the fact that cognitive systems are human intentional systems, and that the appropriate methods are not exhausted by the search for causal laws.

These types of explanation will include the actors' model, examples of which we have already considered. I shall now look at a rather different perspective on symbolic action, exemplified in the recent collection of papers *Science Observed*. In their Introduction to this book, Knorr-Cetina and Mulkay speak of a methodology they call 'internalist', not in the old sense of being exclusively concerned with self-contained technical arguments and experimental evidence but in the sense of offering *micro*-studies of the totality

of institutional activities of specific groups of scientists, relatively cut off from surrounding society. Such studies parallel the Durkheimian concern with symbolic religious systems seen as "a synthesis *sui generis* of particular consciousness" which "sometimes indulges in manifestations with no purpose or utility of any sort, for the mere pleasure of affirming itself".[15] Similarly, sociologists of science go into the laboratory as if into a strange tribe, and like ethnomethodologists they concern themselves with portraits of closed scientific communities with internally defined universes of meaning.

Some of these studies have taken what Knorr-Cetina and Mulkay call the 'linguistic turn', towards modes of scientific communication and analysis of discourse. Literary and taped sources are taken as data, and claimed to be more 'objective' than the participatory impressions of the ethnomethodologist. Again there is a parallel with social anthropology, where symbolic meaning analysis has given way to structural analysis of discourse – reported and repeated myths, rather than interpreted ritual behaviour become the basic data.

What unites such diverse studies is the search for elaborated structures of meaning or text, the elucidation of which demands neither reference to an 'external natural world', nor further explanation. They are seen as being 'for the mere pleasure of affirming themselves'. The methodology may be more or less positivist or more or less historical, but the epistemology of science is generally constructivist. There is an abundance of phrases like "fact construction", "persuading [scientists] to consider certain propositions as factual descriptions", and "negotiation of reality".[16] There is general repudiation of the standard philosophy of science and of scientific realism as misleading distortions of scientific practice. There is much discussion of scientific artefacts, public rhetoric, power struggles within the laboratory, and between laboratories and funding agencies, and the false consciousness said to be associated with an ideology of positive knowledge.

If we look at scientific institutions from a more macroscopic standpoint, however, this embattled and incestuous picture of the sciences (as akin to the asylum and the prison) can surely not be the whole story. Woolgar[17] rightly complains that the latent aim of constructivist sociology of science seems to be to debunk or demystify science with political purposes in mind, and that if this is the case, the aim should be made explicit and pursued positively instead of by default. And Collins[18] reports the objection to constructivist studies that they are a prescription for anarchy in science. If this objection is to be overcome, there need to be more studies of both the manifest and latent functions of scientific institutions. These will have to take positive account of their overt ideology and its successes, as well as more latent functions that are deviant relative to these. Some of the papers in *Science Observed* begin to provide such explanations. Let us look at two of them.

Still in the constructivist vein, Knorr takes biological evolution as the most useful analogy for knowledge production. Just as evolution shows how creation and progress emerge out of nothing by chance mechanisms, so "it is the thrust of the constructivist conception to conceive of scientific reality as progressively emerging out of indeterminacy and (self-referential) constructive operations, without assuming it to match any pre-existing order of the real".[19] Science as it were 'secretes' a continuously increasing number of entities and relations which constitute the constructed natural world as a cultural object. There are no criteria of selection to be drawn from any fixed scientific method, for method also evolves with world view, and the criteria of success are themselves subject to chance development.

Knorr's conception of the construction of *new* scientific entities is perhaps more relevant to certain forms of advanced science than to the mundane process of finding reliable empirical generalizations. Even so, it is ambiguous. It may refer to the undoubted fact that new entities are *literally* created – new strains of bacteria, new synthetic substances, new elements and isotopes. But the

locution of 'construction' is intended to suggest something epistemologically more obscure – namely the creation of entities in thought, not in reality, and the claim that these then constitute 'our' world. This is a conception that may on the face of it apply to the proliferation of 'entities' in fundamental physics or cosmology (quarks, black holes ...), but there is still ambiguity. Postulation of a hypothetical entity in a scientific theory may be construed more realistically than Knorr allows. Particle models and the like may be taken as ways of expressing lower-level generalizations, and whatever may be the case with the particles themselves, generalizations are intended to state real relations between observable objects, and they are testable by traditional methods of prediction and experiment. As Duhem pointed out long ago, the elementary mathematical laws of light remain locally true whatever model of light is proposed, and to this extent light particle or wave models do refer to reality as interpretations of real, though local, regularities.

But there is a more important objection to Knorr's evolutionary conception, and that is that no analogue is proposed for the functional criteria of 'fitness' or 'selection by survival'. There is no close analogy between chance mutation and biological survival of the fittest here any more than there is in other theories of social evolution. Knorr rightly holds that scientific criteria of 'success' themselves evolve in history, but she is wrong to suggest that they evolve by chance. Their existence and development are socially mediated and socially explicable case by case, and no generalized model of chance mutation is sufficient for their explanation.[20]

With regard to the 'overt' scientific function of prediction and control, Knorr does indeed remark that this is expressed as a manifest function by scientific practitioners themselves, and therefore forms one of the causal and functional bases of explanation. Latour[21] gives a subtle illustration of this in his account of Pasteur and the problem of anthrax. He shows how Pasteur deliberately united the traditional methodology of the laboratory and applica-

tions on the farm, by idealizing the problem of anthrax in the laboratory, and then recreating these ideal conditions on the farm. By so doing, Pasteur effectively *constructed* anthrax as a scientifically and publically recognized problem, whereas before it had been no more than a fact of life – a social plague about which no problem arose because in the vast variety of circumstances where anthrax struck, nothing could be done.

There are constructivist overtones in Latour's paper, for example the suggestion that what counts as 'success' in treatment of anthrax is a statistical artefact. But there are also implicit presuppositions of realism. Anthrax was a collective name for killing and uncontrollable diseases; their incidence was eventually reduced and controlled. To claim that what was regarded as *disease* was itself constructed would be like claiming that the fact that bodies fall is an artefact of Galileo's idealized thought experiments and some invented laws of mechanics.

It is perhaps appropriate to call studies like Latour's *neo-functionalist.* In sociology pure functionalism has been found unsatisfactory, but usually for reasons that apply rather to magical and religious than to scientific systems. There have been objections to postulating latent purposes for societies as such which are not the manifest purposes of any of the individuals concerned. It has been objected that the identification of these social purposes as stability, survival, and the like, puts a premium on conservative values and neglects the facts of social conflict. But in the case of science the first objection does not apply, since the overt purposes of attaining objective knowledge of nature cannot be excluded from explanations as unintelligible, as can the expressed rationale of the rain dance or the ritual murder of the king. As we have seen, the 'received ideology' must usually be taken as part of the functional explanation, and studies like Latour's show how intimately connected are manifest and latent functions in real scientific case histories. It may be true to say that the farmers, the media, the Government, were manipulated by Pasteur by appealing to their own

interests and motivations which were not those of technical science, and Pasteur's own motivations may have been less than purely scientific. Nevertheless, the function of successful prediction and control was served, and Pasteur appears to us as a scientific genius because our society makes this function relatively manifest, and generally speaking approves of it.

In respect of the objection that functionalism presupposes a conservative mode of explanation, it can hardly be said that the manifest purposes ascribed to science in our society favour predictions of stability and survival. Even apart from the threat of scientific holocaust, the official ideology of science is one of continual testing, change and progression, and in spite of the deviations from this ideology documented by sociological study, it has frequently been subversive of tradition and prejudice.

In the case of scientific institutions and practices, therefore, there seems little reason to reject a general form of functionalism that asks for explanations in terms of manifest purposes, as well as the unforeseen causation and latent mechanisms of social evolution. In the sociology of Western societies, overt statements of intention must sometimes be taken at face value, that is, actors' and symbolic action models, constructivism and functionalism must be combined as appropriate in particular cases. Indeed the symbolic action model as I have described it is not inconsistent with most other models in sociology of knowledge under suitable conditions of application. It can even incorporate rational action and realist models of scientific belief in two sorts of circumstances: (a) when actors themselves defend their actions on one or more of these grounds, and (b) when de facto adoption of 'scientific rationality' by actors (even if not explicitly) can be explained in institutional terms by critical methods such as functionalism or structuralism. The important ideological conflicts between types of explanation in the sociology of scientific knowledge are not now between rationalist and symbolic action models, but are rather concerned with the balance between actors' accounts and micro-ethnometh-

odology on the one hand, and macro-explanation by external social and psychological causes on the other.

What, then, are the characteristics of a socialized epistemology? We may draw three conclusions from this survey of recent work:

(i) Where a system of knowledge or cognitive belief is either accepted (as in our own science) or claimed to be cognitive in other cultures or by subgroups in our own culture, there can be internal epistemological study of the relations claimed to hold between data, theory and conceptual frameworks, and external explanation of the genesis of concepts and methods, and of the goals and interests served by the cognitive system. These tasks of a socialized epistemology may be carried out in relation to any socially recognized cognitive system, and depend on the fact that any social system is held together by some set of social rules or norms.

(ii) 'Cognitive systems' can be defined sufficiently widely to capture all symbolic systems or 'models of the world' that are exhibited in mimetic ritual or developed for technical and social purposes.

(iii) The modes of explanation of natural science in a socialized epistemology need not be the methods of positivist science itself, as is assumed in some versions of the strong programme. The whole range of models exploited in general sociology and social anthropology is available, and has been used in recent studies – from the microsociological point of view: the actors' model, the rational action model, and ethnomethodology; from the macrosociological point of view: various types of functionalism, evolutionism, and structuralism. There is no single 'correct' model, only models that are more or less useful and more or less illuminating in particular cases.

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NOTES

[1] This is particularly clear in Quine's 'Epistemology Naturalised', *Ontological Relativity*, 1969, p. 26.

[2] E. Durkheim, *Elementary Forms of the Religious Life*, Eng. trans. 1915, Introduction. E. Durkheim and M. Mauss, *Primitive Classification*, Eng. trans. 1963, was an earlier work in which the aim of socializing 'rational' norms was explicit. It is clear, in fact, that the socialization of epistemology was prior in Durkheim's thinking to his better known socialization of the categories of religion.

[3] See for example, Durkheim's *The Rules of Sociological Method*, Eng. trans. 1938. 'Positivism' will be used broadly in this paper to include all methodologies that maintain the objective, value-neutral stance of scientific explanations, and adopt criteria of empirical test as decisive. I have considered and rejected the claim that sociology of science is a self-refuting enterprise in 'The Strong Thesis of Sociology of Science', *Revolutions and Reconstructions in the Philosophy of Science*, 1980, Ch. 2.

[4] See Douglas's 'grid and group' theory of styles of cognitive thought in *Natural Symbols*, 1970, Ch. 4, and Bloor's *Knowledge and Social Imagery*, 1976.

[5] See the discussion in B. Barnes, *Interests and the Growth of Knowledge*, 1977, Ch. 1. See also the 'weak programme' of D. E. Chubin and S. Restivo, in *Science Observed*, ed. K. D. Knorr-Cetina and M. Mulkay, 1983, pp. 60, 76, which does not presuppose that 'scientific method' is the best way to study science.

[6] P. Forman, *Hist. Stud. Phys. Sci.* 3 (1971) 1; B. Wynne, in *Natural Order*, ed. B. Barnes and S. Shapin, 1979, p. 167.

[7] J. Hendry, 'Weimar Culture and Quantum Causality', *Hist. Sci.* 18 (1980) 155.

[8] See S. Shapin, 'Homo Phrenologicus: Anthropological Perspectives on an Historical Problem', in *Natural Order*, p. 41.

[9] See, for example, P. Pettit and G. Macdonald, *Semantics and Social Science*, 1981, Ch. 1. The rational action model goes back to Weber's dicussion of *Zweckrationalität*, but it should be noticed that Weber supplemented this purposive action by a concept of *Wertrationalität*, that is, action recognized to be of value for its own sake.

[10] R. Needham, *Belief, Thinking and Experience*, 1972.

[11] E. Leach, *Political Systems of Highland Burma*, 1954, p. 14.

[12] R. Horton, 'Tradition and Modernity Revisited', in *Rationality and Relativism*, ed. M. Hollis and S. Lukes, 1982, p. 201; E. Gellner, *Legitimation of Belief*, 1974, p. 157.

[13] This approach is developed by M. Arbib and M. Hesse, in *The Construction of Reality* (an expansion of the Gifford Lectures, Edinburgh, 1982), 1986.

[14] H. M. Collins, 'Stages in the Empirical Programme of Relativism', *Soc. Stud. Sci.* 11 (1981) 3.

[15] *Elementary Forms of the Religious Life*, op. cit., p. 423.

[16] *Science Observed*, *op. cit.*, pp. 101, 123, 63. In his paper 'Irony in the Social Study of Science', *ibid.*, p. 239, Woolgar rightly points out that even social constructivists about science must be realists at *some* level of acceptance of their own social data,

although at any given level they may remain aware of the ironic possibility that *that* level is socially constructed.

[17] *Ibid.*, pp. 254, 261.

[18] *Ibid.*, p. 98.

[19] 'The Ethnographic Study of Scientific Work', *ibid.*, p. 135.

[20] In his forthcoming book, *Styles of Scientific Thinking in the European Tradition*, A. C. Crombie discusses the historical evolution of what count as scientific criteria, from Greek mathematical science to the genetic explanations of scientific biology.

[21] 'Give Me a Laboratory and I Will Raise the World', *Science Observed*, *op. cit.*, p. 239.

ERNAN McMULLIN

THE GOALS OF NATURAL SCIENCE

1. THE BEGINNINGS: BABYLONIA

If one searches in the ancient world for the first traces of what one could in some plausible sense call science, attention immediately focusses on two regions, Babylonia and ancient Greece. Historians have put forward priority claims for each of these areas as the original cradle of science.

The Babylonian side of the story can be said to begin somewhere about the time of Hammurabi in the 18th century B. C. According to the cuneiform tablets, the Babylonians believed that the gods communicated to men by means of omens, both in the sky and on earth.[1]

The typical omen will specify a celestial configuration and then go on to associate with it some significant event on earth. "If the stars of the True Shepherd of Anu (i.e. Orion) scintillate, an important person will become powerful, and commit evil deeds." There is no record, however, in this early period of actual observations.

The omen texts made no attempt to *predict* when the celestial phenomena would occur. They just specified: when the following celestial configuration occurs, expect such-and-such to happen on earth. Nevertheless, the diviners whose responsibility it was to interpret, extend, and hand on the texts must have been alert to changes in the sky.

The great period in Babylonian astronomy comes quite late, during the Seleucid era from 250 B. C.–50 B. C. At this point, astronomy became fully predictive, utilizing mathematical techniques developed a millennium and a half before,[2] and drawing (presumably) on a tradition of planetary and lunar observation

Imre Hronszky, Márta Fehér and Balázs Dajka (eds), Scientific Knowledge Socialized, 27–58.
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that by then spanned several centuries. No attempt was made to follow the *path* of the planets in the sky. Only the 'significant' moments were treated, namely the first appearances of the planets over the horizon, the last sightings, and the stationary points; it seems plausible to regard this restriction as an echo of the omen tradition in which these and only these moments were singled out.

The goal of Babylonian astronomy was *prediction*, nothing more. One can see easily enough how this goal might have been linked to an earlier set of questions the diviners would have wrestled with. They would have wanted to know in advance when the omen events were likely to occur. The desirability of such knowledge must have been evident long before the techniques for its realization gradually began to be forged. Indeed, what strikes us now is the length of time it took for this to happen. Desire, need, interest, is obviously not enough.

What the diviners would *not* have asked was *why* the heavenly bodies moved as they did. If the sky is a celestial message board, the actual movements of the moon and planets are not significant. Only the configurations at the 'message' times count. Perhaps this may help to explain why the astronomers of Babylon apparently never sought the *causes* of the planetary motions, even in contexts – think of the lunar eclipse – where a causal understanding would have aided in prediction. If their astronomy is to count as science – and that too is a question they would not have raised – it must be on the score of its predictive power, its observational basis, its computational techniques. It has no *theoretical* dimension as natural science; it makes no attempt to explain *why* the celestial bodies move as they do.[3]

Let us call this sort of system a *P-science*. Its goal is to organize phenomena as economically as possible with a view to exact prediction. Predictive accuracy and operational convenience are, therefore, the criteria upon which it is to be judged. Its predictions in turn may serve practical ends, so that P-science is likely to have a practical as well as a cognitive goal.

2. THE BEGINNINGS: GREECE

We can turn now to more familiar ground. At a time when the Babylonians were still scrutinizing the skies for omens of what the gods would bring about on earth, their neighbours to the West were beginning to do almost exactly the opposite. They were formulating connected notions of *nature* and of *cause* which would make the world intelligible in its *own* terms. No message from the gods would be needed; their intervention in the affairs of men would not be excluded but would effectively be minimized. Things have a *nature*, a regular mode of acting, and a properly conducted inquiry can reveal the principles of this nature. The goal of such an inquiry is to *understand* the nature, to grasp why the changes in question occur as they do.

The political life of the Greek city-state, historians remind us, was such as to favour open and critical discussion. Opinions were challenged; grounds were sought for disputed views. Radical political debate led to the questioning even of the most fundamental religious beliefs. The outcome was occasionally scepticism but more often a sophisticated appreciation of the argument structures appropriate to the claim being made. The topic of argument itself, dialectics, came to be discussed at length. Lloyd remarks that philosophy and science "can only begin when a set of questions is substituted for a set of vaguely assumed certainties", and so "they may be represented as originating from the exceptional exposure, criticism and rejection of deep-seated beliefs".[4] The dominant question becomes the epistemological one: What kind of knowledge can you claim?

The distinguishing mark of science, as the Greeks came to define it, was thus the quality of the knowledge it embodies. *Epistemé* is the best in the way of knowledge that can be attained. Aristotle saw no reason why it could not be "eternal and necessary as geometry seemed to be", and so the ideal of a definitive science of nature was born. Such a science would be *demonstrative*. It

would *rest* on premisses intuitively grasped as true, and proceed by deductive rule. The first explicit goal is therefore certainty; though the scientist must make use of dialectical reasoning along the way, there is no place for it in the finished structure of the science.

The second characteristic of science is to be its search for *causes*. Historians have noted that the Greek term, *'aitia'*, which came to have the quasi-technical meaning among philosophers of *cause*, was originally a term denoting personal responsibility. To ask for the *aitia* of an action was to ask: who is responsible for it? In certain contexts, it could mean: who is to blame? who is guilty?[5] The conviction that had animated Ionian science was that every event has a cause, or as Leucippus put it: "Nothing comes to be at random, but everything for a reason and by necessity."[6]

Aristotle's doctrine of four 'causes', or four complementary ways of explaining change, is too well known to need comment. "The causes being four", he remarks, "it is the business of the physicist to know about them all".[7] The goal of natural science is thus to grasp the *causes* of change, to *explain* it in terms of one or other of the four canonic patterns of explanation. I will call Aristotle's ideal natural science a *D-science*. Its goal is a knowledge of causes where knowledge is construed as demonstration and causes are thought to be directly grasped.

Prediction plays no particular role in a D-science. Testing is not needed, since the premisses are intuitively seen to be true once their constituent concepts are grasped. Of course, if the nature of a being is understood, one will be able to 'predict' what the normal activities of that being will be. But this is prediction only in a very weak sense. There is no suggestion that a *goal* of the science is to enable us to discover some outcome we would not otherwise have known. The goal is *contemplative*, a *theoria*, not in the sense of being constructed in isolation from experience (Aristotle's D-science is construed as resting directly upon experience), but in the sense of its being an intellectual grasp, an understanding, of some part of nature.

Did Plato and Aristotle ignore the claims of P-science entirely? The astronomy of Plato's contemporaries had not yet really attained any predictive power, so the question did not perhaps arise for him directly. His treatment of observational astronomy is, nevertheless, quite puzzling. According to Simplicius, he is willing to allow that astronomers may "save the phenomena by using combinations of circular motions".[8] But in the *Republic* he appears to contrast this sort of astronomy with what he somewhat mysteriously calls 'real' or 'true' astronomy.[9] Observational astronomy cannot qualify as knowledge, he insists: "If anyone tries to learn about the things of sense, whether gaping up or squinting down, I would never say that he really learns; for nothing of the kind admits of true knowledge."[10] Even though the stars and planets are "the fairest and most exact of material things", "we must recognize that they fall far short of the truth, the movements namely of real speed and real slowness in true number and in all true figures ... These can be apprehended only by reason and thought, not by sight".[11] Predictive astronomy can at best serve only as propaedeutic.

Does Plato really seek an astronomy which would not rest on sense observation? Or is he only saying that reason has to contribute the geometric figures in terms of which the observations can be converted into a proper science? His pupils appear to have taken him in this latter sense, but the texts we have are at best equivocal. And his assertion that it is pointless for the observational astronomer "to strive to determine the truth (about the stars) by every possible means"[12] suggests that he had failed to grasp the importance of the highest standards of observational accuracy attainable. All in all, then, it seems fair to conclude that Plato's views on sense knowledge blocked him from taking seriously the notion of a 'science' which would have empirical accuracy as a primary warrant.

Aristotle made a more successful effort to incorporate the troublesome science of astronomy in his account of natural knowl-

edge, but in the end he failed also. He devotes a chapter in the *Physics* to the differences between the physicist and the mathematician, and gives over astronomy to the mathematician. Those disciplines which make use of mathematics in their discussions of nature – he lists optics, harmonics, and astronomy – are, he says, the "more physical of the branches of mathematics",[13] not (as one might have expected) the more mathematical parts of physics. Since they draw their principles from mathematics, they pertain more properly in his view to mathematics than to physics. And since he is insisting on a fundamental distinction between physics and mathematics on the score of (among other things) their differing levels of abstraction from matter, those disciplines which use mathematical methods in discussing physical topics seem not to fit properly anywhere in his scheme of the sciences.[14] They seem to be uncomfortably intermediate, 'mixed' or 'middle' sciences as they will come to be called later.

But where does this leave his physics? Is he interdicted from using mathematical concepts in it? It does not seem so; they occur frequently in the pages of the *Physics* and the *De Caelo*. And he himself remarks that it would be absurd for the physicist to try to discover the natures of the sun or moon without taking into account their spherical shape.[15] Yet if he cannot incorporate the results of mathematical astronomy into his account of the planetary motions, what sort of science can he have? It is at this point that the tensions between P-science and D-science come fully into view.

Aristotle derived his astronomy mainly from Eudoxus (c. 408–355 B. C.) who was the first to try to account for the paths of the sun, moon, and planets, in terms of combinations of circular motions. It seems unlikely that Eudoxus ever succeeded in using this ingenious model to predict, except in a very rough way.[16] But what he *did* succeed in doing was to suggest a mechanism whereby the complex irregular motions of sun, moon, and planets might in a general way be understood.[17]

Aristotle took over the system of Eudoxus as amended by Callippus,[18] and tried to unify it into a single dynamic system. He obviously thought that the enormous increase in complexity he was proposing was offset by the unification of the system into a single mechanism, and by the promise this gave of a complete mechanical explanation.

A mechanical explanation tracing all motion upward to a First Mover could not, in fact, work.[19] Perhaps this is why Aristotle in the *Metaphysics* tries a quite different model in which motion in the celestial realm is brought about not by contact but by desire, and a separate Unmoved Mover is associated with each sphere, the sphere itself being assumed to be capable of desire.[20] This represents, once more, an enormous increase in complexity over the simple cosmology of the *Physics* which terminates tidily in a single Mover for the cosmos as a whole. One can see how high a price Aristotle was willing to pay for a model that, at least in principle, could actually *explain*[21] why the system worked as it did, even though the style of explanation used had to be an unhappy blend of the mechanical and the teleological: "only thus is it possible for the whole system to produce the revolution of the planets".[22]

Aristotle seems to have been aware of the limitations of the awkward union he was attempting between (in our terms) D-science and P-science. A D-science ought to 'conclude with necessity'. But how could a model that depended on the cumulative results of observation as well as on the ingenuity of the geometer ever do this? In his own count of the spheres, after proposing 55 as the total and then adding a proviso that 47 might do, he concedes that this is only 'probable': "the assertion of necessity must be left to a more powerful thinker."[23] And then with a return to his characteristic confidence, he adds: *whatever* the number of the spheres, this *must* be the number of Unmoved Movers. And since *all* movements must trace back to such Movers, "the end of every movement will be one of the celestial bodies which move through the heaven".[24]

This is an enormously ambitious claim. In retrospect, it is easy to see how vulnerable it was, and just *why* it was so vulnerable. The explanations it suggests had an intuitive plausibility to them: after all, contact action and response to desire are the two sources of motion we can most easily identify in everyday life. But there was no real testing of the insights; there was no way that a provisional insight could be systematically evaluated; there was no source of insight save ordinary mid-level human sense experience. In consequence, when there was a *conflict* between the findings of D-science and P-science, there was no way to arbitrate. Aristotle had found a temporary way to patch the differences. But it could last only until P-science developed sufficiently for it to become impossible for even the most ingenious D-scientist to hook the two together.[25]

3. A SCIENCE OF ASTRONOMY?

Over the next few centuries, progress in astronomy was slow because of the difficulties in making and compiling observations of the accuracy required, and even more because of the daunting complexity of the spherical geometry needed to bring the observations into a single order. The most significant development was the introduction by Apollonius and Hipparchus of two alternatives to the concentric geometry of Eudoxus. These were the eccentric and the epicycle,[26] each of which employs *two* uniform circular motions instead of one, so that there is a second center of rotation in the system, a mathematical point-center of no apparent physical significance. Their main advantage was that they allowed the distance and hence the brightness of the planets to vary periodically, which the concentric model did not. Since the brightness variations were known, even in Aristotle's time, this failure of the concentric model came to be recognized among astronomers as a major fault of the Aristotelian system.[27]

By this time, the tensions between D-science and P-science were coming to be noted. Writing in the 1st century B. C., Geminus asserts that it is physics that reaches the truth of things. The astronomer can only construct hypotheses to save the phenomena: he cannot ask questions about causes. Epicycles and eccentrics, though geometrically different, can be used equally well to predict. The inference is clear: the geometrical models of themselves are means of prediction *only*.

The tension came to a head, finally, in the work of Ptolemy, the great Alexandrian astronomer of the 2nd century A. D., in whom the full traditions of Babylon and of Greece were finally to meet, and who made the most sustained attempt to blend them. In the preface to the *Almagest*, he draws a quite new distinction between physics and mathematics. Physics has to do with the realm of the corruptible and the qualitative and can thus yield only conjecture, not a full understanding. Mathematical science treats of the quantitative aspects found in all things and capable of abstraction from the sensible order.

He thus reverses the order given by Geminus: now it is to 'mathematics' that we look for the truth of things. But, of course, it is to a 'mathematics' greatly enlarged, so enlarged that it would incorporate much of the physics of Aristotle. In Book I of the *Almagest*, for example, Ptolemy gives a set of proofs of the immobility of the earth at the center; they are similar to those given by Aristotle in the *Physics*, but they are evidently taken here to pertain to 'mathematics'.

But can this sort of proof give the kind of certainty that D-science can? Ptolemy acknowledges that it cannot, but he is obviously convinced that this is the best that can be done in the understanding of the heavens. A summary outline of the 'hypothesis' of the earth's immobility will suffice, he says, because it and his other basic hypotheses

will be completely confirmed and further proven by the agreement with the phenomena of the theories which we shall demonstrate.[28]

This is a long way from D-science. Here it is the *predictions* that carry weight.

The *causal* character of D-science has also had to be sacrificed, a much more serious loss. Though he frequently says that the circular motions he attributes to the planets are 'by nature', his epicycles had no plausible causal interpretation. Further, he had to place the center of the planetary deferent circles at an eccentric point away from the earth, and worst of all, suppose that the motion of the center of the epicycle on the deferent circle is non-uniform,[29] thus giving up the last remnant of the uniform circular motion on which the old D-science of astronomy had been built. By the time the last book of the *Almagest* is reached, Ptolemy is pessimistic about the cost in terms of simplicity and of causal plausibility that the predictive criterion of saving the appearances has entailed.[30]

So we are back to pure P-science again, one that, as Ptolemy insists, does give us a truth about things, even though the claim to explanatory power has been virtually abandoned. There are no carrying spheres, only circular motions, some of them of imaginary points or around imaginary points. The phenomena have been saved,[31] but the devices used cannot be interpreted causally. And the only available *causal* system, that of Aristotle, has been refuted, since it has failed to save the phenomena. From Ptolemy's point of view, one is not at liberty to use *both* systems, one for prediction and one for explanation; the concentric model is simply *wrong* on his criteria.

For Averroes, Aquinas, and other medieval thinkers who discussed this discrepancy, what made it especially difficult to allow that physics could learn from astronomy was their clear perception that in astronomy one had to argue from effect back to cause. In such an argument, as Averroes put it, "the principles are hidden from us (and) are in no way necessitated by the known effects".[32]

But this is incompatible with that other feature of D-science, the demand that inference be demonstrative, that it proceed from cause to effect. "In astronomy", Aquinas remarks in the *Summa Theologica*:

> We account for epicycles and eccentrics by the fact that we can save the sensible appearances of the heavenly motions by this hypothesis. But this is not a really probative reason, since the apparent movements can, perhaps, be saved by means of some other hypothesis.[33]

What is striking to us now as we look back at the long record of astronomy between Ptolemy and Galileo is how little the natural philosophers worried about the fact that their spheres lacked any sort of predictive force and how little concern the astronomers in their turn showed about the fact that their epicycles were incompatible with the causal principles of the best physics of the day. It was not as though the philosophers could have been unaware that a true account of the causes *ought* also to be predictive, or that the astronomers could have been indifferent to whether their geometrical models represented real motions or not. But each side bracketed the awkward questions, and went about its own business, as professionals are supposed to do.

4. THEORETICAL SCIENCE

I am going to proceed immediately now to the denouement you have been expecting, the coming together of explanation and prediction in the new science of the 17th century. Focussing on astronomy first, four stages can be distinguished. First, Copernicus's astronomy was still caught in the Ptolemaic dilemma: it predicted planetary motions, but did not explain them causally. And Copernicus was not willing to accept the fictionalist status for his astronomy that so many of his predecessors had settled for. Though his system had clear explanatory advantages over that of Ptolemy,

he could not give a causal interpretation of the epicycles he was still forced to use. Nor could Galileo, who though he had successfully undercut Aristotle's physics, had not yet been able to fashion an alternative causal account of his own. His union of celestial with terrestrial physics gave every reason to expect that the long separation between physics and mathematical astronomy was over. But it must have seemed hopeless to try to explain epicyclic motion causally if even the simplest notions of fall were as yet only described, not accounted for in causal terms. It was unclear to Galileo how causal explanation should proceed in mechanics, so in the interim he built his kinematics without it.

It was Kepler who finally did away with the epicycles and the eccentrics, and the mathematical construct of a 'mean sun' which Copernicus had been forced to postulate as the center of his planetary system. By showing that the planetary orbits were elliptical, he made it plausible that a single sort of causal action centering in the sun might be responsible. And it was Newton, of course, who carried this speculation through to the level of science. But what kind of science? Something of the old ambiguity still remained. Newton's celestial mechanics was assuredly predictive. But was it explanatory?

Many of his critics did not believe it was. They argued that his concept of attraction did not really *explain* why the planet circled the sun. Berkeley was convinced that the new mechanics was a P-science, no more: it gave no causal understanding of motion at all in his view. Even Newton himself in the *Principia* asked the reader to lay aside the question as to what sort of causes these attractions were, and to focus on the descriptive mathematics he had provided for the motions. We know enough of Newton's own thinking, however, to be sure that he *did* take his dynamics to provide a causal account even though he realized fully well that his forces could only be specified through their effects.[34]

If the only natural sciences were mechanics and astronomy, it would be hard to decide *what* sort of science resulted from the

union in Newton's *Principia* of the two disparate lines going back over the centuries to Babylonia and to Greece. But fortunately there were other natural sciences even in the 17th century, and it is much easier to decide in the case of the others what the goals of the new science would be and how they relate to those of the D-science and P-science that preceded it.

Let me retrace my steps just a little first. The reason that natural philosophers in the Aristotelian tradition had tended to regard the epicycles of Ptolemy as fictive practical devices was *not* merely because they were arrived at in hypothetical fashion or because they were thought to be predictively equivalent to other possible geometrical models. It was because they were *incompatible* with what was thought to be the true explanatory account, the D-science of concentric spheres.[35] Nevertheless, it is true to say that it was in astronomy that the issue of the validity of arguing from effect back to cause in a hypothetical way was most squarely joined, as the famous texts in the *Posterior Analytics* about the nontwinkling character of the planets and the manner of waning of the moon already indicated right at the beginning of the story.

The more general issue of *ex suppositione* (or hypothetical or consequential) reasoning was extensively debated throughout the Renaissance. But what blocked any sort of resolution was the fixed presupposition that somehow the equivalent of a demonstration *had* to be reached, that the *regressus* from effect to cause must be made to yield the sort of certitude one could immediately have in a *propter quid* argument from causes to effect.

The turning-point may be said to have come with Descartes. His experience with such sciences as optics, meteorology and physiology, convinced him, to quote the famous lines from the *Discourse on Method*, that:

> The power of nature is so ample and so vast, and these principles (of my science) so simple and so general, that I almost never notice any particular effect such that I do not see right away that it can be derived from those principles in many different ways.[36]

Since each of these different ways yields a different cause, demonstration is impossible. This conclusion was reinforced by the theological voluntarism which led him (as it had done many earlier thinkers) to assert that God could have brought about the effects we perceive in an infinity of causally different ways. What, then, was to be done?

> I know no other expedient than again to search for certain experiments which are such that their result is not the same when we explain the effect by one hypothesis as when we explain it by another.[37]

Bacon had already shown in much more detail in the *Novum Organum* how this sort of systematic experimental testing of hypotheses might be carried out. And as the century wore on, this method of working back from effect to causal hypothesis, which is then tested by drawing consequences that can be observationally checked, became more and more the norm in optics, in chemistry, in the physics of gases, and elsewhere.

But there were two reasons, in particular, why the method of hypothesis was gradually recognized to be required, why the hesitations of the centuries on the part of those who held out for a demonstrative science were now so firmly set aside. The first was the telescope, an almost accidental discovery, accidental at least in its timing relative to other developments in thought at the time. It raised issues about the nature of distant objects, like sunspots or comets or lunar shadows, which could not possibly be handled in any other than a hypothetical way. The conditions for an Aristotelian D-science clearly could *not* be realized, since an intuitive grasp of these distant natures could in no way be attained. Only a hypothetical argument, tested by the predictions drawn from it, and not by any quality of self-evidence or perceived necessity, would suffice.

Even more fundamental was the almost universally shared conviction that the perceived qualities of things *had* to be explicable in terms of minute corpuscules, themselves imperceptible to us. It

is not easy to explain just why this corpuscular philosophy so quickly took hold in the 17th century, since it had as yet (and indeed for a long time to come) no really testable consequences. But the success of the new mechanics made it plausible that *if* the macro-properties of bodies were to be explained at all, it would have to be by postulating an 'invisible realm', as Newton called it, whose properties and motions accounted for the perceived qualities of things and thus made them in an ontological sense secondary. It was evident to all that this realm could *only* be approached by the tentative methods of hypothesis testing. And to the more empiricist-inclined philosophers like Locke, it seemed as though the prospects of this yielding much in the way of knowledge, were not bright.

What we have in the making here is, of course, a new ideal of science. The goal can still be expressed in Aristotle's phrase as 'a knowledge of causes', but there are two crucial modifications, one in the term 'knowledge' and one in the term 'cause'. The *knowlegde* that science yields is no longer viewed as demonstrative, as definitive, as necessary. It is probable, fallible, dependent upon continued testing. True, this will continue to be disputed until well into the 19th century, not least because Newton's mechanics seemed to many to provide at least one instance of an old-fashioned D-science. But the notion that testing is needed, and thus that prediction is now a *necessary* part of science, was grasped, I am tempted to say once and for all, already in the 17th century. No longer could prediction and explanation be sundered as they had been for so long. Now the main testimony of an acceptable explanation will be precisely its ability to *predict*, that is, to entail testable consequences. Note that this was an *empirical* discovery; it had to be shown that a science fully joining the goals of explanation and prediction was *possible*. It could plausibly be argued that this was one of the most revolutionary discoveries in that century of 'scientific revolution'.

The transformation in the notion of cause was equally fundamental, but more ambiguous. Tracing observed effects back to causes not themselves observed required a *theory*, and in time the notion of a 'theory' came to mean primarily the product of this sort of retroduction from effect to postulated cause. The cause in such a case is defined by the theory, and its warrant is no more than, and no less than, the theory itself. The cause is thus what came later to be called a 'theoretical entity'. The minute particles of the corpuscular philosophy would be 'causes' in that sense. They bring about, explain, cause, the observed effects. They are agencies postulated for the very purposes of explaining. They resemble, therefore, the efficient or agent causes of the Aristotelian scheme; where they differ is in being 'hidden', that is, not given in observation. They may be constituent parts of the entity whose properties are being explained, or they may be distant or imperceptible entities like sunspots or ethers, whose nature or even whose existence can be inferred only in a theoretical or indirect way.

The notions of theory and theoretical entity are thus central to this newly emerging model of science, which we can call *T-science*. In T-science, the predictive power characteristic of P-science blends with the explanatory force characteristic of D-science. There can no longer be a science which merely predicts, or one which merely explains. If it predicts but without being able to specify a causal substructure, it may be useful but it is not science in the full sense. If it purports to explain but does not permit testable consequences to be drawn, there is reason to doubt the genuineness of its causal claim.

When I say that the goal of T-science is a knowledge of causes, I do *not* mean 'cause' to be taken as Hume took it. Or if I were to say that T-science aims at theoretical explanation, I would not want 'theoretical' to be construed as 'deductive-nomological', to jump to the modern version of the Humean doctrine. What is wrong with that doctrine is that it does not do justice to the rich-

ness of theoretical science, even the science of Hume's own day, let alone the immensely complex structural sciences that began to take shape in the 19th century.[38]

5. SOME MORALS OF THIS STORY

And now it is time to draw some morals from this long tale, morals that bear on my topic: 'The goals of natural science'. But before I do, a few necessary clarifications first. When I speak of the goals of *science*, I am thinking primarily of science as an activity on the part of a skilled community, not of science in its other, commoner, sense as a body of propositions set down in a textbook or research article. And I am taking the term, 'goal', in both of the main senses of that slippery term. There are the *ideal* goals (also called explicit or acknowledged), the goals that scientists expressly specify as the aims of their work. And then there are the *actual* goals which are embodied in the activity itself, somewhat as a *telos* was thought by Aristotle to be embodied in all natural motion. The ideal goals can be discovered from what scientists *say*, though there can be considerable disagreement within the community of scientists in regard to them. To learn the actual goals, one must scrutinize what scientists *do*, and try to determine from the structure of the activity itself what would count as 'success' in it, as I tried to do with Babylonian astronomy. There is room for disagreement regarding the actual goals of a particular instance of scientific activity also, mainly because different interpreters will estimate success differently.

A striking feature of Greek science was the divergence between the ideal goals (as enunciated in the *Posterior Analytics*, for example) and the actual goals of the enterprise so far as we can reconstruct it historically. But that sort of divergence is not peculiar to Greek science. One finds it again in the work, say, of Descartes or Newton. Even today, we tend to distrust the pronouncements

of major scientists on the methods and goals of science, 'distrust' in the sense that their reflections do not always do justice (so far as one can tell) to what is actually going on in their laboratory or study. Einstein's aphorism about watching what scientists do rather than listening to what they say, comes to mind, and is particularly apposite, it happens, in regard to Einstein himself. Historians have to take into account both sorts of goals, and weave them together in one way or another in making sense of deep changes in past science.

And now, at last, for the morals of my story.

Moral 1: The goals of science have changed fundamentally over the centuries. What leads me to speak of *change*, rather than discontinuity or simple pluralism, is the underlying continuity provided by communities of scholars and researchers, formed by the same books, taught by the same methods, in active communication across space and time. When I say that the goals of science have changed, I mean that there has been a gradual evolution in method and expectation from one generation to the next, or sometimes within the same generation. What permits me to speak of *science* across all of these changes of the years, despite the wide differences between the aims of the Babylonian astronomer and those of the Florentine physicist, is that there is a living continuous link across the centuries, as the communal activity of inquiry into nature is gradually transformed. To direct the same term, 'science', to all of this diversity is not to render the term equivocal.

Moral 2: What brought about the changes of goal were for the most part *reasons* internal to the activity itself. One adopts a goal only if it appears desirable. One modifies it if it proves not to be achievable as it stands. One may not know that a certain activity *is* desirable.

But once the activity begins, and given the social and economic conditions that permit its continuance – a crucial proviso – what will mainly tend to modify it (so it would seem) will be discoveries made by the practitioners themselves. For example, it may gradu-

ally be realized that the ideal goals that have been propounded are in fact not achievable as they stand. This is what happened with D-science in the 17th century. The transition was very slow; it was nothing like a Gestalt switch when it came. One finds scientists of the day living in both worlds, as it were: effectively constructing elements of a T-science (as Galileo did in fields other than mechanics) yet speaking as though demonstration of the old sort could still be attained.[39] Some attempted the old ideal of science in one area, usually mechanics, while abandoning it in others. Kant's work provides perhaps the most striking example of this sort of divided mind. In the *Metaphysical Foundations of Natural Science* he came as close to a D-science of mechanics as anyone has ever done, while relegating such fields as chemistry to the lowly empirical of a P-science.

Or the goals may be modified by extending them in ways that seem profitable. Ptolemy realized that the predictive goal of Babylonian science could be achieved in a different way if astronomers could establish continuous orbits for the heavenly bodies and not just privileged points like risings and settings. This allowed the use of more powerful geometrical methods, replacing the limited and unexplained arithmetical computation devices of his predecessors. But this began to shift the science in an explanatory, and ultimately a theoretical, direction since the orbits are theoretical entities, and questions immediately pose themselves about the reality of the paths attributed, as well as about the agencies involved in making the planets follow the paths, questions that simply never arose within the simpler perspective of Babylonia. Again, instruments like the telescope and the microscope may be discovered which open up a whole new realm of inquiry, an inquiry which cannot perhaps be carried on in the traditional way. New or modified goals then impose themselves if the inquiry is to have an acceptable outcome.

What makes this second moral timely is the tendency on the part of recent theorists of science[40] to argue that the goals of sci-

ence, like the theories of science, are shaped by (and ultimately explainable in terms of) cultural and economic factors external to science understood in the traditional cognitivist sense. That such factors are important in this context cannot be gainsaid. We have seen that the origins of predictive astronomy lie deep in Babylonian religious beliefs. The Babylonians were not originally interested in prediction but only in the correlating of certain celestial events with significant happenings on earth. Likewise, the initial attitude towards traditional beliefs in medicine and religion so important in the genesis of natural science in Ionia may well have been conditioned by political and social features of the Greek city-state. And the search for eternal and necessary truth that influenced the formation of the ideal of D-science may well have been partly religious in origin.

But all this having been said, it is important to recognize that *changes* in these goals, once the goals themselves came to be articulated in a successful communal practice, seem to have been much more due to factors internal to that practice. There is no evidence that the formation of the T-ideal in the 17th century owed anything *in its content* to cultural factors. I say 'in its content' because it is trivially true that the practice of science, then as now, depended on the socio-economic base of the society, as well as on the traditions of learning in that society. It was no accident, one supposes, that the North Italian city-states furnished a fertile ground for scientific activity during the late Renaissance. But this in no way implies that the content of science or the articulation of its goals derived in general in some differential way from one sort of cultural background rather than from another. We may want to say – indeed we *must* say – that the activity of scientists is causally related in all sorts of ways to broader psychological and social regularities. But this is *not* to hold that changes in the goals of that activity – Descartes' adoption of hypothetical methods, for example – can be explained by recourse to facts about French culture in the early 17th century.

Moral 3: The goal of technical control played virtually no part in the origins of science. It is usual to point to the social separation between artisan and full citizen in the ancient Greek world, and to the craft mysteries which excluded outsiders from the secrets of the high technological skills already found not only in Greece but in at least as developed a state in many other parts of the ancient world. But the matter runs deeper than this. The Greek D-science did not depend for its warrant upon the test of consequences. And the gap between natural science, as Aristotle conceived it, and actual technological control was so great that the idea that one might actually inform the other simply would not have occurred to anyone. Of course, there were people like Archimedes and Hero of Alexandria whose inspiration was not Aristotelian. But in general, the structure of D-science was such that the supposition that its goals *ought* somehow to include control of Nature, if it were to make a genuine claim to an understanding of Nature, a supposition that seems entirely plausible to us, was blocked right from the beginning.

There is a tendency sometimes among praxis-oriented contemporary writers to dismiss Greek science because of its being, as they say, 'contemplative' in its goal. This term can easily mislead, however. It is true that the ideal of *theoria* was knowledge for the sake of knowledge, for the pure pleasure of understanding. Aristotle argued (at times, at least) that understanding of this kind constituted the highest human goal, the goal that set man closest to the Divine.

But the further implication sometimes given the term that science itself was thought of as an armchair study, as something to be pursued primarily by inward reflection, is of course false. Aristotle was nothing if not an empirically-inclined researcher, whether he was dealing with marine organism or with political constitutions. D-science was supposed to begin from premisses anchored by an intuitive grasp, an *epagoge*, of the concepts involved. But Aristotle made it quite clear, both in his discussions of method and in his

own practice, that such a grasp did not come easy, that it often required a long experience with the matter involved. We might criticize him today for his failure to test some of the claims he made about specific nature. But we can only marvel at the breadth and intensity of his lifelong involvement in the most minute details of the natural order.

If the goal of technical control played very little direct role in the origins of science, what about the emphasis given it by such writers as Bacon in the 17th century? "The true and lawful goal of the sciences is none other than this: that human life be endowed with new discoveries and powers."[41] Here we have to be careful. It is true that Bacon hailed the promise of the new sort of knowledge for the improvement of the human condition, for undoing (in his view) the damage done to man by the Fall. It is also true that one of his strongest criticisms of the older natural philosophy was of its inability to produce any tangible fruits in the technical order. But he emphasizes more than once in the *New Organon* that in the order of the new science, understanding must come first.

Bacon believes that the process of inquiry may be distorted if too much emphasis is given too soon to matters of practical application. Technical control is to be a *consequence*, and in that sense a touchstone, of the new science rather than a goal in its own right.

Furthermore, in a century so concerned with the phenomena revealed by the telescope, it would hardly have occurred to anyone to make control a *general* goal of science, in the sense in which this is often done today. The phrase 'prediction and control' is dotted throughout the pages of neo-Marxist theorists like Habermas or sociologists of science like Barnes, as though somehow the two always go together. But scientists of Galileo's generation, both in word and practice, made it clear that astrophysics (as we have come to call it) aims solely at an *understanding* of the phenomena of comets or sunspots or novae. In no sense was it pursued because of a hope of technological advantage.

It would only be in the 19th century that the new sciences for the first time began to fulfill the promise that Bacon had held out for them of guiding and accelerating technological change. It proved to be a far longer road from theoretical understanding to the details of the material processes that affect human life than Bacon or Descartes or the French encyclopedists had assumed it would be. In a real sense, it had to be discovered that it *was* possible. Though logic might suggest that a science tested by its consequences ought to revolutionize a technology built solely on craft skills, there was nothing a priori about that happening in this particular possible world.

Moral 4: Scientific rationality is discovered in the process of attempting to realize the goals of science. It has become customary today to distinguish between 'logicality' and 'rationality', between the formal processes of deductive logic and the much broader repertoire of cognitive procedures that the scientist requires. Rationality involves the assessment of the validity and significance of experimental evidence; it extends to such requirements as reproducibility and to such techniques as idealization. Above all, it requires the skills of theory appraisal, skills more akin to the discerning of a value than to the application of a rule.[42] By now, I suppose, it would be generally admitted that the criteria in terms of which a theory is assessed, such criteria as predictive accuracy, coherence, consistency, fertility, simplicity, do not function as an algorithm, as a set of rules of inference. This is what allows two sets of scientists looking at the same evidence – and I mean the *same* evidence – to support two different theories relative to that evidence.

Now all of this was already becoming evident in the 17th century. Once one moves from the demonstrations of D-science or the mathematical predictions of P-science to the assessment of hypothesis characteristic of T-science, a new approach to evidence is needed. Indeed, something of this had been grasped long before in the discussions of *ex suppositione* reasoning during the medieval

period and in the rich tradition of analysis and synthesis going back to Aristotle himself. But it was only when hypothesis became the accepted method of working scientists that one begins to find detailed discussions of the criterion of simplicity or of the weight to be given to fertility, for instance. One finds them in Bacon, in Descartes, above all perhaps in Kepler, who in the *Apologia Tychonis* provided perhaps the most elaborate account of hypothesis assessment given by any working scientist up to that time, an account which derives almost entirely from his own practice.[43]

There has been much discussion in recent years as to whether the rationality of science changes over time, and if so, what this does to traditional claims for the objectivity of science. In *The Structure of Scientific Revolutions* Kuhn argued that the choice between competing paradigms "cannot be determined merely by the evaluative procedures characteristic of normal science, for these depend in part upon a particular paradigm, and that paradigm is at issue".[44] The implication of this is, of course, that the evaluative procedures of science change when paradigms change. In a later paper, however, he says that "the values deployed in theory-choice are fixed once and for all, unaffected by their participation in transitions from one theory to another".[45] He allows that *small* variations may occur, but then goes on to make a striking comment:

> Though the experience of scientists provides no philosophical justification for the values they deploy (such justification would solve the problem of induction), those values are in part learned from that experience and they evolve with it.

This comment enables me to situate my own position in regard to these values as well as to the broader rationality of which they are the key element. This rationality *is* learned by the experience of scientists, as Kuhn says, and as I have tried to illustrate here in detail. Furthermore, the evaluative procedures *did* change markedly as the community of scientists moved from D-science to T-sci-

ence, and later, as the new procedures were debated and tested out throughout the 18th and 19th centuries.

The values involved in theory appraisal and the rationality of which they form a part are in the end *instrumental* values, means to the ends science in general is expected to realize. They are not ends in themselves. They can be justified only by the extent to which they further the goals that science is taken to aim at. And the justification here is in the first instance of a practical sort, as scientists gradually learn how best to understand these epistemic values, what relative weights to attach to them, and perhaps what other values might also serve as a means to further these goals more effectively.

Does making rationality relative in this way to the goals of science concede to the sceptical challenge of the social constructionists or of Feyerabend? I think not. The goals of science have developed, as we have seen, in a progressive and reason-giving way. They embody basic human interests: the changes, as we have seen, have come mainly from the realization that the goals would have to be modified in order that something *like* the original goals might still be achieved. The crucial point is that we can tell when ideal goals *are* being achieved or when actual goals are being more effectively reached than *before*. The goals of theoretical science have been more and more successfully reached, in their *own* terms, from the 17th century onwards. This is the sort of warrant on which the rationality of science rests in the first place. But it is not its only warrant. And that brings me to:

Moral 5: The rationality of science can be philosophically justified. Here I disagree with Kuhn. What philosophers of science have laboured so long to show is that such values as fertility *are* an appropriate criterion of theory. Their arguments are in a broad sense logical or epistemological. Whewell more than a century ago, made an elaborate theoretical case for the 'consilience of inductions' as a desideratum in evaluating theory at least as important as empriric-al accuracy. What happens in philosophy of science reflects at the

second level what happens in science itself. That is, it is empirically discovered in scientific practice that certain kinds of evaluative procedures or of epistemic demands (like the reproducibility of experimental results) are effective in bringing about the broadly-stated goals of science. Then a theory of a philosophical sort is constructed to *account* for this. If the theory is a good one – and this raises further issues – it serves as an indirect and tentative justification of the practice itself.

In the end, then, historicism and logicism may still converge. We may still be able to construct a philosophy of science that derives *both* from the learning that has gone on in history *and* from a more general logical and epistemological framework. Logic or epistemology of themselves would never have had the resources to come up with anything like the complex and pluralistic scientific methodology that has characterized the natural sciences in recent centuries. On the other hand, that methodology is not just a contingent empirical fact; it can be 'explained' in the rather special sense in which philosophical theory constitutes an explanation. There has been, of course, much disagreement as to what the best theory is here, a disagreement that has been particularly marked in the last two decades. But even the possibility of a good theory here would suggest that philosophy of science can take on a normative role as it surveys the diversity of historical and contemporary scientific practice. I need hardly say how fragile a balance this makes between the historical and the logical, the descriptive and the normative. But one moral no one *expects* me to draw, I hope, is that philosophy of science ought to offer simple solutions.

You will have noted that I did not carry the historical story much beyond 1700. Were I to extend it as far as the present day, the principal development I would want to grapple with is the growth of questioning in our own century, from sources in philosophy, in history of science, and from within science itself, in regard to theoretical entities. The outcome of such questioning *could* be a substantial modification in the goals of T-science, a

dilution of its theoretical component for some parts of science, like quantum mechanics, at least. In its most radical forms, as for example in classical instrumentalism or in the constructive empiricism recently proposed by van Fraassen, the distrust of theoretical understanding would lead us back to something very like the minimal goals of P-science.

There is no way to handle these issues without looking at both the practice of contemporary science and the objections brought against realism from so many different quarters. In his most recent work, *Realism and the Aim of Science*, Karl Popper has this to say: "The task of science, which is to find satisfactory explanations, can hardly be understood if we are not realists."[46] Critics of scientific realism have perceived this link quite clearly, and have argued either that explanations, so construed, is *not* in fact a proper goal of science, or else that it does not furnish a sufficient warrant for realism. The debate has been an unsatisfactory one, not least because the term, 'realism', has proven almost infinitely elastic. It is striking, for example, to find so many of those who begin by announcing that realism is dead ending with an 'acceptable' version of the supposedly defunct doctrine...

I realize the hazards of drawing morals from history, as I have been doing, and the likelihood of finding there the morals one is predisposed to expect. But in the end, I see no better way to proceed. The goals of *any* complex human activity are best grasped by recreating, as faithfully as one can, how that activity has taken shape.

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* Presidential Address delivered before the Eighty-second Annual Western Division Meeting of the American Philosophical Association, Cincinnati, Ohio, April 27, 1984.

[1] See A. L. Oppenheim, *Ancient Mesopotamia: Portrait of a Deal Civilization*, revised by E. Reiner, Univ. of Chicago Press, Chicago 1977, pp. 207–208.

[2] One of the oddest features of this story is that Babylonian mathematics attained its height in the very earliest period of which we have records (1800–1600 B. C.); no further development occurred until the Seleucid era (300 B. C.–50 B. C.), when the needs of the new mathematical astronomy may have furnished the incentive for such improvements as the introduction of the zero. Babylonian mathematics remained entirely arithmetical in approach: problems (regarding areas, for example) that we would regard as geometrical were solved numerically. Two classes of tables are found. In one, a problem is formulated and the method of solution is carefully specified, step by step. In the other, a list of problems is given, usually arranged in order of difficulty. The test in each case is to find a number which satisfies certain conditions of an inheritance, of a division of land, or of an assignment of wages. A quasi-algebraic technique allowed, equivalently, the solution of quadratic equations for two unknowns. A generalized algebraic symbolism was never developed. Though the problem types were practical in origin, it seems clear that the techniques had become an end in themselves for the scribal group specializing in mathematical problem solving. Neugebauer notes that the problems often "disregard reality". "It is a lucky accident if the unknown number of workmen, found by solving a quadratic equation, is an integer. Obviously, the algebraic relation is the only point of interest." (O. Neugebauer, *The Exact Sciences in Antiquity*, New York 1969.)

[3] Aabae speaks of the "theories underlying the procedures" followed in Babylonian astronomy, and calls the ephemerides themselves "theoretical schemes" ('Scientific Astronomy in Antiquity', in *The Place of Astronomy in the Ancient World*, ed. F. R. Hodson, Oxford Univ. Press., London 1974, pp. 30, 35). He rightly wants to emphasize that the Babylonians *understood* the mathematical techniques they were using. Another procedure used by them was to note when a planet 'repeated itself'. Jupiter, for example, repeats itself every 71 years; what it did 71 years ago, it will do again this year on the same dates. So the record of the earlier year can be used to predict. But this, Aabae argues, is like the technique used in modern weather forecasting; it is 'pre-scientific' because it does not give us "control over the irregularities within each period", and so observational records must still be constantly checked. A properly "scientific astronomical theory" is "a mathematical description of celestial phenomena capable of yielding numerical predictions that can be tested against observations" (*Op. cit.*, p. 23). In our usage, this is not a sufficient criterion for theory in a natural science such as astronomy. But it *is* sufficient for P-science.

[4] G. E. R. Lloyd, *Magic, Reason and Experience*, Cambridge Univ. Press, Cambridge 1979, pp. 265, 266.

[5] Lloyd, *Op. cit.*, p. 52.

[6] Fragment 2.

[7] *Physics*, II, 7; 198a 22.

[8] *In Aristotelis de Coelo Commentaria*, I. Heiberg, Berlin 1894, 253a 37–253b 2; 248b 40–285a 1. There is some doubt as to whether Plato really *would* have allowed that the phenomena could be saved by any mathematical formalism. The phrase 'saving the phe-

nomena', often identified with the Platonic tradition, is not found before Sosigenes, a philosopher of the second century A. D. Simplicius uses it very frequently, as a glance at the subject index in the Heiberg edition will show. D. R. Dicks laments the ambiguity of the phrase which, in his view, "has led to the misleading idea that the Greek astronomers were concerned mainly with distorting the results of observation to make them fit into preconceived theoretical schemes" (*Early Greek Astronomy to Aristotle*, Thames and Hudson, London 1970, p. 258). In his book, *Sozein ta Phainomena* (*To Save the Appearances*, trans. E. Doland and C. Maschler, Univ. of Chicago Press, Chicago 1969) Pierre Duhem takes the opposite (and commoner) view that *saving* the phenomena meant fitting a mathematical formalism to them, independently of any concern with theoretical interpretation.

[9] 528E–530C.

[10] P. Shorey translation, in *Plato: The Collected Dialogues*, ed. E. Hamilton and H. Cairns, Pantheon, New York 1961, 529b. Translation slightly modified.

[11] 529D.

[12] 530B. I follow Vlastos's translation ('The Role of Observation in Plato's Conception of Astronomy', in *Science and the Sciences in Plato*, ed. J. P. Anton, Delmar, New York 1980, p. 3) in this instance.

[13] *Physics*, II, 2; 194a6.

[14] It is extremely difficult to reconcile the different texts in the *Metaphysics*, the *Posterior Analytics* and the *Physics* where he treats this question, but the issue is too large to deal with here.

[15] 193b 25–30.

[16] In a famous paper, G. V. Schiaparelli showed how Eudoxus's concentric model could be made fairly accurately predictive (except for the retrograde motions of Mars and Venus which, in principle, this model *cannot* explain; see Aabae, *Op. cit.*, p. 39). But as Dicks (*Op. cit.*, Chapter 6) convincingly demonstrates, this is as much a tribute to Schiaparelli's ingenuity as to the merits of the Eudoxan system. Our knowledge of this system comes entirely from references in other ancient writers, since all Eudoxus's own works are lost. Schiaparelli postulates, for example, that our two main sources, Aristotle and Simplicius, got Eudoxus quite wrong in regard to the relative roles of the second and third lunar spheres. This enables him to set up a much more successful account. He also assumes an observational knowledge of the retrograde motions of the planets that Eudoxus is most unlikely to have possessed. Dicks argues that what modern commentators, like Schiaparelli and Dreyer, are really showing are the *potentialities* of a model of the Eudoxan type, *provided* that one has a fairly exact knowledge of planetary motions and of spherical geometry. But these came only much later.

[17] J. L. E. Dreyer argues that "as Eudoxus made no attempt to connect the movements of the various groups of spheres with each other, it seems probable that he only regarded them as geometrical constructions suitable for computing the apparent paths of the planets" (*A History of Astronomy*, revised by W. H. Stahl, Dover, New York 1953 [1906[1]], p. 91). This seems a weak argument, both because we have nothing at all from Eudoxus on this point and because there was no reason why each planet might not have been assigned a dynamically independent system. Dicks likewise takes Eudoxus's spheres

to have been "mathematical abstractions", on the grounds that he did not speculate on the material of which they were composed or the connections between them or the powers that moved them (*Op. cit.*, p. 257). But first we only have a few fragments of his work, and second, a lack of speculation on these latter issues by no means entails that he regarded the spheres as calculating devices. Dicks himself points out how the poles of one sphere are 'attached' to the next outer one in such a way that it is 'carried round' by it. This is a highly physical conception.

[18] Who added seven spheres to the 26 of Eudoxus, two each for sun and moon, and one each for Mercury, Venus and Mars. Schiaparelli shows how the extra planetary sphere *could* have enabled the Eudoxan model to handle the problem of retrograde motion, the two spheres for the sun *could* have solved the problem of its variable apparent motion in the ecliptic. Since Callippus's work is lost, we can only guess at his reasons.

[19] The motion of one sphere cannot explain, since it does not determine, the motion of the sphere next below it. (Recall that the two axes do not coincide, and the speed of the second rotation is quite independent of that of the first.) The Eudoxan system was meant to show that *if* one had a set of four spheres, interlocking and rotating in the way specified, the resultant combination of the four circular motions could explain the apparently irregular motion of the planet attached to the lowest sphere. When writing the *Physics* and the *De Caelo*, Aristotle may have been tempted to infer from this that the motion of the first sphere *caused* that of the others. Confusion between the downward causality of D-science and the postulational upward explanation of astronomy was to accompany Aristotelian science right to the end.

[20] XII, Chapters 7 and 8.

[21] The Unmoved Mover proper to each sphere would be responsible (teleologically) for the simple circular motion of that sphere. Then each sphere would be (mechanically) carried by the one above it and carry the one below it, thus sharing the motion of the former and passing it (with its own motion also) to the one below it. This could work, if the two types of action (desire, and contact action between spheres) be conceded. Many commentators have suggested that the inelegant multiplicity of the 55 Unmoved Movers can be brought under the agency of a single First Mover if the latter is made the object of desire on the part of the former. But Dicks is, I think, right to argue that this has no basis in the *text* (*Op. cit.*, p. 214), even though it *would* bring about the sort of explanatory unification that Aristotle usually sought for.

[22] This is Dicks's rendering of 1074a 5 (*Op. cit.*, p. 200). Ross says: "only thus can all the forces at work produce the observed motion of the planets". The term 'forces' here gives an overly mechanical rendering of a mode of action which the previous section has asserted to operate (at least, in part) by desire.

[23] *Metaphysics* XII, 3; 1074a 16.

[24] 1074a 31. One can see here the roots of an astrology quite different in its basis from the omen lists of the Babylonians.

[25] One other attempt to unite them during that period ought to be noted. Heracleides, a contemporary of Aristotle's, proposed a geocentric model in which the earth was given the single movement of rotation, thus eliminating the need for a fast-moving sphere of the fixed stars and of the consequent adjustments needed for the planetary motions. In

the following century, Aristarchus (according to Simplicius) proposed as an 'hypothesis' the full heliocentric model, in which sun and stars are at rest, and the earth has a double movement of rotation and evolution. (See T. L. Heath, *Aristarchus of Samos*, Clarendon Press, Oxford 1913.) Dreyer infers that the reasons motivating the hypothesis would have been likely to be 'physical' rather than 'mathematical', since there is no evidence that he ever worked out a predictive account. Aristarchus, Dreyer concludes, was the last Greek philosopher "who seriously attempted to find the physically true system of the world" (*Op. cit.*, p. 149). Later astronomers made no attempt to develop the heliocentric model, probably because of what seemed to be two insuperable objections, one that the physics of Aristotle clearly showed that the earth must remain at rest at the center, and two, that this hypothesis would entail that a parallax effect ought to be seen for stellar movements unless the stars were at an incomprehensibly great distance. These were precisely the objections that Galileo had to overcome two millennia later.

[26] In the eccentric model, the planets' center of rotation is not the earth but a point close to the earth which is either at rest or makes a small circle around the earth. In the epicycle model, the planet is carried on a smaller circle which rotates around a larger circle. The two systems were early seen to be geometrically equivalent in the case of the outer planets; only the epicycle model will work for the inner planets, which is told in its favour.

[27] Simplicius notes that it was known that the moon varied in apparent size on the basis of direct and especially of eclipse observation; this with the well-known brightness variations of Venus and Mars, counted heavily (he says) against any kind of concentric model (*In Aristotelis de Cælo*, 225b 39–226b). Autolycos of Pitane (c. 310 B. C.) was the first astronomer we know of who used this discrepancy to reject concentric models in general.

[28] *Almagest*, I, 9; p. 45.

[29] It still has a uniform motion, but around a point, the equant, which is not the center of its rotation.

[30] He also had to introduce a complicated double rocking motion of the epicycle circles to account for movements in latitude (departures of the planets from the ecliptic), or as we might put it, to compensate for his taking the lines of nodes of the planetary motions to intersect the earth instead of the sun. Dreyer remarks: "In no other part of planetary theory did the fundamental error of the Ptolemaic system cause so much difficulty as in accounting for the latitudes, and these remained the chief stumbling-block up to the time of Kepler" (*Op. cit.*, p. 200).

[31] Only the apparent *positions* of the celestial bodies, not their distances. Ptolemy's lunar model would make the apparent diameter of the moon twice as great at some times than at others, a variation which would be easily visible. Dreyer concludes that since Ptolemy must have known this yet never mentions it, he must at this point have been conceding that his theory "was merely a means of calculating the apparent places of the planets without pretending to represent the true system of the world" (*Op. cit.*, p. 196). Yet Ptolemy *would*, I think, have wanted to add to this, that there was no other method of attaining a truer one.

[32] *Commentaria in Aristotelis de Coelo*, II. Hae g. 5. comm. 35; Duhem, *Op. cit.*, p. 30.
[33] I, 32, 1 and 2. Quoted by Duhem, *Op. cit.*, p. 42. Thomas Litt's work, *Les Corps Celestes dans l'Univers de St. Thomas d'Aquin* (Publications Universitaires, Louvain 1963) contains a very detailed treatment of the many texts where Aquinas refers to Ptolemaic astronomy. He makes it clear that Duhem's interpretation of Aquinas as holding that the epicycles are no more than 'hypotheses' which the physicist need not take seriously, needs qualification in several regards.
[34] For a detailed account of these issues, see my *Newton on Matter and Activity*, Univ. of Notre Dame Press, Notre Dame 1978.
[35] Laudan seems to assume that the late medieval scepticism about astronomical models derived solely from the perception that they were hypothetically arrived at (*Science and Hypothesis*, Reidel, Dordrecht 1981, pp. 72–85). I think that this is not what the texts are saying.
[36] Sixth Part, trans. P. Olscamp, Bobbs-Merrill, Indianapolis 1965, p. 52.
[37] *Loc. cit.*
[38] See my 'Two Ideals of Explanation in Natural Science', *Midwest Studies in Philosophy*, ed. H. Wettstein, Univ. of Minnesota Press, Minneapolis, Vol. 9, 195–210.
[39] See my 'The Conception of Science in Galileo's Work', in *New Perspectives on Galileo*, ed. R. Butts and J. Pitt, Reidel, Dordrecht 1978, pp. 209–257.
[40] Especially the sociologists of knowledge associated with the Edinburgh 'strong programme'. For example, see B. Barnes, *Interests and the Growth of Knowledge*, RKP, London 1977; *Science Observed*, ed. K. D. Knorr-Cetina and M. Mulkay, Sage, London 1983.
[41] *New Organon*, Book I, aphorism 81.
[42] See T. Kuhn, 'Objectivity, Value-judgement, and Theory-choice', in *The Essential Tension*, Univ. of Chicago Press, Chicago 1977, pp. 320–339; C. G. Hempel, 'Valuation and Objectivity in Science', in *Physics, Philosophy and Psychoanalysis*, ed. R. Cohen and L. Laudan, Reidel, Dordrecht 1983, pp. 73–100; E. McMullin, 'Values in Science', *PSA 1982*, ed. P. Asquith and T. Nickles, PSA, East Lansing 1983, 3–25.
[43] See N. Jardine, *The Birth of History and Philosophy of Science*, Cambridge Univ. Press, Cambridge 1984, Chapter 7: 'The Status of Astronomy'.
[44] Univ. of Chicago Press, Chicago 1970^2, p. 94.
[45] 'Objectivity, Value-judgement, and Theory-choice', in *Op. cit.*, p. 335.
[46] Rowman, Totowa (N. J.) 1983, p. 145.

DAVID BLOOR

RATIONALISM, SUPERNATURALISM, AND THE SOCIOLOGY OF KNOWLEDGE

I

Living in a secular culture brings with it the conviction that scientific knowledge must have a different and superior status to religious belief. We have no difficulty seeing that religion calls on the support of traditions, institutions, conventions and vested interests. We may then be tempted to conclude that, by contrast, propositions about the natural world do not depend for their credibility upon any such sources. Do they not emanate simply from a rational curiosity about nature? The shift of credibility away from religion and onto science is a relatively recent development. One of its side-effects is that sociologists of scientific knowledge now find themselves in a similar position to the scholars of the last century who scrutinized the historical credentials of accepted Biblical doctrine. Similar arguments fly back and forth about their work, and similar postures are taken up. I shall begin by developing this comparison, and then use it to make some observations about certain objections that have been urged against the sociology of knowledge.

The most outstanding representatives of the critical movement in 19th-century theology were the Tubingen school around Ferdinand Christian Baur (1792–1860).[1] Using the most advanced data and techniques that were available these scholars examined the Bible as an historical document, checking its claims for reliability and consistency. This programme was not in itself new, but the determination with which it was carried out certainly was novel, and so were the results. Baur exhibited what would now be called the 'social construction' of the New Testament. He showed how its doctrinal content emerged from the competing interests

Imre Hronszky, Márta Fehér and Balázs Dajka (eds), Scientific Knowledge Socialized, 59–74.
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of different Church parties, and described their strategies of negotiation and compromise. In his study of the varying relations between the Pauline and Petrine parties in the early Church, Baur showed the importance of locating what he called the 'tendency' of a text, that is, the purpose and interest that informed it. By detecting changes in tendency, and relating them to the historical context, he cast doubt on the authenticity of many of the Pauline epistles. More than one hand had been at work. The Biblical 'Paul', Baur concluded, was the name of an evolving theological position, not that of an individual.[2]

Baur did not seek to decide whether the tendencies he described were theologically correct. Nor did he pass judgement on the inner authenticity of the religious life of historical actors. The question of whether Constantine's conversion was for religious or political motives was dismissed. It was the political fact that mattered, not the unfathomable psychology of the individual.[3] For Baur, Church history only made sense when fully integrated into the realm of historical causes and effects. Christian doctrine was an historical product, transmitted from generation to generation and interpreted afresh in the light of new circumstances. The Divine forces informing that doctrine were not qualitatively different from the power behind all human and natural processes. Baur's stance was therefore *monistic* rather than *dualistic*.

II

Many historians and sociologists of science are now beginning to study our most cherished beliefs in the same fashion that Baur and his colleagues studied theirs.[4] Just as the Tubingen scholars were accused of attacking Christianity – which was far from their intention – so today a sociological analysis of science is frequently read as if it were an attack.[5] The assumptions that underly this modern-day response to the work of sociologists were clearly ex-

pressed by Karl Mannheim in the following words. Existential – or social – determination, he said:

> may be regarded as a demonstrated fact in those realms of thought in which we can show... that the process of knowing does not actually develop historically in accordance with immanent laws, that it does not follow only from the "nature of things" or from "pure logical possibilities", and that it is not driven by an "inner dialectic".[6]

Imre Lakatos has developed the same standpoint but in a different terminology.[7] Science, said Lakatos, is made up of 'research programmes' which are based on a 'hard core' of theoretical ideas. Scientists unfold the ideas of the hard core by following a set of built-in directives called the 'positive heuristic' of the programme. Mannheim's 'inner dialectic', then, corresponds to the 'theoretical autonomy' that Lakatos imputed to research programmes, that is, their ability to sustain a continuous development in the face of anomalies. The counterpart of the 'immanent laws' that govern the growth of knowledge is to be found in what Lakatos called the rational reconstruction of the 'internal history' of science. Internal history displays the congruence between scientific behaviour and a set of methodological rules derived from some chosen philosophy of science. Action which accords with the rules is called 'rational', and is said to be explained by the influence of the rules in question. External history, by contrast, consists of everything that scientists do that deviates from the specified rules. This irrational behaviour, and this alone, said Lakatos, is to be explained by sociopsychological causes. So the immanent laws by which knowledge grows are the laws of rationality.

Similar positions to those expressed by Mannheim and Lakatos have been endorsed by many other writers. The common theme has been expressed with great clarity by Laudan:

> When a thinker does what is rational to do, we need enquire no further into the causes of his action, whereas when he does what is in fact irrational – even if he believes it to be rational – we require some further explanation.[8]

That further explanation, said Laudan, is a matter for the sociologist. Only irrationality is sociologically problematic.

III

I suspect that most adherents to this 'rationalist' approach do not know what Baur knew – that this picture has long informed and impoverished the writing of Church history. Mannheim, Lakatos and Laudan are recommending to us the historiographical principles that Baur knew under the name 'supernaturalism'. As Baur explained in his *Epochs of Church Historiography*, supernaturalists assume that Christian dogma represents a uniquely potent truth. Church history unfolds through the apostolic and priestly succession which progressively reveals the full meaning of that truth. Institutions and persons which embody this truth derive their entire significance from this fact, and the historian's task is to exhibit their special standing. The supernaturalists acknowledge that there is also error and evil, so the full history of the Church comprises two parts standing in dramatic opposition to one another. The primary part concerns 'authentically apostolic truth'; the secondary part concerns 'heretical falsification'.[9]

The connection of supernaturalism with the rationalist history of science is easily proven. Both are dualist theories. Both divide the world into opposing principles with a characteristic asymmetry of evaluation and explanation. The opposition of spirit and flesh becomes the opposition of knowledge and society. The word of God expressed through Church doctrine is replaced by the inner dialectic which drives knowledge forward. Dogma becomes the hard core and heuristic of a research programme. The internal history of science thus replaces the history of apostolic truth. The category of error replaces that of sin, and heresy in its modern form is hunted down under the name of 'irrationality'.[10] We may

even express supernaturalism by commandeering the very words used by our rationalist philosophers. Thus:

> When a Christian believes what is orthodox, we need enquire no further into the causes of his belief, whereas when he believes what is in fact heretical – even if he believes it to be orthodox – we require some further explanation.

Rationalists want us to structure our understanding of scientific development in the same way that supernaturalists structured their histories of the Church. Where Baur would analyze a conflict between Church parties by relating their theologies to their divergent interests, the supernaturalists would relate the dogmatically 'correct' stance to divine inspiration, and the 'incorrect' stance to worldly ambition, ignorance or sin. In the same way, in addressing a scientific dispute rationalists are committed to an explanation in which one party is guided by the rational indications of a body of ideas and evidence, while the rival party is guided by mere social influence. One party will be subject to the rational explanation of internal history; the other to the socio-psychological explanation of external history. Of course, a rationalist could say that *both* parties to a scientific dispute were irrational, but that would involve his handing it over in its entirety to the sociologist. At the opposite extreme he could decide that both parties were being entirely rational. But if rationality can be equivocal in its implications and divided in its results, then it cannot explain the divergence of opinion. Some other factor must explain why this aspect of rationality held sway here, while that aspect held sway there. These two limiting cases represent the points at which the dualism of the rationalists' theory breaks down. They therefore provide no escape from the requirements of their basic scheme of explanation.

IV

How do rationalist historians of science identify the particular doctrines through which the inspiring force of rationality is channelled? They fully appreciate that naive appeals to truth and falsity are no help. The empirical evidence can never prove that one of two competing theories is true. Nor, in practice, can it decisively prove that one of them is false. Duhem showed that a theory is tested in conjunction with a network of subsidiary assumptions and auxiliary hypotheses. A false prediction can be blamed on these, and it is usually possible to modify the surrounding network in order to reconcile fact and theory.[11] This removes the 'asymmetry' of verification and falsification, and it is precisely a form of asymmetry that the rationalist needs. Perhaps some more delicate discriminations will serve the rationalist, for example the degree of confirmation afforded to a theory h by a body of evidence e. But here too there are problems. Just as it is difficult to capture religious experience in a dogmatic formula, so it is difficult to formalize our inductive intuitions. One important class of confirmation theories treats evidential support as a 'timeless' logical relation between propositions.[12] If h implies e, then e confirms h, where the degree of support depends on some given confirmation function. No approach of this kind, however, will allow the rationalist to evade Duhem's argument, because it makes the flow of confirmation itself subject to that argument. Not only is the choice of confirmation function a matter for negotiation, but evidence which at first disconfirmed a theory will confirm it after the theory has been adjusted to bring it into line with observation. Thus, the fact that the calx (or oxide) of a sample of metal weighs more than the original metal disconfirmed some versions of the phlogiston theory, but confirmed the modification that says that water enters the calx as phlogiston leaves.

One of the most instructive attempts to solve the problem of rationalist historiography was made by Lakatos and his followers.[13]

They said that to be rational was to be impressed if a theory can predict novel facts. A successful prediction yields more evidential support than a fit between fact and theory produced by retrospective adjustment. They replaced a 'logical' by an 'historical' view of confirmation.[14] Lakatos argued that it is rational for opposing research schools to adhere to their own theories, and to avail themselves of Duhem's defensive measures, as long as their respective programmes were 'progressing'. This means yielding a sequence of theories that predict novel facts, though not every prediction need be correct. A 'degenerating' programme is one whose input of novel facts does not come from its predictive success, but from the discoveries of the rival programme or from chance. Its theoretical elaboration becomes an ad hoc response to external contingencies. A scientific controversy will then be resolved rationally if scientists switch from a degenerating programme to a progressing programme, if one is available. This, said Lakatos, is what usually happens – the only exceptions being the few cases where the workings of science are subject to a socio-psychological distortion.

Critics immediately asked at precisely what point a rational scientist must abandon a degenerating programme?[15] Not being able to supply an answer Lakatos changed his definition of rationality. He came to say that rationality does not reside in *what* is done, but in *how* it is done. It transpired that scientists can rationally stick with a degenerating programme, even after it has been overtaken, as long as they do not deceive themselves about the problems they face.[16] Rationality is now equivalent to honesty. This shift evaded the original problem but compromised the whole analysis. Consider the case of Priestley and his long defence of phlogiston theory. By 1796 he was an isolated heretic. Was he irrational? Given Lakatos's basic idea, he surely was; but not on the new definition. According to Musgrave this case fits Lakatos's (new) theory "like a glove" and, we are told, Priestley "was always perfectly *rational*".[17] The tendency is clear: what we are seeing is a retreat to the limiting cases that I identified earlier,

where *all* the parties to a scientific debate are deemed rational. The dualism at the heart of the rationalist position has proven difficult for the rationalists themselves to sustain.[18]

V

The reason for these difficulties is that the rationalist picture treats our inductive propensities as if they work *against* our social natures, rather than *through* them. It is better to follow Baur's example and develop a monistic approach in place of this troublesome dualism. Suppose we treat induction as a natural urge, like the urge to eat. Every cultural form recognizes the urge to eat and provides channels for its proper expression. Complex rules of preparation and consumption, feasting and fasting, integrate the satisfaction of hunger with the rest of social life. In the same way, inductive propensities stand behind *all* systems of knowledge. But just as the eating practices of a culture cannot be understood in purely biological terms, so a body of knowledge cannot be understood purely in terms of our natural inductive urges.[19] Our inductive conclusions will be mediated by the *a priori* probabilities inherited from our culture and by all the other parameters that inform our appraisals of a theory; for example, our concern with boldness, unity, analogy, naturalness, fertility, generality, simplicity, consistency, picturability and, in its many forms, utility.

The significance we give to predictive power has a *weight* attached to it. Be it high or low that weight will always be a factor in our responses, and its particular value will call for an explanation.[20] Priestley could see the attractions of Lavoisier's theory – he tells us so – but other factors outweighed these. Because this weight varies from case to case, and time to time, it is little wonder that Lakatos could provide no plausible a priori answer to his critics' questions. Nor is it any wonder that he was driven to saying that the point at which to abandon a degenerating programme

lay with 'common sense'.[21] What is common sense in this context but a matter of collective decision, that is to say, a *convention*?[22]

The science which studies our basic animal urges is, of course, biology. The department of biology which is concerned with our, as yet little understood, natural capacity to register and respond to the probabilistic texture of our environment is psychological learning theory.[23] The science which studies the conventions which always attend and structure the expression of our urges and capacities is sociology. Socio-psychological considerations, then, will not be distributed in the one-sided manner favoured by rationalists. Like inductive considerations, they will be a feature of *all* knowledge.[24] It is, of course, always tempting to think that any group who protect their theories from refutation, and who resist the lure of a few predictive successes, must be deviating from the requirements of scientific rationality. Furthermore, these conservative responses often seem explicable by socio-psychological causes. Scientific reputations are often based on theories, so the scientists who have invented and developed them have a vested interest in their survival. Clearly, though, the rationalist position depends on our generally *not* being able to find a corresponding interest behind the work of those who want to innovate and who place more weight on this or that predictive success. And here the rationalist is in trouble. The circumstances that create interests of the kind in question generally create them in opposing pairs. Whenever someone's reputation will suffer if a theory is discarded, someone else's will be enhanced by their association with the new theory.[25] Whenever one discipline or technique relinquishes its rights over some area of nature, the practitioners of some other discipline or technique will be found to have advanced their claims.[26]

The historical literature on scientific controversy typically shows neither side compromising what we may assume to be their natural reasoning propensities. Rather, it shows both sides making sequences of decisions which can be understood by examining the local and contingent circumstances in which the actors find them-

selves, and the goals and interests they are pursuing. Why did the corpuscular philosophers take issue with the ancient atomists by insisting that matter was passive?[27] Why was Pearson, unlike Yule, adamant that the measure of statistical association for nominal variables had to be analogous to that developed for interval variables?[28] Why do herbarium taxonomists and laboratory-based botanists continue to differ over the definition of plant species?[29] The determinants of credibility uncovered by historians in these and many other cases are essentially the ones to which Baur drew attention in his field of study – group conflicts born of divergent interests, problems of control, autonomy and boundary maintenance, problems of conflict and mediation and their symbolic counterparts.[30]

The point is *not* that scientists attend to these considerations *rather than* the experience of nature. The claim is that these and similar factors are intrinsic to the collective apparatus that they must create and use to give that experience a shared structure and meaning. The continuity, identity and implications of a theory are problematic things which are argued back and forth by its defenders and critics. A theoretical doctrine, like a Biblical doctrine, is a collective work, continually modified, supplemented and interpreted in ways that were not anticipated in its initial formulation. Cognitive order must be maintained in the face of a continual stream of anomalies. There is therefore no essential doctrinal core that is being unfolded and whose content can guide those who work upon it.[31] Nor do we possess any single, privileged touchstone of intellectual virtue that guides us when we must choose to enlarge our knowledge in this direction or that.

VI

I have developed this claim in connection with large-scale units like 'theories', but nothing depends on this. The picture does not

alter if we replace large, complicated units by smaller, apparently simpler elements like concepts. The argument applies to every single act of concept application. When we bring an object under a concept we are claiming a sameness relation between that particular and the finite collection of other particulars already classified under that heading. Applying a concept to an instance is like applying a theory to a new observation, and we have seen that what a body of theory says about a new case is not predetermined by its previous applications. It involves decisions and judgements.[32] In the same way the next application of a concept is not fixed by its past applications. These do not create something called the 'meaning' of the concept which settles in advance whether the subsequent applications are correct or incorrect. There is no 'universal' for the mind to grasp. However routine, each new application of a concept is in principle open to negotiation.[33] In a formal sense it will involve decision and judgement, and these will be related to goals and interests. In the last analysis stable usage will always have the character of a convention. So there are not two ways of applying concepts: one (correct) way flowing from the meaning; the other (incorrect) way determined by mere convention. Not even single concepts possess the efficacy and unity – the inner dialectic – that can explain the behaviour of those who apply them.

Rationalism seems plausible because we think that rules and meanings furnish us with invisible rails which reach ahead of behaviour, giving guidance to those rational enough to intuit their presence. But we cannot make concepts or rules explain the activity of concept or rule users. It is the activity of the users, their habits and goals, which explain why a concept or rule is applied in the way it is.[34] Does this mean that nothing corresponds to Mannheim's inner dialectic, or to the moving force of rationality itself? The answer is that something *does* answer to these terms, but that it has been misdescribed. Continuities of doctrine, the constraints furnished by correct meanings and implications, the feeling of re-

ality speaking to us through the evidence, are all engendered and sustained by the behaviour of people around us. Like all divine and magical forces and queer mental processes, the force of reason – where it is not animal instinct – is the force of society mislocated and mystified.[35]

One final comment on Baur and the Tubingen school. I have presented them as heroic innovators in the sociology of knowledge. Their great contribution is now largely forgotten, but I have no doubt that this is a correct evaluation. Of course, the analogy between the movement of 'higher-criticism' in theology, and the sociology of knowledge, is not perfect. I have played down the extent to which Baur was immersed in idealistic and Hegelian modes of thought. The surprising thing, though, is how little this affected his historical practice. The charge levelled against him by some contemporaries that he forced history into the Hegelian mould of thesis and antithesis, does not withstand careful examination.[36] Predictably, the standard criticism of Baur's sociological and political approach was that he ignored the really significant forces at work in Church history: the *person* of Christ, and the influence of his *word*. In the place of the contingencies that Baur brought to light, his critics preferred mystical pseudo-explanations and a religious teleology. Why, they asked, are theological controversies and party conflicts ever resolved if there is not a Divine Reality that eventually impresses itself on the minds of those concerned?[37] This Reality, working through Christ and his gospel, provided the inner dialectic of Church history. I have tried to indicate why the underlying assumptions of today's rationalists – about our being guided by rules and meanings and, of course, Reality – are no less mystical and impenetrable than those of Baur's reactionary critics.

NOTES

[1] F. C. Baur, *The Church History of the First Three Centuries*, 2 Vols, translation revised by A. Menzies, Williams and Norgate, Edinburgh 1878; P. C. Hodgson, *The Formation of Historical Theology: A Study of Ferdinand Christian Baur*, Harper and Row, New York 1966. For a contemporary view of Baur see Mark Pattison, 'The Present State of Theology in Germany' (1857), in H. Nettleship (ed.), *Essays of the Late Mark Pattison*, Vol. II, Clarendon Press, Oxford 1889, pp. 210–262.

[2] Baur only accepted Galatians, I and II Corinthians and Romans as authentic; he raised doubts about Colossians and Ephesians, and rejected Thessalonians, Philippians and Philemon.

[3] Baur, *Church History*, Vol. II, *op. cit.*, pp. 225, 227.

[4] For useful introductions to this literature see B. Barnes and S. Shapin (eds), *Natural Order: Historical Studies of Scientific Culture*, Sage, Beverly Hills 1979; B. Barnes and D. Edge (eds), *Science in Context: Readings in the Sociology of Science*, Open Univ. Press, Milton Keynes 1982; S. Shapin, 'History of Science and its Sociological Reconstructions', *History of Science* XX (1982), 157–211; H. Collins (ed.), *Sociology of Scientific Knowledge. A Source Book*, Bath Univ. Press, Bath 1983.

[5] Thus a reviewer of B. Barnes's book *T. S. Kuhn and Social Science*, Macmillan, London 1982, reads it as an act of 'denigration'. See J. Grove, 'The Sociological Denigration of the Rationality of Science', *Minerva* XX (1982), 550–556.

[6] K. Mannheim, *Ideology and Utopia: An Introduction to the Sociology of Knowledge*, Routledge and Kegan Paul, London 1936, p. 239.

[7] I. Lakatos, 'History of Science and its Rational Reconstructions', in C. Howson (ed.), *Method and Appraisal in the Physical Sciences: The Critical Background to Modern Science 1800–1905*, Cambridge Univ. Press, Cambridge 1976, pp. 1–39.

[8] L. Laudan, *Progress and its Problems: Towards a Theory of Scientific Growth*, Routledge and Kegan Paul, London 1977, p. 189. I shall not discuss Laudan's theory in detail. For a sociologist's response see B. Barnes, 'Vicissitudes of Belief', *Social Studies of Science* 9 (1979), 247–263.

[9] F. C. Baur, 'Epochs of Church Historiography' (1852), in P. Hodgson (ed.), *Ferdinand Christian Baur on the Writing of Church History*, Oxford Univ. Press, New York 1968, p. 53. There are, of course, both Catholic and Protestant versions of supernaturalism, the first locating the internal history of dogma in the Church itself, and the second seeing the Church as the embodiment of corruption and tracing the early history of Christianity through individuals who opposed the Church. I have presented the Catholic version.

[10] Rationalist commentators have sometimes exploited the links that Kuhn notes between scientific and religious orthodoxy, and used them as a vehicle for criticizing the view of science proposed in the *Structure of Scientific Revolutions*. See for example J. W. N. Watkins, 'Against "Normal Science"', in I. Lakatos and A. Musgrave (eds), *Criticism and the Growth of Knowledge*, Cambridge Univ. Press, Cambridge 1970, pp. 25–37. It is amusing to see how deep are the theological roots of their own view.

[11] P. Duhem, *The Aim and Structure of Physical Theory* (1914[1]), trans. P. P. Wiener, Atheneum, New York 1962, Ch. VI.

[12] For reasons that will become clear I am going to follow the useful discussion of A. Musgrave, 'Logical versus Historical Theories of Confirmation', *British Journal for the Philosophy of Science* 25 (1974), 1–23.

[13] See the interesting papers in C. Howson (ed.), *Op. cit.*

[14] It may seem strange to use the notion of confirmation in connection with Lakatosian and Popperian theories, but in this I am following Musgrave.

[15] For example, 'Reflections on My Critics', in I. Lakatos and A. Musgrave (eds), *Op. cit.*, p. 239.

[16] Cf. C. Howson (ed.), *Op. cit.*, p. 15.

[17] A. Musgrave, 'Why Did Oxygen Supplant Phlogiston?: Research Programmes in the Chemical Revolution', in C. Howson (ed.), *Op. cit.*, p. 207.

[18] Obviously this is a sketch of a complicated argument. Lakatos's ideas have been rendered even more complex by distinguishing between rational requirements that apply to groups and to individuals, as well as distinctions between rational 'appraisals' and 'advice'. For example, though any individual might be rationally advised to abandon a degenerating programme, it might be useful for science as a whole to have a subgroup who stay with the old approach. This ensures that none of its resources are wasted, and acts as an insurance policy. The community can 'hedge its bets'. But if this is so then was the change to oxygen theory rational, given that Priestley was almost alone in hanging on? The old, unsolved problem of *how long* to defend a theory is replaced by the new insoluble problem of *how many* should defend it? The knots in which Lakatos tied himself are mercilessly exposed by one of his own followers in A. Musgrave, 'Method or Madness?', in R. Cohen, P. Feyerabend and M. Wartofsky (eds), *Essays in Memory of Imre Lakatos*, Reidel, Dordrecht 1976, pp. 457–491. Musgrave's desire is that we return to fundamentals, place less emphasis on theoretical autonomy and more on the controlling influence of reality. (See note 37 below.)

[19] Rationalistic thinkers have long tried, and failed, to give biological cum medical reasons for dietary prohibitions like avoiding pork. Sociological theories get further. See M. Douglas, *Purity and Danger: An Analysis of Concepts of Pollution and Taboo*, Routledge and Kegan Paul, London 1966; and *Natural Symbols: Explorations in Cosmology*, Penguin, Harmondsworth 1973.

[20] The weight can be zero. We can say that the feeling that the hypothesis should be formulated in advance of the data it explains is an illusion to be discounted – like the gambler's feeling that a long run of 'heads' increases the probability that the next coin toss will be 'tails'.

[21] Cf. C. Howson (ed.), *Op. cit.*, p. 16.

[22] Musgrave notes that having more than one desideratum for a good theory has "interesting repercussions". "In such a situation two scientists might make their choices in different ways. And (short of attempting to *weight* his *desiderata*) the methodologist will have to say that both of them are acting rationally." (p. 22.) The repercussions, then, are that the requirements of rationalist historiography are violated, and Lakatos's approach is shown to be based on an abstraction: the rationalist is pushed back to the limiting cases again. (See note 12 for full ref.)

[23] What I am proposing is that confirmation theories should be read as conjectures about our underlying cognitive dispositions. They are then contributions to psychology and, as such, descriptive not normative. Some, at least, of the students of confirmation see their work in this fashion, and have much to offer the sociologist of knowledge.

[24] In putting our inductive propensities 'behind' or 'outside' our verbalized culture I am identifying them, along with social interests, with what Mary Hesse would call the 'coherence conditions' of her network model. See M. Hesse, *The Structure of Scientific Inference*, Macmillan, London 1974, p. 52.

[25] An obvious example would be the reciprocal fortunes of Lavoisier and Priestley. For clear evidence that Lavoisier was a highly interested, highly motivated and highly skilled operator in the nasty micro-politics of French science, see the revealing and perceptive account of C. Perrin, 'The Triumph of the Antiphlogistians', in H. Woolf (ed.), *The Analytic Spirit: Essays in the History of Science*, Cornell Univ. Press, Ithaca 1981, pp. 40–63.

[26] For example, the competition between geological and biological ideas about adaptation: D. Ospovat, 'Perfect Adaptation and Teleological Explanation: Approaches to the Problem of the History of Life in the Mid-Nineteenth Century', *Studies in History of Biology* II (1978), 33–56; or, the competition between chemical and biological theories of infection and fermentation, see R. Kohler, 'The Reception of Edward Buchner's Discovery of Cell-Free Fermentation', *Journal of the History of Biology* V (1972), 327–353; and by the same author, 'The Enzyme Theory and the Origins of Biochemistry', *Isis* LXIV (1973), 181–196.

[27] Of the extensive literature on this theme see: J. R. Jacob, 'Boyle's Atomism and the Assault on Pagan Naturalism', *Social Studies of Science* VIII (1978), 211–233; M. C. Jacob, *The Newtonians and the English Revolution, 1689–1720*, Cornell Univ. Press, Ithaca 1976; S. Shapin, 'Social Uses of Science', in G. Rousseau and R. Porter (eds), *The Ferment of Knowledge: Studies in the Historiography of Eighteenth-Century Science*, Cambridge Univ. Press, Cambridge 1980, pp. 93–139.

[28] D. MacKenzie, *Statistics in Britain 1865–1930: The Social Construction of Scientific Knowledge*, Edinburgh Univ. Press, Edinburgh 1981, Ch. 7, 'The Politics of the Contingency Table'.

[29] J. Dean, 'Controversy over Classification: A Case Study from the History of Botany', in B. Barnes and S. Shapin (eds), *Op. cit.*, pp. 211–230.

[30] S. Shapin, 'History of Science and its Sociological Reconstructions', *History of Science* XX (1982), 157–211.

[31] This is why Kuhn's account, based round the notion of a paradigm or exemplary achievement, is so good. The paradigm is a resource to be added to and used, not 'followed' or 'unfolded'. Lakatos's methodology of scientific research programmes can be read as a rationalist gloss on Kuhn – a gloss in which the virtues and insights of the original are lost. In particular the continuity of a research tradition is, for Kuhn, the product of a sequence of socio-psychological linkages. For Lakatos it is the result of rationally following out premisses and rules. For some effective criticisms of Lakatos's ideas on continuity, see E. McMullin, 'The Fertility of Theory and the Unit of Appraisal in Science', in R. Cohen, P. Feyerabend and M. Wartofsky (eds), *Op. cit.*, pp. 395–432. To

appreciate why Kuhn's approach is so useful to sociologists of knowledge, see B. Barnes, *T. S. Kuhn and Social Science*, Macmillan, London 1982. Appreciating that Lakatos's theory is just a rationalist gloss on Kuhn lends force to Feyerabend's astute observation that if the Lakatos school of historiography has demonstrated anything, it will be of the form of a *sociological law*. Cf P. Feyerabend, 'On the Critique of Scientific Reason', in C. Howson (ed.), *Op. cit.*, pp. 309, 323.

[32] B. Barnes, 'On the Implications of a Body of Knowledge', *Knowledge, Creation, Diffusion, Utilisation* 4 (1982), 95–110.

[33] For a full justification of this claim see M. Hesse, *Op. cit.*

[34] The essential point was argued by Wittgenstein. For a recent cogent exposition see S. Kripke, *On Rules and Private Language*, Blackwell, Oxford 1982.

[35] E. Durkheim, *The Elementary Forms of the Religious Life* (1912[1]), trans. J. W. Swain, Collier, New York 1961. See also D. Bloor, 'A Sociological Theory of Objectivity', in S. Brown (ed.), *Objectivity and Cultural Divergence*, Cambridge Univ. Press, Cambridge 1984, pp. 229–246.

[36] The charge is answered and rejected by P. C. Hodgson. Cf. *Op. cit.*, pp. 200, 209.

[37] Rev. G. Matheson, *Aids to the Study of German Theology*, Clark, Edinburgh 1875. "What, then, is this mysterious bond, which runs through all differences of opinion, and forms a meeting-ground for the most diverse schools of thought? What is it but the Person of the Redeemer Himself? In Him all tendencies meet... the influence of that presence is stronger than the influence of school, or sect, or party. ... So far from that concurrence being the cause of the gospel it requires itself the gospel to account for it, and finds its solution in that great truth which is at once the centre of history and the foundation of spiritual religion – the union of the Infinite with the finite." (p. 155.)

MÁRTA FEHÉR

EPISTEMOLOGY NATURALIZED VS EPISTEMOLOGY SOCIALIZED

(A quest for a sociology of methodologies)

> "If new models of scientific rationality are wanted, it is in an analysis of the goals of knowledge rather than the internal techniques of natural science that they should be sought."
>
> (Hesse 1981)

The rapid revival and development of the sociology of knowledge we are witnessing in the last ten years is, in my opinion, a process worth while reflecting upon. The more so since philosophy of science is challenged by the Strong Programme (of Bloor 1976 and Barnes 1977) which offers alternative conceptualizations of traditional epistemological problems.

The reason for this recent revival of the sociology of knowledge – after so many attempts (e. g. Fleck 1935, Mannheim 1931) fallen into oblivion during the thirties and later – is, I think, that philosophy of science has got into serious trouble during the last decade, into a state of Kuhnian-type crisis, in consequence of the problems connected with scientific change and rationality, and is thus in need of a new 'paradigmatic' point of view which can be given by the sociology of knowledge. I will argue that although there have been diverse attempts within philosophy of science to solve the problem of rationality of science and so to get out of the crisis, these attempts have failed and thus epistemology needs to be sociologized.

Imre Hronszky, Márta Fehér and Balázs Dajka (eds), Scientific Knowledge Socialized, 75–96.

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From among the consequences of the Kuhnian (1962) historical conception of science, the most serious issue was that which concerned the change of scientific *methods* or *criteria of evaluation* of knowledge claims, as distinct from mere change of beliefs.

The Kuhnian criticism of the Received View undermined the image of science as a paragon of rationality and thus endangered its privileged position among other cognitive enterprises (such as magic, myth, theology, and the diverse pseudo-sciences such as, e.g., parapsychology or astrology). The question was, as Feyerabend put it:

> What's so great about science? What makes science preferable to other forms of life, using different standards and getting different kinds of results as a consequence? What makes modern science preferable to the science of Aristotelians or to the ideology of the Azande? (Feyerabend 1976, p. 110.)

His (1978) answer is that science is one among the ideologies and that no set of cognitive methods can claim a privileged status for itself as uniquely embodying rationality.

There emerged thus a quest for the justification of scientific method itself, the former touchstone of all knowledge claims. The quest for certainty shifted thus to a deeper level: from the problem of justification of knowledge claims (beliefs) to the problem of justification of justification, that is, of scientific method. According to the Kuhnian picture, science has no invariant truth-gaining method and its authority has no basis beyond the consensus of its practitioners. Consensus, however, can be, and actually was throughout the history of science, formed around different sets of methodological norms, thus mere consensus does not suffice to select one among them as the only *correct* one.

According to one of the interlocutors in Gutting's recently appeared *Dialogue*:

scientific practice has authority only if it follows correct scientific method; and we can't just take whatever scientists do as definitive of correct scientific method. There must be norms of scientific practice that are established independently of the fact of that practice. This is just where philosophers come in. They provide the conceptual analyses of truth, rationality, etc. that establish the validity of scientific method. (Gutting 1984, p. 5.)

In the philosophy of science (beginning at the end of the sixties with Lakatos and Quine) there was accordingly a shift towards meta-methodology and, with it, towards self-reflection, as contrasted to the former lack of reflectivity of self-assured normative methodology.

As to the question of the justification of the standards of rationality, three different types of answers could be and were actually given by philosophers of science.

The first was the a priori or fundamentalist solution. That is, along the Cartesian line: to try to ground scientific rationality on some firmer basis, on a first philosophy and, in a Kantian vein, to construe a transcendental deduction for the set of norms constituting the only correct method. This solution, however, no longer seemed acceptable after the fall of logical empiricism in the seventies.

The route to follow in a search for justification of scientific method seemed thus to be that of a posteriori or empirical grounding, as distinct from the above-mentioned a priori, philosophical one. The first version of doing this was Quine's way to meta-methodology, and to self-reflection, through the naturalization of epistemology (in his 1969, 1981). For him the relation between epistemology and natural science, especially empirical psychology, is one of mutual containment. As he says: "Epistemology, for me, is only science self-applied." (Quine in Davidson and Hintikka 1969, p. 293.) Quine seeks to ground epistemology as the evaluation of knowledge claims on empirical psychology as the study of how man comes to acquire and accept his beliefs. Quine's reason for doing this is just the doubt about the existence of a firmer (a

priori) ground, a kind of first philosophy which would precede and underlie all science and thus could serve as the basis upon which to lay the fundaments of the criteria of justification for science.

He denies that we could find a better warrant for our standards of evaluation than those that current science endorses, since we cannot step out of our general conceptual scheme of the world in order to evaluate it. Thus the most we can do is to disclose and *understand*, by means of empirical psychological investigations, *why* certain beliefs, reasonings and justificatory procedures are accepted within science. That is, instead of legitimating or justifying the evaluative standards, Quine seeks to describe and explain them by empirical means. Quine reduces the question of meta-scientific justification to that of scientific justification which is, for him, a question of empirical psychological investigations, aimed at disclosing our mental constitution, in order to see

> how it is that man works up his command of natural science from the limited impingements that are available to his sensory surfaces. (Quine 1974, p. 3.)

This Quinean solution is, however, no remedy in the present state of epistemology. For, first, I claim, if epistemology is regarded – as by Quine – as part of natural science, and in particular, of empirical psychology, then it becomes subjected to the underdetermination thesis (given that Quine denies any extrascientific constraints on it). This means then that a plurality of different methodologies could be construed on the same empirical psychological grounds, all of them logically incompatible but equivalent with respect to empirical data or content. Moreover, as Stabler writes:

> it is not clear that there is any naturalistic basis for the view that every dispute over methods will ultimately be resolved by a scientific demonstration that exactly one of any two incompatible methods is of the same psychological kind as our less controversial scientific method. It could be that two different methods, two methods which yield different results are both of the appropriate kind. There is no guarantee that rational in-

vestigators (i. e. investigators that are on naturalistic grounds rational) will reach consensus. (Stabler 1984, p. 72.)

There is another serious counterargument for Quine's naturalized epistemology, put forward by Ron Amundson who pointed out that

the epistemologist cannot take the epistemological message of a substantive [psychological] theory at face value. For how can she be sure that she is not simply gazing on the reflected face of the cognitive theory's ancestor epistemology? Perhaps the epistemologically relevant features of the theory were conditioned by the descriptive features of the epistemology with which the theory was associated. The problem is supposed to be solved by a bootstrap procedure within the paradigm – the epistemology has been gradually adjusted as the empirical results demand. (Amundson 1983, p. 335.)

Since, according to Amundson the cognitive scientist, that is, the empirical psychologist, is himself using the same epistemology which is the object of his study, thus a cognitive empirical psychological theory

will not, by definition, lead to the discovery of facts which can be used to question its own epistemology. (Amundson 1983, p. 336.)

And so he concludes that

The rejection of an epistemology in cognitive science is rarely, if ever, a result of empirical discoveries, inconsistent with the epistemology ... The epistemology and the substantive theory seem to come in a package, and the epistemology's rejection occurs only through a 'paradigm change' which rejects the theory along with the epistemology. (Amundson 1983, pp. 336–337.)

But let us disregard these counterarguments for the moment and see what picture recent empirical cognitive psychological investigations show about simple human (not even scientific) rationality. Do they attest to the existence of a set of common, fundamental (or basic-level) principles of rationality with which wise

and benevolent Mother Nature endowed us? Or altering somewhat the title of L. J. Cohen's paper: Can (at least) human rationality be experimentally demonstrated? Quine's standpoint was based on the assumption that the norms of rationality can, in this way, be if not justified, then at least explained, or disclosed. Thus we can see why, in acquiring knowledge, we are proceeding according to a particular set of norms: simply because we cannot proceed otherwise, given our psychological (mental) constitution and 'our position in the world'.

Actually existing empirical psychological investigations, however, show a very puzzling picture. Recent empirical studies in the psychology of deductive and probabilistic reasoning (of ordinary, lay persons) have, according to the investigators, "bleak implications for human rationality" (see Nisbett and Borgida 1975; Nisbett and Thagard 1983; Tweeney and Doherty 1983). L. J. Cohen, in his (1981) paper sets out to refute this view, fairly common to empirical cognitive psychologists, according to which:

> The ordinary person is claimed to be prone to serious and systematic errors in deductive reasoning, in judging problems, in correcting his biases and in many other activities. (L. J. Cohen 1981, p. 217.)

Cohen proceeds by hypothesizing a competence–performance distinction (on linguistical analogy) and by postulating the existence of a competence in human beings for reasoning validly: this then provides the background against which we can study the admitted defects of their performance.

By this competence–performance distinction Cohen answers the question put in the title of his paper – Can human irrationality be experimentally demonstrated? – in the negative. For according to him what can be experimentally demonstrated are only the defects of performance and not the lack of competence.

The relevance Cohen's considerations have for our discussion of the problem of naturalized epistemology lies, I think, in show-

ing that empirical psychology is in principle not able to solve the task Quine assigns to it. For if Cohen's hypothesis is accepted, then performance is to be judged regarding its correctness by means of a normative theory of rationality which, on its part, is based on the assumed existence of a competence which would give the ultimate credentials to the normative theory. Thus what can be empirically investigated (performance) is not apt for grounding the norms of rationality (only intuitions belonging to competence can do this), and what is apt for grounding the norms of rationality (competence) is not empirically accessible. Competence is, so to say, per definition exempted from empirical investigation; it simply must exist because of explanatory reasons, in order that the epistemologist should be able to judge performance and to build his normative theory upon it. Short of any higher or commonly accepted authority to which the normative theory could be ascribed, we ascribe them to our inborn (or, with Cohen, to a perhaps culturally inherited) capacity.

Please notice how at the same time this way of reasoning of an epistemologist, like Cohen, instantiates the validity of Amundson's argument for the impossibility of empirically supporting or disconfirming the underlying normative epistemology of cognitive psychology.

To return now to my main line of argument, let me in a Quinean vein assume that to discover what rules of inference or norms of rationality we can and do actually follow in reasoning is an empirical task. The question, however, remains: How are these given to us and why ought we to follow them? The answer, which is as old as Hume, or even older, runs that it is Nature which endowed us with the fundamentals of rationality and that we should follow the dictates of reason in order to survive in nature. An argument in favour of this assumption is usually based today on an evolutionary-adaptationist position according to which the continuous use of fallacious reasoning would have lethal and thus highly unlikely

genetic implications: mankind would not survive for long (see Lycan 1981). And therefore it is assumed that

> Science, as the best product of human reason, is based in the best and surest foundation that is realistically conceivable (i. e. in conformity with Nature). ... science should be praised and viewed as the paradigm of human reason in action; only human reason is instinctive, and its success is guaranteed by its essential 'naturalness'. (Bechler 1981, p. 71.)

So, in this view:

> Our innate capacity becomes the basis, and our science the extension, of a vast survival instrument. (Bechler 1981, p. 76.)

But the fact that mankind has successfully survived so far cannot serve as an empirical proof that this was ensured by what we take to be our rationality. Since, as Einhorn and Hogarth argue:

> We do not find the existence of persistent dysfunctional mechanisms to be incompatible with evolutionary framework. This is particularly the case when one realized that humans adapt the environment to overcome their deficiencies ... Moreover the time-frame of evolution is such that we do not know whether any particular mechanism is being selected for or against, or is a vestige without function. (Einhorn and Hogarth 1981, p. 335.)

And after all, as G. Doppelt says:

> The changing requirements of *human* survival reflect not imperatives of biological adaptation (or survival of the fittest, in Darwin's sense) but rather historical transformations in our cultural conceptions of what constitutes 'human survival'. (Doppelt 1980, p. 33.)

Besides, we must not leave out of account that, for man, 'survival' means not survival simply in an unchanging natural environment, but in a changing natural and an expanding artificial environment. The epistemological import of the latter was duly emphasized by S. Lelas in his (1983) paper on 'The Humanization of Epistemol-

ogy'. As to the former, it was Bloor (1976) who gave us valuable hints when speaking about the implications of the 'social use of nature'.

To sum up, I doubt that even elementary forms (or levels) of human rationality could be established and justified by empirical psychological means only. Our basic natural reasonableness, competence or interestedness in following the so-called dictates of reason is assumed on philosophical grounds, rather than shown by empirical psychological investigations. And even if the set of elementary principles of rationality with which Mother Nature has endowed us could be discovered, this essentially invariant, because genetically given, set of norms would be insufficient for establishing, let alone justifying, the historically changing forms of *scientific* rationality.

Let us consider now what other types of a posteriori solutions have been offered (as distinct from the naturalization of epistemology) in the process of seeking meta-criteria for science. These types constitute a class which I would bring under the heading of the historicization of epistemology. To this class belong the attempts at giving an a posteriori meta-criterion by historical (as opposed to psychological) means, that is, those attempts which use "the history of science as a test for its rational reconstructions" (Lakatos). Among the major representatives of this conception I could mention Lakatos and Laudan. The common feature of their efforts, as I see it, was that, accepting the Kuhnian picture of historically changing scientific methods, they tried to find those meta-methodological norms which were themselves historically invariant and could be assumed to govern as well as justify the change of scientific methodological norms and theories.

In Lakatos's view:

> Science is rational, but its rationality cannot be subsumed under the general laws of any methodology. (Lakatos 1971, p. 41.)

He, however, rejects the Feyerabendian conclusion that if there is no *general* theory of scientific rationality, then there is no such thing as scientific rationality at all. The Lakatosian solution of the problem posed by the plurality and change of scientific methods in the history of science consists in finding the unicity and invariance of science on the meta-level. This unicity and invariance lost in methodology reappear in meta-methodology. As he says:

> We can easily have a second-order methodology of methodological (as opposed to scientific) research programmes: the methodology of research-programmes self-applied. (Lakatos 1974, p. 321.)

And, by this move, he thinks to have accomplished, at the same time, the justification of his own methodology of the scientific research programmes as well (which is in fact a circular way of reasoning). In the last resort, then, to Lakatos everything is justifiedly rational what promotes for a research programme to predict and corroborate novel facts, to have excess corroborated empirical content.

Laudan's solution (in his 1977) bears a certain similarity to that of Lakatos, in that he takes scientific rationality to consist in doing whatever one can to maximize the progress of science, that is, to increase the problem-solving effectivity of scientific research traditions. In this conception – as is well known – the common denominator of changing scientific methods is that no matter what methodological norms are actually followed, they all can be taken as justifiedly rational if they enable the followers of a research tradition to solve more empirical problems and generate fewer conceptual ones than their rivals. In Laudan's view:

> Rationality consists in accepting those research traditions which are most effective problem-solvers. [...] ... there are certain very general characteristics of a theory of rationality which are trans-temporal and trans-cultural, which are as applicable to pre-Socratic thought or the development of ideas in the Middle Ages, as they are to the more recent history of science. (Laudan 1977, p. 130.)

But by this he falls back to the traditional conception of a unique and general theory of rationality valid for the whole history of science. The lost methodological invariance is again rediscovered on the meta-level. And when applied to actual cases in the history of science, this overall theory of rationality which ought to be based on the more primitive and independently defined concept of scientific progress, appears to be grounded merely upon sound intuitions of rationality. Laudan himself admits that there is no non-circular way to establish of paradigm cases of rationality in the earlier history of science that they were in fact rational; this remains, as he says, "entirely a matter of faith" (Laudan 1977, p. 161). As McMullin pointed out in his review article, Laudan fails in his

> attempt to 'invert the presumed dependency of progress on rationality' [p. 6] ... At the crucial moment, he falls back on intuitions of rationality. (McMullin 1979, p. 632.)

Let me summarize now my criticism of naturalized and historicized epistemologies. As I see it, their common feature and fundamental mistake consist in their striving to find a single, static metascientific theory of rationality for the evaluation of historically changing scientific methods or scientific rationality. The principles of this unique rationality theory, which could justify methodological change, are assumed by the proponents of the above-discussed standpoints to be attainable by a kind of inductive inference either from empirical psychological investigations of human cognitive capacities (as in Quine) or from a historical investigation of actual science (as in Lakatos, Laudan and others). That is, they assume that by historico-anthropological investigations a single (unique) theory of metascientific rationality will emerge, in spite of, and underlying all historical change of scientific methods. That is why this problem can – to quote Hempel – "be regarded as a modern outgrowth of the classical problem of induction" (Hempel 1983, p. 92). And, in fact, all these attempts at

conceiving of metascientific rationality as a historical or naturalistic invariant have tacitly been assuming a 'principle of uniformity' – as is usual with validating induction. This principle of uniformity is, in the case of the naturalized epistemologies, the assumption of the historico-genetical invariance of the cognitive agent (i. e. man) as far as his perceptual, conceptual and reasoning capacities are concerned (they are assumed to be genetically given); in the case of the historicized epistemologies, on the other hand, it is the assumption that the *goal* of science has remained the same during its whole history from the beginnings up to now. In other words, that the meaning of the term 'science' remains the same in different ages and societies.

Thus epistemology naturalized and epistemology historicized can account for change of methods only if the changes ringing on scientific methods were such as to allow us to better achieve *the* (constant) goal of science; that is, increasing predictivity – as with Lakatos, or empirical verisimilitude – as with Newton-Smith, who expressly denies "that there has been an evolution in the goals of science" (in his 1981, p. 269), or increasing problem-solving effectivity – as with Laudan, and formerly Kuhn; or, again, promoting some unspecified form of survival in an invariant Nature, as with the naturalists.

I think, however, that the principle of uniformity underlying the inductive construction of a general, trans-cultural theory of rationality has to be given up (in both of its forms, i. e. as the invariance of the cognitive agent, and as invariance of the goal of science). I take the first-mentioned form of it as a remnant of the epistemological individualism of empiricism which posited a single standard individual as the knowing subject, took scientific method as apt for being practiced by isolated individuals, and scientific theory as the sum total of the products of the working of abstract individuals endowed by Nature with the capacity to learn from experience and follow the transcendentally given logical norms.

To use a Wittgensteinian analogy: this was a kind of 'private language' model of science.

But that science is an essentially collective – or more than that, a social – enterprise was formerly pointed out and argued for – without any echo – in the thirties by L. Fleck, later on by Kuhn, and recently by the proponents of the Strong Programme, Barnes (1977) and Bloor (1976). The latter has shown (in his 1982) that our fundamental conceptual net and our perceptive competence by which we are able to perceive empirical similarities are essentially socially determined and change with changing social conditions.

I do not want to expand, however, on this point; I would, rather, like to argue for the change in the goal, or rather in the system of goals or objectives of science and their social determination in the course of its history. I think, namely, that the seeming irrationality, the 'anything goes' character of changing scientific methods disappears if we construe them as changing *means* to achieve diverse, changing *goals* and not, as so far, as capriciously and unaccountably or even evolutively changing or improving means for achieving a constant goal, say truth, or predictivity. Formerly, that is, in the pre-Kuhnian philosophy of science, it was *the* scientific method which was taken as static and unchanging, as the definitive feature of anything worth being called 'science'.

And now it is the goal of science which is assumed by all the above-discussed authors to be something unique and unchanging. The authors differ only in what they take as goal: for instance truth, predictivity or problem-solving effectivity. In my view it is, however, inadmissible because anachronistic to project e. g. predictivity (one of the goals of our modern science) back, say, in the Middle Ages, and then give a poor mark to their methods for scoring low in predictivity. And no wonder that in doing this, philosophers of science relegate the causes of this seeming irrationality of scholastic methods to the 'external history' of science, that is, try to account for the methodological divergencies in terms of so-

cial influences or interests, inhibiting (or hindering) the free development of natural or scientific rationality. In this conception, the changes in scientific method are so many steps on the road towards freedom of social influences and social determinatedness. And the history of science appears to be an evolutionary process towards a pure methodological ideal, embodying uninfluenced scientific rationality. In this way cumulativity seems to reappear on the level of scientific methods, after it was lost, according to the Kuhnian picture, on the level of scientific theories. Thus, although the conception of more and more encompassing deductively inclusive theories had to be given up (after Kuhn), some strived to regain a picture of the history of science which improves on *methods*, and in which, hence, the idea of progress is restored. This picture, though very attractive, is, I think, false. In my view we should rather go one step further on the Kuhnian way and purge the – perhaps – last remnants of the Received View: the assumption of the invariance and unicity of the goal of science. Science is a multi-purpose enterprise. It is in vain and void of sense to seek for a single goal which would define and govern scientific cognition during its whole history from the beginnings onwards. For to say that science is and has ever been a search for truth is vacuous and ideological; that it is and was a search for empirically more successful theories is true, at most, for the science of the last two hundred years; and that it is, say, a search for more and more comprehensive theories is vacuous as well, for this is a mere syntactical feature without epistemological content. For let us take truth as the alleged universal goal. Now, truth *simpliciter* is of no use in this context. Taken qualified, it appears that the goal of e.g. Aristotelian science was the discovery of *necessary* truth and not any truth whatever. Accordingly, the method of this science was taken to be demonstration (by apodictic-deductive inference drawn from necessarily true first principles). Another component of its goal or system of goals was intelligibility (as distinct from our preferred predictivity), that is, a search for *illuminating*, or enlighten-

ing truths. Accordingly, the use of mathematics, as a methodological tool, was confined to certain disciplines (e. g. astronomy, but it was not to be used in physics). And that experimentation had been avoided can be accounted for by referring to the goal of the Aristotelian scientists, which was: to acquire knowledge about the *spontaneous, uninterfered with*, workings of nature, which excluded any artificial setting of the stage for cognition.

Later on, at about the end of the 17th century, the goal changed for a search for contingent, empirically predictive truth.

At first these truths were supposed to be empirically certain, later on to have a high degree of probability. Accordingly, in order to serve the newly emerged goal of empirical predictivity, inductive reasoning came to the foreground in methodology and the use of mathematics became universal. And:

> If the basic hypotheses of science were not candidates for being proved demonstratively but were to be supported by inductive arguments, it was natural to explore the possibility that the theory of probability could be invoked in representing the process of theory choice. (Newton-Smith 1981, p. 215.)

Moreover the aim of Aristotelian and to a great extent of medieval science was to demarcate itself as genuine, reliable and systematic knowledge *(epistemé, scientia)* from scattered lay beliefs *(doxa, opinio)* regarding nature, and to delimit thereby a territory of its own as distinct from mere practical, ordinary cognition. Accordingly, while leaning heavily on ordinary experience, their methods differed from ordinary cognition in using expert opinion (authorities) and non-ordinary, syllogistic forms of reasoning in justifying their knowledge claims. From about Galileo's time on, the main purpose changed for demarcating science from another form of expert knowledge, *theology*, which claimed general cognitive authority, and used the methods of divine revelation and expert opinion (the Scriptures and privileged commentaries) to justify its claims. Accordingly, the – mainly self-appointed – scientists of the 17th century had to find other methods by which to produce

and justify a new *kind* of knowledge. Descartes was seeking for fundaments which were to be as firm as the former one, and upon which necessary truths could have been erected. Newton, however, rested content with contingent though more precise and predictive truth, thus with the experimental method and inductive reasoning. (At least as far as his rhetoric goes.) Later on a new goal emerged which was to demarcate science from *politics* and from *non-institutionalized* science (so-called pseudo-science). That meant the denial of any connection with social factors and influences on science. This gave birth to the myth of its neutrality and induced the search for those transcendental methodological norms which were supposed to be trans-culturally valid and could govern non-committed and legitimate cognition. Today the fight goes on for official (social) recognition (and thus for financial support), for *legitimacy* (and prestige) and that is why the problem of rationality of science, the uniqueness of its methods and by that token the privileged status of science among other cognitive enterprises seems to be so important in epistemology.

Goals change as radically as scientific methods, the variation of which is dependent on the goals they serve to accomplish. Methods are not, as it were falsified, that is, rejected in a process of metascientific justification, nor are they continuously improved upon (except *within* a paradigm); they are, rather, abandoned when not yielding the expected results or when new goals emerge which require new methods to achieve them. Methods and their changes can thus be justified only in relation to their goals and to the changes these goals undergo. Thus the rationality of scientific methods should be construed, I think, as a means–ends relationship. We must not try to justify scientific methods within traditional epistemology, lest we should face the notorious 'Münchhausen (or Fries) trilemma' of rationality (see Murphy and Hendricks 1984). Scientific rationality could thus be taken as a kind of practical rationality, following the lines of reasoning of I. Levi (1973, 1980) and C. Glymour (1980) who argued that scientific

inquiry is itself a form of practical deliberation, though one with distinctive cognitive goals; and further that certainty, hence rationality, are pragmatic, problem-relative notions. Methods gain a privileged status with respect to the goals attainable by their means. Means and ends (scientific methods and goals) are at the same time supporting each other; given the goals, the means appear to be the best available for attaining them, and once given the methods, *no other goals* seem attainable or *worth while* attaining. In my view, then, the problem of the justification of scientific rationality and the establishing of its privileged status is not a purely epistemological task, it cannot be solved within the confines of traditional philosophy of science. That is why it is in a state of crisis now, and needs the help of the sociology of knowledge, which I suggest to supplement with a *sociology of methodologies* – following the advice of Mary Hesse (1982). Hence, I claim, the goals as well as the means (methods) of science are, in the last resort, socially determined, and cannot be explained nor justified without referring to their social context.

Otherwise the question would remain unanswered which lurks in Lakatos's following words criticizing Popper's methodology:

> Just as the rules of chess do not explain why some people should play the game and, indeed, devote their lives to it, the rules of science do not explain why some people should play the game of science and, indeed, devote their lives to it. The rules decide whether a particular *move* is 'proper' (or 'scientific') or not, but they remain silent whether *the game as a whole* is 'proper' (or rational) or not. The rules say nothing either about the (psychological) motives of the players or about the (rational) purpose of the game. (Lakatos 1974, p. 313.)

Now, I would not regard science as a single-purpose game (like chess) during its whole history, but as a multi-purpose game, or rather a set or family of games connected by a kind of family resemblance. What this family resemblance consists in cannot be exactly given as a set of necessary and conjunctively sufficient conditions defining scientific rationality. For, as Kuhn has argued

lately (in his 1983), what 'science' means changes with the changing context of discourse, and can be defined only relative to other contemporary disciplinary clusters or cognitive enterprises like 'medicine', 'law', 'arts', 'philosophy' or 'theology'; but in each discourse 'rationality' is a necessary property of the referents of 'science'. Kuhn's solution for the problem of metascientific justification is thus that 'rationality' and 'justification' are interdefined terms. And so, in other words, the attribution of rationality to science is necessary and true in Kuhn's language but contingent and false in Feyerabend's (see Hempel 1983, p. 571). For Kuhn rationality is thus relativized to a language game. This Kuhnian standpoint seems to be correct but, without further assumptions, leads to a sterile form of relativism. In order to evade this devastating relativism, which is not necessarily a consequence of self-reflection in the methodology of science, we have to put and then try to answer the above, Lakatosian question: why indeed should people play the game of science? Or rather different games with different rules – methods – in different ages, equally called science?

This question concerns the meaning or reason of the language game itself. Thus, it cannot be answered *within* the game. Kuhn (1983) is aware of this when closing his paper with contending that whoever questions the rationality of science, that is, of learning from experience or that the conclusions based upon it are justified,

> asks ... not for a justification of learning from experience, but for an explanation of the viability of the whole language-game that involves 'induction' and underpins the form of life we live.
>
> To that question – he says – I attempt no answer, but I would like one. (Kuhn 1983, p. 570.)

In order to find an answer to the question why man should play in certain ages and in certain societies a game like science (besides, instead of, and distinct from other games like art, religion, etc.) or why in certain societies they did not, we have to transcend the

confines of science or even epistemology itself and look for a wider social context.

Let me quote Hesse:

> It is not only rationalist illusions about the 'truth'-claims of science that need sociological scrutiny but also the social fact that science as a play is tolerated and indeed encouraged in certain sorts of society, just as religion appeared to be tolerated. (Hesse 1982, p. 331.)

The reasonableness or rationality of a game (including the rationality of science) consists partly in its having rules appropriate to attain its goal, and partly in its having a goal attainable by the rules. The goal, however, is (in the last resort) extraneously given and established; though it sometimes seems – after the game is set, with its goal and rules – as if it were the rules which appoint and establish the goal as the only attainable end of the game.

In the case of science the question is how the goals are appointed, by what authority, which interest-groups fight for the right to determine what goals are legitimate and by what means they can be attained, relegating thereby into a marginal position or into the rank of pseudo-science other, socially non-legitimized cognitive enterprises.

In my view, then, there is an urgent need for the sociologization of epistemology and for the elaboration of a sociology of methodologies (along the lines of the Strong Programme) in order to solve the problem of scientific rationality and to avoid the sterile form of relativism which seemed to follow the self-reflection of methodology. The alternative is not a return to former absolutism, but a fertile form of relativism: the relativization and embedding of knowledge into its social context.

We would thus have no absolute epistemological justification for scientific methods, only a sociological one, by grounding and explaining them as the best contemporarily and socially available tools for achieving socially given goals. As is to be seen, the socially given goal plays in my conception the role played by the na-

turalistic notion of 'the general standing interest' (in Quine's and Newton-Smith's sense). What I am recommending is to historicize and sociologize the goals and by that the methodologies of sciences. And finally I suggest separating the problem of rationality from that of progress. Thus, I think, we should not try to derive either progress from rationality (as Lakatos and others do) or rationality from progress (as Laudan does). Rather, we ought to reduce progress to that of society (whatever that may mean, or let it fall) and drop the search after a universal and purely intellectual concept of rationality. Rationality as we mean it is a special product of certain kinds of societies. The choice between rival paradigms is not a question of rational deliberation (according to some trans-paradigmatic norms, though reasons play a role in it) but a task to be solved by the society which gives new goals for science and demands new results to be attained with new methods. Scientific development is a rational process in which the basic scientific choices are reasonable but underdetermined by reasons and thus in need of an ineliminable dimension of psychological and sociological explanation.

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IMRE HRONSZKY

THE PHOENIX

(Early and Recent Socio-Historical Approaches to Scientific Cognition)

In the early seventies there was a turn in the understanding of science and its history.[1] Its hardest core was an extension of the sociology of knowledge to the topics of the so-called 'hard sciences'. The least that can be said about the new perspective is that it challenges the 'standard view' of science acknowledged almost without criticism for so long. (The expression 'standard view' is used here as it was introduced in the debate on Kuhn's *Structure* in the mid-sixties in order to emphasize the common core of fundamental beliefs evidently shared so far by scientists, and the 'common opinion' of the philosophers of science: science as a model and symbol of rational knowledge, the necessary autonomy of the scientific community, scientists as guardians of the objectivity and rationality of science through their consistent application of scientific method.)

The Kuhnian Gestalt switch touched simultaneously upon the philosophy and history of science, attacked some of the characterizations of science commonly accepted earlier in the received view, perhaps first of all the cumulation view of experience, and queried the normative approach of the philosophers of science to the history of scientific cognition (the attitude of 'imputing' rationality to the history of science, to use the term of E. McMullin). It brought historicity and a moderate sociology (and psychology) into the understanding of scientific cognition; and the knowledge-sociologist attack attempted to transform the whole picture into its opposite. Science should be understood as a thoroughly social phenomenon. Apart from the differences between the various and

Imre Hronszky, Márta Fehér and Balázs Dajka (eds), Scientific Knowledge Socialized, 97–121.
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sometimes even diverging tendencies within the new perspective there is a common denominator of all tendencies. This is the aim of the 'social phenomenon'-type reconstruction of scientific cognition and knowledge. To put it differently: they all claim the acknowledgement of the fact that their sociological perspective is epistemologically relevant, too.

It is useful to differentiate between two things. The *'social conditioning'* of scientific cognition can be used to refer to those conceptions which limit themselves so that they refer to certain presumed necessary social conditions of scientific cognition without acknowledging that these conditions (or at least some of them) have any epistemologically relevant, epistemologically constructive role. The term *'knowledge-sociological'* refers then to those approaches that ascribe an epistemologically constructive role to (at least some) social conditions. Limited social-constructivist approaches were applied already in the early 20th century. (It is only part of the truth that neither Durkheim nor, later, Mannheim crossed the Rubicon.) It might be of some importance to outline these approaches.

The conceptualization of the story of the social interpretation of scientific cognition and knowledge is made here from the perspective of the process of *deconstructing that reificationist understanding* of science which prevailed throughout the numerous successive variations of the 'standard view'. This reificationist approach to science can be observed in those prevailing dogmatically fixed a priori convictions that characterize the various consecutive forms of demarcationist approaches to science in the philosophy and history of science. The story of meta-reflections on scientific cognition can be understood as the big retreat of the reificationist perspective with periods of renewal in increasingly moderate forms from the beginning as a positivistic-scientistic view up to the recent rationalistic defences of this perspective. Knowledge-sociological perspectives show themselves as attacks from outside made in two historical periods. The first part of the present study will deal

with the early, limited knowledge-sociological approaches. It will be shown that there were non-Marxist as well as Marxist variants of them. As a common denominator, their non-liberalist political ideology might be depicted.

The first author to be considered is Thorstein Veblen. Shortly after the turn of the century Veblen sketched an understanding of the *content and truth canons* of scientific knowledge as generated and valid in a type of society that became based on the 'social institutions' of machine-based production and commerce.[2] In a well-known but mostly misunderstood sketch Veblen looked for the social determinatedness of scientific cognition conceived of as a special type of cognition worked out under the impulse of 'idle curiosity' (or as he sometimes called it, 'irrelevant attention'). Working with this anthropomorphic universal, 'idle curiosity', rooted already in animal behaviour, he tries to find *functionally determined congruences* between the 'prevailing institutional structure' of society and the therein typical form of 'esoteric knowledge'. The changes of the types of 'esoteric knowledge' will not occur in his model in the form of a 'logical process', but the new type will dominate as a metaphysics in harmony with the changed institutional structure. The motivation for the change of ontology and validity criteria will be formed by life, not by the autonomous change of thought. Although this 'esoteric knowledge' has no immediate pragmatic relevance to the life of individuals, it has the most important mediating pragmatic role in society, allowing for the adaptation to the community's ruling institutions. The functioning of 'esoteric knowledge' is secured by the methods of cognition and the structure of knowledge. His epistemological starting-point is that to be able to find, to collect and systematize experiences, a previous 'dramatization' is needed, and just the basic social institutional structures assure the dramatization models.

But Veblen was a transitional figure even in the early 20th century. Strangely enough, his socio-historical approach was used to explain the rise and legitimacy of the *unconditioned objectivity*

of modern science. In the modern period 'idle curiosity' received the form of scientific spirit. As to Veblen, the development of modern science had been an advance of *'disanthropomorphization'*. Esoteric knowledge was already based on causality chains in early modern science, but the 'dramatization' was still based on the picture of the craftsman. Causality was understood as the relation between an efficient cause and a certain result, for the achievement of which it was necessary. Writing in the 19th century, Veblen continued his explanation of the world conceived of as a machine, or, what according to him is just the same, as a causal mechanism, a chain of consecutive changes, corresponding to the pressure exerted by industry on thinking. Science is the 'machine-made point of view', the 'metaphysics of machine technology'. The prevailing institutions in society constrain the 'hard-headed acknowledgement' of factuality in life, and this attitude has reached its summit and symbol in science.

Scientific knowledge is for Veblen objective knowledge founded on two methods. 'Disanthropomorphization' makes possible dispassionate, impersonal knowledge (and 'judgemental relativism' will be excluded from the progress of science), and the 'matter-of-fact' character of knowledge will be achieved when the world is understood as an eternal process of consecutive causal changes. Veblen tries to find the 'social conditions' of scientific cognition (as we have introduced the term above) and his approach has the character of an epistemologically relevant sociological approach: he speaks of the necessary role of machine-based production (and commerce) as the 'dramatizing principle' in obtaining scientific knowledge. But the story is more complicated than this, as he conceives of the simple congruence of perceiving the world as a machine and as a consecutive change of causal connections.[3] The explanatory efforts of Veblen were reduced to the task of explaining a presumed possibility of impartial observations and the coming into being of a social institution, the working of which assures

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an ontological net congruent with the working of the world outside human practice.

Hence, in Veblen's view, scientific attitude is appropriate to nature; yet it is not a natural one, but something to be appropriated in specially built up types of society, in the course of a historical process.

1. THE SOCIAL CONSTRUCTION OF SCIENTIFIC KNOWLEDGE AS RESTRICTED OBJECTIVITY AND RATIONALITY – A CONSERVATIVE VARIANT

The second non-Marxist author to be analyzed briefly is Max Scheler. As will be known, his ideas about the rise, development and rightful validity claim of 'positive science' were part of the 'capitalism-debate' at the turn of the 20th century. Max Scheler's views embody an alienation and reification criticism from the perspective of a conservative thinker. The task to be solved was to demonstrate that science is only a cognitive activity of lower level and not objectivity per se as claimed by the positivists.

The task emerging with this knowledge claim was to make plausible how scientific behaviour arose as part and parcel of societal forces which needed it just because of its possible societal function. To sum up, after Scheler the function of science is to serve a special sort of wish for domination *(Herrschaftswille)*, for power over technological disposition *(technische Verfügungsmacht)*.[4] In this reconstruction the emergence of modern science is due to the historical change in which, instead of the former wish for immediate domination over human beings, a new wish emerged aspiring to the productive remaking of natural things. He argues committedly against the types of explanation based on 'pure reason'. He uses 'instinct' and 'ethos' as explanatory variables and sees the ground for the transformation of thinking in the transformation of life, of which thinking makes a part. 'Positive science' was modelled as being a structure or mechanism determined by its only

function of prediction. After Scheler the communal way of life *(gemeinschaftliche Lebensform)* characteristic of the Middle Ages was transformed into a societal way of life *(gesellschaftliche Lebensform)*, and along with this, the 'categorial-biomorph' world view corresponding to it had to change of necessity, too. (In addition to the way of life, the nature of technics was also involved in the explanation.)

According to Scheler, the main issue to be explained is the *transformation, determined from outside, of the cognitive goals.* Concerning the rise of 'positive science', there is an *extreme restriction* of the cognitive interest to be explained. It is nothing but the goal restricted to the demand of the *regular prediction* of natural processes (and that of the soul!) that gave rise to a perspective from which Nature became deprived of the objectivity of sensual qualities and restricted to a mechanism, and which reduced the cognitive interest to the measurable-quantitative aspect of the world and its laws. What is behind the transformation of cognitive goals is the wish to rule over Nature (and the soul). Science is objective within these restrictions. Those who want to transform things productively have to catch those things in normalized forms and develop the capacity for regular prediction.

Scheler gives an explanation of the coming into being of the value-freedom of 'positive science'. "The social reason for conceiving of the world on the basis of value-freedom is a task which is set for the *purpose of some value: for the purpose of the vital value of the rule and command over things.*"[5] Objective knowledge (in its restricted form) is generated and validated through a narrowing down of social goals as a social precondition, itself constitutive of the form of knowledge in this respect.

The scope and limits of Scheler's approach to the constitutive role of social factors can be exemplified by two remarks. He argued rightly against the rationalistic explanation of the coming into being of modern science. The search for antecedent ideas cannot explain why they were used as such just at that time and in

that very form. Scheler believed himself to be able to explain the functioning of the methods of 'positive science' through its goal of being able to make regular predictions. So he conceived of the function of 'positive science' according to its *inherent laws*. With this assumption he supposed the constancy of the goal structure of science and the unity of scientific method belonging to this goal, and he deprived himself of the fruitful question of what the social constructive effects were on the fine structure of science.

2. SOCIAL CONDITIONING OR SOCIAL CONSTRUCTION OF SCIENTIFIC COGNITION? ON EARLY MARXIST VARIANTS

It is a widespread mistake concerning the evaluation of Marxist thinking about the social history of scientific cognition to suppose that there was a uniform Marxist understanding at the turn of the thirties and that is was embodied in the analysis of Boris Hessen (1931) about Newton, itself being the ancestor model of 'externalist' thinking.[6] This part of the study starts by recalling the fact that the basic alternatives of understanding society and history within Marxist thinking can be grasped by reading Bukharin's book *Historical Materialism* and Lukács's *History and Class Consciousness*. Roughly speaking, Bukharin based his model on the assumption of the final determining role of productive forces that were understood in a reductionistic way. Lukács, when treating the social processes, assumed that changing the production relations would reform the whole complexity of social relations. Bukharin reduced the productive forces to technology, giving a neutralized definition to the latter. Lukács's understanding of the production relations and their assumed totalizing effect in society called for a social-constructivist approach to scientific cognition, whereas Bukharin's approach allowed one to treat science as if it were socially determined in its institutional and organizational structure, but not in its cognitive structure. The difference can be

characterized as follows. The 'messianistic-dogmatic' wing (to use Lukács's later expression) of socialist theoreticians to which Lukács also belonged tried to demonstrate how deeply social relations and production relations concretely formed a sort of *scientific cognition functionally adequate* to these relations in both their productive force and their ideological role (world view).

The emphasis was put on the difference between the assumed sorts of cognition. Besides the Lukácsian view, one of the main variants was that of Bogdanow. In his model Bogdanow ascribed different cognitive perspectives to different social classes in bourgeois society, which perspectives themselves were taken to work functionally, according to the producer and controller roles in society. He assumed that these opposite roles were interiorized as cognitive activities of different sorts into science, too, as experimental and theoretical activities, respectively. That is why as a part of his *'proletcult'* movement Bogdanow claimed to develop a new type of science, too. The fact that the emphasis in the theory was put on the continuity or discontinuity of science in the bourgeois and emerging socialist types of society was of course of vital practical-political and ideological importance.

The problem to be solved was how to behave towards such an institution as science in a new type of society, whether it was enough to develop a new type of institutional and organizational scientific structure, or something more should be done: that is, to develop a form of science appropriate to the needs of the new type of society (the needs of productive forces and of the type of world view), is it necessary to develop a new kind of cognitive activity transcending the earlier one, as 'messianistic-dogmatic' writers claimed?

Karl August Wittfogel, in a forgotten book against Bogdanowism, tried to demonstrate a lot of organizational weaknesses in science, regarding them as effects of the individualistic way of organizing scientific activity in bourgeois society.[7] These organizational weaknesses were exactly those features which according to him

should be transformed. In a review of Wittfogel's book Lukács wrote (he used the critique of this book as the application of his ideas on the social production of natural scientific knowledge, which were developed in his *Class Consciousness*): "Nature and all of its forms of mastering are social categories", and he showed Wittfogel the path already taken by bourgeois sociologists.[8] What did Lukács mean by this formulation? Lukács started off with the assumption, as a hidden dispute also with Max Weber's rationality conception, that developed commodity production affected the transformation of all social activities. In a type of society based on the domination of 'formal, calculative rationality', cognitive activity also accommodated to this reification process and became a constitutive part of it. This makes understandable both its higher quality in comparison with the earlier view on the cognition of Nature based on predictivity, as well as its inherent barriers. Just as the cognitive attitude to Nature, the perspective of cognition should be developed further to achieve, as Lukács claimed, a non-formalistic cognitive attitude, which would not be reduced to calculative rationality. And Lukács claimed that this development should be an integral part of developing a new culture. The new science should be deeply philosophical.[9]

We can see that left-wing thinkers developed within Marxism an understanding of science according to which cognitive activity was regarded as formed by society through epistemologically relevant constitutive effects. The cognitive attitudes, it was assumed, were and would be formed and reformed. But none of them tried to work out a microsociologically relevant understanding.

What about then Hessen?

Just contrary to this totalizing understanding of the social effects of production relations, a technocratic-economistic understanding of social history was also developed. (It should not be forgotten that productive forces themselves were also often understood as restricted to the means of production, or, what is more, to working tools, to technics.) This variant of Marxism looked for

the opportunities for accelerating economic and technological development (cf. the neutralized form of questioning!), and hoped to find a social instrument in scientific development. Natural science, understood either as the theory of the operation of 'machines' or as the immediately objective knowledge of Nature, was to be thought of as something that had developed through the effect of economic-technical needs. Positivistic-scientistic objectivism was taken over and used as an epistemological basis. Therein originated the double formula (so often used in the later period) that for the enforcement of the 'scientific way of thinking' we should get rid of all of the 'metaphysical ideologies' hindering it, giving rise to a 'science-appropriate' ontology (materialism) as a philosophical basis and we should give impulses to science from the outside through the emerging needs of economy and technology. Technology is on the one hand a motivating factor for scientific development and, on the other hand, raises problems to be solved scientifically. With the development of the scientific attitude, its objectivity of a new quality (dialectical materialist science) and, on the basis of this, its new quality of technological usefulness were conceptualized. All this is to be read in summary in Bukharin's famous introduction to *Science at the Cross-roads*. In the light of this Hessen's analysis appears as an application of this formula to a historical situation serving as a historical example.

Here it may be of some interest to mention that Boris Hessen, so often called the 'father of externalism', presupposed the autonomy of scientific cognition, 'acknowledging' its closed and timeless internal laws of development which have their origin in a positivistically conceptualized cognitive situation. His externalism was reduced to two epistemologically *non-relevant* factors, thinking of technology as being a *force motrice* of science through its needs and the problems to be solved. The appropriate expression to be used for this understanding of science is perhaps *'externalist internalism'*, expressing the eclectic nature of the view. It was not an appropriate framework to solve the special epistemologically rele-

vant problems of the *emergence of the special cognitive attitude(s) typical of science*, as it was simply impossible to raise this problem in it. The only detailed epistemologically relevant approach (within Marxism) to the understanding of the emergence of the cognitive attitude typical of modern science – which was then social constructivism – was written at the turn of the thirties by Franz Borkenau, then a Marxist of left-wing orientation. His book was entitled *Der Übergang vom feudalen zum bürgerlichen Weltbild: Studien zur Geschichte der Philosophie der Manufakturperiode*,[10] in which he made use of Lukács's above-mentioned ideas, trying to develop some earlier research done by Cassirer in *Das Erkenntnisproblem* into a concrete historical explanation of Marxist type.

According to Cassirer, the mechanical point of view came into being as a common categorial system of the new science and philosophy. Moreover, the domination of knowledge-critical philosophy and that of the mechanical point of view came into being in one process conditioning each other. Borkenau criticized Cassirer's conception for *remaining at the level of plain description.* The further task, he said, is to deduce this change of cognitive attitude from historical processes which are of socio-ontological nature (to use a modern expression). What he claimed was the *historical sociology* of the change of the reasoning attitude and of the ontology of Nature.

How did he think the ontological categories and the cognitive methodology (and goal) had changed when they were forced to do so by the overall changes in social being up to the mid-17th century? In Borkenau's opinion this transformational process had two main levels. First, he traced the origin of the mechanical world picture in the special division of work of manufacturing type as being the blueprint of natural objects as they appear within the division of manufacturing work. According to him, the basic categories and methods of science owe their origin only partially to the interaction between man and Nature in work. The ideas which emerged in the course of the interaction between man and

Nature always had to fit into the whole world picture and also to the knowledge of social processes concerning man–man relations. Furthermore, he ascribed the main impulse to the category-forming effect of societal experiences. For this reason he set himself the task of showing the origin of the main categories of the mechanical point of view as they reflected the changes in social being, that is, in the societal position of the individual. Therefore he tried to demonstrate the changes in the conception of natural law in the political quarrels of the 15th-17th centuries, reconstructing the societal-political fights and the ideas of those taking part in the party struggles. Having a look at the transformation of the basic ontological categories and cognitive attitudes towards Nature as being *overdetermined*, he concentrated his work on the changes in the societal-political framework effecting changes in the world view. Here we have a non-cumulative view of the scientific attitude in its basic characteristics. It is interesting how he argued against the positivistic understanding of scientific cognition, using as evidence the emergence of quantum mechanics. An attack against positivism would have remained earlier an abstract epistemological, philosophical issue, a curious attack against the most certain layer of knowledge, he wrote. The assumed universal validity of mechanics prohibited the questioning of the assumed objective (understood as merely reflective) and cumulative nature of scientific cognition, reducing the curiosity about the process of getting rid of those hindrances which were assumed to distort the natural cognitive attitude. He shared Cassirer's evaluation that the emergence of quantum mechanics made these conceptions dubious, just within scientific practice. In Borkenau's view the cognitive attitudes towards Nature were formed in a double process of material action in work and in wider social processes determining the world view. Knowledge reflects Nature as it shows itself, as the product of this two-sided process. Some Marxist reflections may have thought that his approach was nothing but an implanted Kantian epistemology within Marxism, rejecting it because of its

'agnosticism', but Borkenau's view that the type of organization of work may have affected the knowledge gained does not necessarily imply agnostic features. It can be brought in harmony even with the idea of advancing true knowledge by means of forming a successive series of work interactions with Nature, in which series the 'mechanical part' of the world is that which is explorable through the mechanistic division of work. Such a view may not run contrary to the assumption of the possible radical changes of all of our ontological categories.

Borkenau's aim was to show that a new view of Nature and its cognitive appropriation fitted integrally into social practice as a whole. The new cognitive orientation unified the bourgeois interest in the rationalization of production and the transformation of socio-political conditions, and afforded such a science that could hopefully serve a new form of production and function as ideology.

But the fate of a historical explanation is determined eventually not by its intelligibility, but by its proved explanatory power. And some of the vitally important assumptions of Borkenau's model were proved untenable. The direct fate of Borkenau's hypothesis is rather well known. Here we have in mind Henryk Grossmann's answer (at that time also a member of the Frankfurt School, but neither he nor Borkenau had anything in common with the later Critical Theory approach), in which he proved point by point with regard to all the essential issues that Borkenau's explanation was untenable.[11] He pointed out rightly that at the beginning of the 17th century, organic manufacture simply did not yet exist, that the presentation of the party fights was problematic, and so on. The endeavour to find the basic social ontological structure that determined the cognitive structure and the basic ontological assumptions of the new science in the mode of the division of labour came to a deadlock.

To sketch an alternative, Grossmann returned to the widespread view of his time which emphasized the role of the exploration of

machines in the emergence and development of mechanics. This turn was based on a naive epistemological point of view. In his opinion the need for the more intensive exploration of machines can lead easily to the knowledge of the *laws* of mechanics. (So he thought that Leonardo took certain basic laws of mechanics as if they had already assumed their scientific lawlike form.) He also supposed that a step by step generalization of mechanics, that is, induction by way of analogies, had led to the mechanical world view.

We have to pay attention to the fact that in their debate they explained the origin of modern science in two different ways, as they accepted two different notions of science. According to Grossmann, the simple knowledge of technical rules, when getting a mathematized form, makes out already a scientific law, and the world picture is nothing but the extension of scientific knowledge into the whole world by means of analogies. The branch of Marxism represented by Grossmann acknowledged uncritically the inductivist-empiricist-positivist model of scientific cognition and gave it a simplistic materialist interpretation (cf. later especially Bernal).

Differing with Grossmann, Borkenau conceptualized scientific knowledge as theory, an ontology whose elaboration will be made in the structurally interconnected process of understanding societal and natural processes. Those who accepted a social-constructivist approach to scientific cognition understood the vital importance of the first step to be made when they tried to prove the constitutive acts in working out the basic cognitive attitude and ontological system characteristic of the new science.

It is obvious that after Koyré's results a Grossmanian type of historiography of science was demonstrated to be naive on the grounds that it was not able to see the difference between the two types of knowledge (exemplified, say, by the facts experienced and by theoretical facts), and that, consequently, it tried to point out a continuity where there was a *jump* to be explained. If the

specificity of theoretical science could then be used indeed against the simplistic externalist modes of explaining, nevertheless it would not prove that the history of science cannot work out a social-constructivist understanding of the emergence of theoretical science within the complex of cognitive activities.

I think that the foregoing is enough to demonstrate that some sort of social-constructivist approach had already been developed in the first three decades of the present century. All of them were *macrosociological* in character. Karin Knorr-Cetina expresses her scepticism, as she thinks these sorts of models are based only on congruencies and that the genetic side is spared (dismissed). So these sorts of explanations necessarily remain more than doubtful.[12] This warning is important, nevertheless I think there is something in this objection that cannot be accepted. First, the best macrosociological social-constructivist approaches use a fine mixture of congruential, functional, symbolic and structural argumentation that is not reducible to simple aims of congruence. Secondly, tendencies can be made probable even when vitally important genetic chains (types of questions such as what is the mechanism of the process by which an individual is influenced by some assumed socially constructivist effects in his or her cognitive activity) cannot yet be reconstructed. (This is the problem with which Edgar Zilsel had so much to do in his pioneering studies on the early 17th century.) Nevertheless, there is at least one other serious problem beyond that of the unbelievability of simplistic congruence models. This is the threat of *oversimplification* when defining the object of research. (Remember that Scheler defined science as being nothing but a cognitive apparatus to give regular predictions based on generalizations rooted in a system of laws.) This 'nothing but' reductionism can be highly misleading when followers of some perspective insist on it even when complicated organic ties begin to appear evident.

We have tried to describe knowledge-sociological approaches to scientific cognition in the early 20th century and compared them

with those that remained satisfied with the more moderate 'social conditionist', or, to put it differently, the 'science-in-context' approach. It would be very important for the awareness of scientific research to try to explain from the social situation why they all committed themselves to *limited* knowledge-sociological approaches making *a priori concessions*, but there is no place here to do it. Instead, the first part of the presentation will be closed by asking for the comparison between the classification introduced here and the common understanding of the story of meta-reflections on the history of science classifying the approaches as externalist or internalist. It is well known that this classification was introduced into the debate by internalists in order to separate themselves from all types of people who anyway rejected the idea that the mind works in itself for itself. Hessen or Merton would then be misleadingly thrown into the same bag as the early Lukács or the microsociologists. To demonstrate the basic line of demarcation to be used (within the 'externalist' class?), let us see for a moment the 'externalism' of such 'social-conditionist' researchers as Hessen or Merton. First, we have to make a distinction between two questions – one about the motivation structure of scientific work, the other about the existence of an immanent rationality of scientific cognition, or, to put it differently, the question of the immanent laws of scientific cognition. After the model of Hessen, Newton's *Principia* answered to knowledge needs mainly arising from the technical needs of his age; as a scientist he worked out the principles necessary to the solution. (The same applies to the problems of which he is assumed to have been aware.) In this respect this model is not only 'externalist' but crudely reductionist. First, it is question-begging when it assumes an immediate motivating effect which mobilizes scientific cognition without any mediations only through the synthesizing overview of the knowledge needs of technology. It narrows not only the praxis understanding of original Marxism, but it makes it difficult to get a historical epoch-adequate understanding of the scope of praxis, too.

That is why Hessen's later opponent, G. N. Clark, could ask in 1938 why Hessen had forgotten about such important areas of practice as medicine and music. By the way, Clark gave a 'social-constructivist' interpretation to the emergence of the new, calculative attitude in early modern science when he referred to merchants and so on. But the whole of his attitude was internalist, making sure that the most important factors in the emergence of the new science were the 'internal' ones.

However, when applying the views of the 'externalist' Hessen concerning the preconception of the immanent laws of the development of scientific cognition, we find, perhaps surprisingly, that Hessen shares just the same 'internalism' as his later sarcastic critic, A. Rupert Hall. *The old externalism–internalism debate was, in some of its decisive points, within the limits of the 'standard view':* only motivability, problems and models were allowed to have come from outside.

The social-constructivist social-historical approaches appeared in the 20th century in *waves.* After their relative flourishing such approaches almost *vanished* during the thirties and forties. There is no place here even to sketch (and what is more, much more research should be done to know more exactly) how and why these sorts of approaches were pressed back at that time. This change in the attitude on the meta-level has its more basic correlate in the *changing place of science as social symbol.* (The causes seem to be rather different in Western countries from those in the Soviet Union. Because of the shortage of space here I can express only some ideas concerning the Western milieu.) In Western societies science was felt to be increasingly threatened by various social processes (fascism, the appropriation of science by the big monopolies, and, according to liberals, even by the idea and reality of planning science in the Soviet Union). Liberals took the thesis of the autonomy of mind and that of science ever more emphatically as an important part of the defence of human freedom. Science was thought to be and was really threatened by various social pro-

cesses; and the aim to resist this process reinforced in an ideologically emphasized manner a special sort of questioning like this: What is that unobjectionable *rational method* and *norm system* of science that makes it so outstanding, and what *social conditions* inside and outside science can be favourable as securing a non-distorted development of science? Society and not science was questioned, scientific cognition was a priori, assumed as having an outstanding objectivity, rationality, defining its immanent laws of development, mind as last *explanans*, etc., and its especially deep ideological commitment forbade even the tentative questioning of the attitude itself. The changed social milieu worked, as I guess, as a sort of preventive apparatus. The history and philosophy of science had to develop within the rationalistic-autonomistic framework of conceptualization.[13] There followed the age of Mertonian norms, of the *Logik der Forschung*, aimed at ensuring the scientific character of cognition, Koyré's conceptual-analytic approach, that of Hall's history of ballistics and of the 'Society for Freedom in Science' setting and delimiting the fundamental outlines of how to conceive of scientific cognition and its history.

3. FULLY DEVELOPED KNOWLEDGE-SOCIOLOGICAL METHODOLOGY – WHITHER NOW?

It is already a commonplace to refer to certain roots of the knowledge-sociological perspective of scientific cognition as being *inside the rationalistic approach*. To put it very briefly, the argumentation emphasizes the *shift* within rationality researches from the methodology researches (as were the verificationist and the falsificationist ones) to that of rationality itself, the justification of methodologies themselves; that is, to the argumentation that there can be rational methodologies defined by their problem-solving capacity, for example. It has been becoming more and more obvious that the 'rationality-problem' is not solvable without taking

into account the possibilities of *different epistemic goals* and, as a consequence, different rational methods.[14] The illusion can then emerge that with this step the annoying problem for rationalists of older type, of the necessary step of *decision* when developing theories will be accounted for in a manner satisfactory for rationalist requirements, as it leads the decision problem back to the choice of cognitive goals and so it leaves the autonomy requirement, so important for rationalists, intact.

David Bloor replies elegantly when demonstrating the need for a *(micro)sociological* account of chosen rationality, happily meeting up with the standpoint of Garry Gutting.[15] One bridge between the camps is already built up when, from one side, the *necessity* of social explanation (of some kind) has been acknowledged by some sort of rationalists (I am not sure whether Gutting is willing to acknowledge the necessity of some special sorts of social explanation, too) and, conversely, some sort of knowledge sociologists found it natural to accept that the social construction of scientific knowledge proceeds precisely through rationality.[16] (Now it is of secondary importance that the knowledge sociologists who take part in building up this communication bridge insist on the claim of the demonstrability of all these reasoning processes as being from one end to the other something *in concreto* socially constructed practice of rule following.) Two groups of extremes have already been surpassed. First, those rationalists who cannot accept that social goals and decisions can be translated into cognitive ones, despite the growing set of thoroughly demonstrated empirical examples. Secondly, those knowledge sociologists who, committed to a special reductionistic approach, try to show that reasoning processes are nearly arbitrarily formable even in similar social situations, reducing reasoning processes to pure epiphenomena of special individual social situations. There is a tendency, represented by some sociologists, to look at the cognitive situation as if they accepted the pretty saying of E. Topitsch that *'cuius regio eius paradigma'*. (There is much use of the idea of the 'judgemental-relativist' un-

derstanding of the cognitive situation in some knowledge-sociologist approaches.)

I want to make two further remarks, one concerning a widespread interpretation of the *micro*sociological essence of the new knowledge-sociological approach, and one on the relativist-instrumentalist *epistemological* interpretation given as an ideology to most knowledge-sociological researches. Last, returning to the comprehensive viewpoint of the study, I shall shortly argue for the *'weak programme'*, as it seems to me that it intends to take back most consistently that reificationistic apriorism which, I think, is the most strong hindrance in our mind to understanding science as an *integrative part of the critical practice of the self-creation of free mankind.*

I think the new knowledge-sociological approach is proud of its *micro*sociological perspective. It is true, case studies demonstrated the alternative structure of scientific cognition even in its smallest units and the manifold factors that affect them. It has also been demonstrated in many case studies that the role and meaning of more general cognitive elements can be understood only historically when their functioning will be explored as well as their seemingly 'minute' social aspects. (For example, not puritanism itself but its *moderate* form should be referred to if we want to give a knowledge-sociologist explanation of the puritanism thesis.) That is, the fact that the 'tinkering principle' has been at work is somehow demonstrated. Nevertheless, the success of this principle does not prove that the knowledge sociologist should be a sort of vegetarian and should abstain from looking for the process of development of 'sociological universals'. (The problem is difficult, of course, and I do not think that timeless 'universals' should be carelessly taken over into empirical researches from sociological theories, but a research without responsiveness for generalizations to find tendencies in the history of scientific cognition rests on the level of sociological craftsmanship. You can't see the forest for the trees...)

There are a lot of reasons for quarrelling with knowledge sociologists of different shades, but the big stumbling-block is surely the commonly shared idea of the thoroughly *relativist-instrumentalist* epistemological interpretation of the work to be done by the knowledge sociologist. There are different sorts of strategies for the defence of this attitude, e.g. developing a theoretical interpretation of scientific cognitive practice based on the Wittgensteinian understanding of action, then something like the method of 'place your finger into the wound' from the Scriptures, itself in a generalized fashion and a more individual one. For example, to mention just one at random, the reader can find in Daryl Chubin and Sal Restivo the following argumentation that speaks for itself. We have to replace such terms as 'objective statement' and 'truth' with the term 'insight', as no insight can ever be final or absolute, no system for arriving at insights can ever be universally valid and unchanging in its foundations, and there is always a broader context or higher level for establishing an insight than that of any given system of inquiry. That these characteristics do not necessitate the change of our terminology and let drop our truth problem has been demonstrated in detail by Mary Hesse.[17]

But the more serious problem is not the epistemological *interpretation* of what the knowledge sociologists do. It could be neglected as ideological self-defence developed to the extreme, needed for securing the early development, but, as I assume, and I think a lot of knowledge sociologists would readily accept it, that there are serious *methodological* differences between a knowledge-sociological approach committed to a consequently relativistic-instrumentalistic understanding and others. For example, some writers committed to the instrumentalist approach seem to come close to a conventionalistic understanding of language, which brings with itself the same question-begging situation as the acceptance of the radical Duhem–Quine thesis. I think Mary Hesse argued convincingly for a minimum acceptance of the realist interpretation of scientific knowledge. It brings us nearer to the prob-

lem of the *artefact* character of the objects to be explored by science and knowledge. It is a widespread belief among knowledge sociologists that it is this artefact character that requires the acknowledgement of a throughly instrumentalist understanding of the cognitive situation. It is true that we can meaningfully speak of truth as correspondence only when we take into account that our knowledge corresponds to Nature through our artefacts. This is the problem that leads (epistemological) constructivists (the followers of Lorenzen) to their foundational-intention researches and to the assumption that science is nothing but knowledge of technology. Marxists should try to understand the situation in a more complicated way, the possibility of interpreting truth in a double way is given within their system of knowledge. First, as a correspondence relation between knowledge and that 'artefact' which is *worked out to be in congruence* with this knowledge by building up 'walls'. Mechanics is valid knowledge of mechanical artefacts. This statement is not intended either to bring back essentialistic ontologies or to cover the fact of the many-to-many relation between facts and theoretical facts. Nevertheless, I think it allows us to say more of truth than Hesse's learning machine does. In this way we can then speak about the problem of the truth of our knowledge in the meaning of corresponding to Nature.

Returning back to the criticism of the thoroughly instrumentalist interpretations of a fully developed knowledge-sociologist methodology, I think that insisting on the essential feature of scientific cognition that it looks for truth (truth claims being formulated within some cognitive goals, nevertheless truth understood in the meaning of correspondence, too, not as a reducible part of it) helps the sociologist to be sensitive to those constraints which work in the social process, and hence in which 'a part of the *raison d'être* of the laboratory is to serve the carrier of the scientist when he/she wants to persuade the readers of his/her papers that the statements involved in them should be accepted as facts'.

Bloor and most of the knowledge sociologists want to explore the social structure of scientific knowledge taking as their model up-to-date sociology in order to give a *descriptive* account of the issue. There is a reificating moment left in this perspective, which became the focal point of criticism by Daryl Chubin and Sal Restivo.[18] They claim that there is a role of meta-inquirer as a *social critic and activist.* I think one sort of development got its final point with this 'weak programme'. There are no more a priori fixed commitments as starting-points that make our understanding of science in some moment scientistically committed. The self-reflection of science began with the narrowest possible perspective laid down by positivists, who declared science to be the natural objective behaviour, and continued as the long-lasting effort to look for the self-grounding of science in the form of rationalistic approaches, the achievement of which has not been what was intended, but the demonstration of the systematic defects of the rationalistic approach to science. This is a very important achievement, as defects can be systematically recognizable only in comparison to structure and elements. The rationalist approach is now handing over the field to sociological understanding as the description of the social nature of scientific cognition. But social knowledge is for social action, and Chubin and Restivo formulated the programme of a social knowledge manifestly committed to social action. This critical approach led them to their overall questions: In what kind of world would what kind of epistemic activity lead an epistemic community to conclude that it was (according to its own definition) improving its model of the world? *And what are the implications for those inside and outside the epistemic community* (in terms of quality of life, the distribution of power, and so on)?[19] They want to realize their social-critical and activist approach in the form of ethical and value analysis and criticism,

respectively. Whether this perspective will or will not be reductionistic is a problem which is stated here without the opportunity to explore it.

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NOTES

[1] Various aspects of this turn are exemplified by Forman's well-known study on Weimar culture, by Ravetz's book on the social institution of science, by the programme of the Starnberg group and by B. Young's reinterpretation of natural science.

[2] Cf. T. Veblen, *The Place of Science in Modern Civilisation and Other Essays*, Russell and Russell, New York 1961. The articles used here for analysis are mainly 'The Place of Science in Modern Civilisation', and 'The Evolution of the Modern Scientific Point of View'.

[3] Veblen should not be misunderstood. He conclusively denied the view which claimed that modern science came into being for industry, by means of achieving some direct pragmatic goals of the latter. He supposed that the accordance between industry and science was guaranteed by a connection of 'higher level' than that. "While even the scientist's curiosity is as idle as that of the pueblo myth-maker, ... the canons of validity under whose guidance he works are those imposed by modern technology, through habituation to its requirements; and therefore his results are available for the technological purpose... Hence the easy copartnership between the two." (Veblen, *Op. cit.*, p. 17.) This justification of technological usefulness in principle appears e.g. in the light of the 'finalization' researches of the Starnberg group as too abstract and too general. Moreover, according to Veblen, 'pragmatic interest' can only be a hindrance in the process of theoretical research. We should think of the difference between the cases of preparadigmatic, paradigmatic and postparadigmatic stages of research.

[4] Cf. M. Scheler, *Die Wissensformen und die Gesellschaft*, Francke, Bern–München 1980 (1926[1]).

[5] *Op. cit.*, p. 101.

[6] Cf. *Science at the Cross-roads*, Frank Cass Comp., 1971 (1931[1]).

[7] K. A. Wittfogel, *Die Wissenschaft der bürgerlichen Gesellschaft*, Malik, Wien 1922.

[8] G. Lukács, *Geschichte und Klassenbewußtsein, Werke*, Vol. II, Luchterhand, Neuwied 1968, pp. 609–612.

[9] There is a widespread opinion that Lukács in his *Class Consciousness* excluded Nature from the philosophical reflection when he acknowledged the validity of the 'formal-calculative' approach to Nature and only looked for an appropriate form of cognition for social processes. There is some evidence for this interpretation in the book and even in Lukács's own reflections on it, nevertheless I think it is more correct to regard his conception as one in which he looked for transcending this narrowed down perspective to explore Nature, the contradictory character of which he exemplified in the problem of

the Kantian *Ding an sich*. And it seems to be quite likely that he supposed to get the social-constructivist forming effects from a new type of society.

[10] Alcan, Paris 1934.

[11] *Die gesellschaftlichen Grundlagen der mechanistischen Philosophie und die Manufaktur*, Alcan, Paris 1935 (Z. für Soc. Forsch. IV.).

[12] K. D. Knorr-Cetina and M. Mulkay (eds), *Science Observed*, Sage, London 1983, p. 116.

[13] We have no space here to speak about the decline of scientific research in Marxism under Stalinism till about the early sixties.

[14] Márta Fehér argued (in different articles; cf. e.g. the present volume) for a lesson to be learned from the problem shift to the meta-level and hence to a broader context of rationality. She found a consequence to be drawn in the working out of a sociology of cognitive goals. It is not a big step to interpret some decisions for (i.e. in favour of) certain cognitive goals instead of other ones as 'interiorizations' of social goals outside the scientific community. Wider groups are yet to be explored so as to become part of the 'epistemic community', taking part in the formation of the cognitive attitude. It can be supposed that this 'interiorization' will be made, at least partly, unconsciously. On the other hand, there are a lot of changes in the cognitive attitude within science, which means a challenge for the 'society outside'. (I am using this term here acknowledging Bloor's description of 'insulation' in his Wittgenstein book.)

I find the term 'epistemic community' as it was introduced by Knorr-Cetina very useful, and I think it can be used to explicate not only microsociological situations. Think, e.g., of the social types and forms of rationality researches.

[15] Cf. J. R. Brown (ed.), *Scientific Rationality: The Sociological Turn*, Reidel, Dordrecht 1984. (My lecture is completed by this reflection on the book which was not yet available at the time of the Conference.)

[16] It becomes more and more obvious that reasoning itself is a social product in the non-trivial meaning that its emergence and working are social-constructively produced and reproduced.

[17] Cf. the article of D. Chubin and S. Restivo in K. D. Knorr-Cetina and M. Mulkay (eds), *Op. cit.*

[18] *Op. cit.*, p. 61.

[19] *Op. cit.*

LUDMILLA A. MARKOVA

ON RECENT PROBLEMS OF THE SOCIOLOGY OF SCIENCE IN THE CONTEXT OF K. MARX'S IDEAS

When the social character of science and its history comes into question, the direction of discussion is usually determined by our understanding of science itself. Indeed, what is science? Science can be understood as: (1) a system of ideas and their development; (2) a social institution; (3) a cognitive system.

Let us confine ourselves to these three approaches, which now seem to be the most widespread in the literature.

(1) When science is considered as a complex of ideas, it is supposed to exist in society, where there are many spheres of activity other than scientific, and the question concerns the influence of other social institutions on science. The development of scientific ideas in one sense or another is influenced by *external* social factors (production, religion, politics and so on). According to different estimations of the influence of external social factors on the development of scientific ideas, two methodological trends have formed in the historiography of science: the internalist and the externalist approaches. Representatives of the first trend do not study social factors, while those of the second one mainly investigate the influence of society upon science. All discussions between adherents of these trends resulted in a somewhat paradoxical conclusion: they proved to be in no disagreement over the influence of social factors on the development of scientific ideas. Representatives of both trends held the conclusion that external social factors do not influence the content of scientific ideas. That is why externalists, while studying the role of these factors in the history of science, disengage themselves from the logic of the development of scientific ideas, and internalists analyzing the internal regularities of the development of scientific knowledge do

Imre Hronszky, Márta Fehér and Balázs Dajka (eds), Scientific Knowledge Socialized, 123–137.
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not care for the social context of this development. So a kind of division of labour took place among historians.[1]

Such a division of labour became possible due to interpreting the functioning of science in society as in one way or another stimulated by social needs. Nevertheless, not all the aspects of internalists' and externalists' investigations could be included in the framework of such a view of science. In the works of some externalists (for instance, J. Needham, E. Zilsel, R. Merton, bearing in mind the latter's study of English science) definite stereotypes of activity in different spheres of social life – in science, production, politics, the arts, and in religion – were revealed. These stereotypes proved to be inherent in definite epochs of history or in some geographical regions, with their own distinctive features, uniqueness, and peculiarity.

Analogous tendencies can be seen in internalist works. One should note first of all the concept of the style of thinking of a definite epoch (A. Koyré contributed much to its elaboration). Scientific thinking was placed in the general context of culture, and it was shown to have characteristics which change from one historical epoch to another. The connection of scientific thinking with philosophy is emphasized by this approach (as opposed to positivism).

In studies on the history of science in which this history is understood as a social process, an analysis of the functioning of science in society reveals the role of final scientific results as utilized by society. It is not important how they have emerged in science as a result of the creative activity of a scientist relying on the available scientific knowledge, in accordance with the laws of its development. What is important is to study and to understand the external stimuli to scientific creativity, notwithstanding the fact that these stimuli can only realize the possibilities of development already available and cannot create new ones. This kind of social analysis of science and its history does not touch upon the process of the making of new scientific knowledge as unique and peculiar.

At the same time, some historical works take on the task of analyzing the characteristic uniqueness of the different (including scientific) forms of activity in different historical epochs, through the study of styles of thinking (for instance, the style of thinking emerging after the scientific revolution in the 17th century, or the basic principles of scientific thinking in antiquity, in the Middle Ages, or in the modern times).

Science is subjected to historical investigation in two forms: first, science as a totality of final results no matter who obtained them, when, and on what principles, within what form of social relations, social intercourse among scientists, businessmen, politicians and so on; secondly, science as a definite form of activity, possessing its own characteristic features in each historical period and oriented to the making of new knowledge. Every historical situation is unique, and the results of activity in this situation are also unique; they are of interest as bearing the 'stamp of the creator', be it conceived as a historical epoch, a community of scientists or as a single scientist.

Thus, we deal here with two types of cooperation of people: cooperation through the exchange of final results (the output of scientific activity, on the one hand, and the outcome of social development, expressed in the form of social needs, on the other), and the cooperative activity of people in the process of producing new, unique scientific results. The separation of scientific knowledge from the activity directed at its creation proved in many cases to be ineffective in solving the fundamental problems of the history of science. Scientific knowledge itself is sometimes understood as an active form of intercourse.

(2) Another concept of science, when science is considered as a social institution embedded in society, presupposes the sociological study of the characteristic features of that institution, which possesses its own hierarchy, ethical norms of behaviour, stimuli and motives of activity, and system of encouragement. As a rule, sociologists (first of all R. Merton and his school) study the social

institution of science apart from the content of scientific ideas; they do not pretend to analyze scientific knowledge. The social institution of science functions as an intermediate link between the development of scientific knowledge and society, as a means of transmitting social needs to scientists – and static scientific knowledge to society. Scientific knowledge is here a kind of 'black box', ready for use.

These sociological investigations also lead to some ambiguity in our understanding of science: the same problem of two types of activity is put forward, although in another way and in connection with the elaboration of the concept of scientific community. Relations among members of a scientific community (taken in the Kuhnian sense) depend on their adherence to scientific ideas, and on their ability to defend those ideas in discussions with scientific opponents. The development of scientific ideas and the emergence of new ones in the process of scientific revolutions is connected with the activity of the scientific community. The same can be said of such concepts in contemporary sociology of science as 'problem group' or 'invisible college'. Science can be understood by sociologists not only as a social institution, dealing with final scientific results, but as an activity organized for the production of new knowledge.

Again, we come across the same problem concerning the possibility of uniting the structure of scientific knowledge with the activity directed at its invention, and concerning the opposition of this form of activity to another one, when the exchange of the final results of the activity takes place.[2]

(3) Finally, cognitive sociology of science reconsiders the same relations. In this case, it is the cognitive possibilities of scientific thinking that are in question. Different scientific methodologies and different ways of cognition are formed in different historical epochs depending on the peculiarities of the social structure and of culture. But the application of different methods implies different results, the content of scientific knowledge changes from one

epoch to another. There is nothing stable or constant in it; it loses its objective character and the significance of truth becomes doubtful. Scientific knowledge in its structure and in its content merges with creative activity, and this activity in turn is determined by society. In the end, the conclusion is that scientific knowledge is formed by society in the sense that it does not depend for its content on the world of nature.[3]

While an earlier period was dominated by the idea of separating the structure and content of scientific knowledge from the act of discovering, nowadays, on the contrary, scientific knowledge is seen to merge completely with that activity, and thus with the world of human relations. It is supposed that schemes of scientific theorizing are directly taken and transferred from society to science. It is not taken into consideration that the inner sociality of science itself integrates and pushes forms of thinking of a definite epoch to their utmost logical idealizations and, subsequently, norms of thinking are spread in society out of science and philosophy. Different methodologies, different types of thinking in different epochs realize different possibilities of the reality which always remains the subject of scientific study.

As stated above, the centre of gravity in the analysis of science by historians and sociologists shifts to the study of human relations directly connected with the contentual aspect of scientific knowledge. A scientific theory or its logical structure is considered in the perspective of its possible competition with other opinions, its development into a new theory, defended by other representatives of a scientific community. In this case, conclusions of a subjective and relativistic character result from a definite shift in the analysis of sociality. In the middle of the 20th century, the study of the history of science as a social phenomenon focusses on the relations of external causal influence, that is, human relations, which are determined by how the results of scientific activity function in society. Most investigators, as stated above, have come to the conclusion that this kind of social relations cannot directly in-

fluence the content of scientific ideas. However, the further false conclusion has also been drawn that the content of scientific knowledge is completely outside any sociality – that it is not social.

In the past one or two decades, the situation has changed essentially. External causal determination has receded into the background, and human relations, which are assumed to dominate the production of new knowledge, have come in the focus of attention. In this situation investigators face some quite new and real difficulties. Indeed, is it possible to call these relations social? Is it justified to reject the traditional idea of sociality, and what will the consequences of such repudiation be? Can we change the concept of sociality radically? Is it necessary to contrast these types of relations between people as different, or are they of the same nature?

However, many representatives of contemporary microsociology are disposed to mechanically bring together different types of human relations and to attribute to all of them the function of forming the contentual aspect of scientific knowledge. Both the relations which exist in science as a social institution and the ('less social') relations between scientists in the process of the creative activity and the activity directed at the justification of scientific knowledge are presented in their works side by side. It seems to me that this way of investigation is not productive. In order to tackle difficulties which nowadays face philosophers of science, it is necessary, on the contrary, to realize that sociality in science is a definite problem, that there are different types of human activity and they are not equivalent. It is no mere chance that in historiography, sociology, and philosophy of science either one type of relationship is pushed into the foreground, or the other, and depending on this, there are different interpretations of scientific ideas from the point of view of the possibility of understanding them as a social phenomenon. Two or three decades ago most investigators of science sought to isolate the content of scientific knowledge from social influences. Nowadays, on the contrary, it

is maintained that knowledge is completely formed or determined by society, and it does not possess any independence of its own.

The question arises whether it is possible to explain by some social circumstance the sharp fluctuations in the points of view of sociologists and historians of science concerning the character and degree of influence of society on science, or rather on scientific knowledge. What real forms of science in society provide grounds for the emergence of contrary opinions?

I shall try to answer this question on the basis of Marx's ideas on science.[4] First let us remember, as has been discussed above, that science is presented in works on the history, philosophy and sociology of science in two forms: science as a totality of final results, functioning in society, and science as a production of new knowledge through creativity.

According to Marx, science in modern society, that is, in the society of machine production, seems to exist in two perspectives. On the one hand, there is science as universal labour for the production of new knowledge in the context of culture. This aspect of science involves a conception which associates culture with a corresponding type of human relation. Here the question arises as to what extent and in what sense these relations can be qualified as social, that is to say, how we can correlate them with social relations in the framework, for instance, of civilization.

Secondly, we have the perspective of science as functioning in society within the division of labour, as interacting with other forms of activity through its results. In this case, an exchange of activities is realized through the exchange of the results of activities. Marx concentrated his attention in general on the second case. He mainly studied capitalist economies, where cooperative or combined (not creative) labour is dominant. Accordingly, Marx was first of all interested in the politico-economical laws of science as functioning in society. I would like to emphasize that these laws are of great importance for the understanding of the social role of science in modern society.

According to Marx, the division of labour as a historical phenomenon only becomes truly such when a division of material and mental labour appears. The emergence of capitalism gave rise to special relationships between science and material production, which differ essentially from those in previous social and economic formations. First of all, Marx speaks about the separation of science from material production.[5]

In machine production, science as a spiritual potential of production is separated from direct material labour. In a society with division of labour, science exists as a particular subdivision side by side with other forms of activity in the sphere of material production, in the sphere of religion, politics and so on. But science as a particular sphere of cooperative labour, presupposing a strict division among fields of activity, possesses its own essential specificity: science is a *particular* subdivision of society which at the same time presupposes the *universal* character of creative scientific activity as a necessary condition for the functioning of scientific results in society. Science cannot be utilized successfully in material production unless it possesses its own existence outside the framework of the division of labour in the context of culture, as universal labour. As universal labour, it interiorizes all other forms of activity.

The character of human cooperation within the social framework of cooperative labour is fundamentally different from that within the social framework of universal labour. The correlation of these two types of sociality defines the place and role of science in the society of modern times.

In the system of the division of labour there exists a definite relationship between science, on the one hand, and production, culture in general, political institutions and so on, on the other. In this case the other spheres of social activity appear as an influencing cause, an external social factor, or as an object of influence, when science stimulates material production.

Marx includes science as a *creative* activity in the context of culture, whereas the social determination of science is realized through channels fundamentally different from the system of civilization or the system of the division of labour. In *Capital* he writes: "A distinction should be made between universal labour and co-operative labour. Both kinds play their role in the process of production, both flow one into the other, but both are also differentiated. Universal labour is all scientific labour, all discovery and all invention. This labour depends partly on the co-operation of the living, and partly on the utilisation of the labours of those who have gone before. Co-operative labour, on the other hand, is the direct co-operation of individuals."[6]

Marx opposed universal labour as invention in science and art to cooperative labour, characteristic of material production. He paid little attention to this side of the matter; nevertheless the opposition of universal labour as creative to cooperative or complex labour in material production is fixed by Marx clearly.

The inclusion of science in the system of cooperative labour presupposes the complex interaction of a scientist through the product of his activity (theories, formulae, laws) with many representatives of different spheres of activity (workers, capitalists, managers and so on).

As a part of the system of cooperative labour, science is connected with the final material product through the mediation of a machinery as a materialized result of scientific investigation. In order to function successfully in the system of machine production, scientific results must be expressed in such a form that they can be used by a non-specialist, by a layman. They must be a kind of 'black box', and a machine is such a 'black box'. A worker is not obliged to know the scientific principles underlying the machine design.

A complex final product is made as a result of many kinds of activity, including scientific. Everyone fulfills his part of the work, scientists as well. A final complex product unites all the different

forms of activity as a result of their *adding* up. Cumulative products of material production come about as a sum of the results of the working operations of different people, operations of the same kind, resulting in the same things. (It is in this sense and only in this sense that scientific labour favours the production of surplus value and turns into constant capital.)

The way of integration of different kinds of activity in the context of culture, that is, in the context of universal labour as creative labour is quite different. In this case, all kinds of activity have to be united in a single mind with the end of creating something new, previously known to nobody. I would like to emphasize three moments which are necessary: *all* kinds of activity have to be *integrated* in *one* mind. A scientist can only be successful in his creative work if he keeps up with the standard scientific achievements of his time; he has to learn these achievements in their universal form without any mediators, without any 'black boxes'. In this case an individual scientist's labour directly, without any intermediaries, interacts with universal labour. For the purpose of this kind of activity, scientific results have to be formulated for specialists, for whom they appear not in their completed, final form, but as possibilities for a new theory or a new scientific achievement. For a scientist, a result is an open system, oriented to the stimulation of somebody else's thought.

Thus, in the society of machine production, science becomes absolutely necessary for industry. It is rooted in material production through its results. In material production, labour no longer depends on the muscular efforts of people, but on their creative efficiency in understanding the laws of nature. However, even though scientific creativity is necessary for production, it is separated from it. At the time of machine industry, science and production cannot exist without one another and, merging one into the other, they completely lose their specific features. While the needs of science demand one kind of formulation of scientific results which stimulates thinking, the inclusion of scientific results

into cooperative labour demands another formulation, suited for the utilization of these results by a layman. The social direction of universal labour and that of cooperative labour are different. If science is oriented exclusively to practice, it will stop developing and, consequently, will not be able to serve practice either.

Science is separated from production, according to Marx, in two senses. First, as universal labour from cooperative labour, from the aspect of the character of the acivity itself: universal labour taken as creative activity which results in new scientific achievements, and cooperative labour resulting in complex products of material production. Secondly, within cooperative labour itself, the universal *results* of the creative capacity (productive force) of a man are differentiated from the partial labour of a single worker.

In the system of the technical division of labour a worker cannot have a claim on the product of cooperative labour as his property. This product acquires a *universal* character as the result of the functioning of a system of machines, in which the mechanical laws of nature are materialized. Scientific laws underlie the combination of the labours of single workers fulfilling some partial functions, as well as of technological processes and machinery constructions. This imparts a universal character to the complex product, first, as the sum of everybody's labour (both workers and scientists taking part in production with the *results* of their creative activity) and, secondly, as a product belonging to everybody through the market, namely, through exchange.

Scientific activity as such functions in society through its results. In a society with a division of labour science can only influence the development of industry if it is included in production; if it is submitted to the laws of the development of production itself and ceases to be an *external* cause. The same can be said about the determination of scientific development by production. The needs of production have to be interiorized by science. Instead of being *external* factors, they have to turn into the inner problems of science itself; only then is it possible to speak about

the influence of production on the development of scientific ideas in the true sense of the word.

Marx elaborated the first aspect of the question, namely, the introduction of science into machine industry. Scientific labour, rooted in the system of machinery, is universal scientific labour in its results, a technological application of science, contrary to the direct, partial labour of a worker. In this case science is universal labour in opposition to partial labour, as an indirect expression or result of this partial labour. Science is a totality of final results, rooted in production and forming its 'soul'. At the same time, the 'soul' of scientific results themselves as *scientific* remains outside production, in the sphere of universal labour as creative labour, realized in the context of culture and not of production. Under the conditions of machine production, sociality, formative in the context of culture, recedes to the background, and seems to be of little significance.

Marx did not limit himself to the analysis of the then existing forms of production; he also outlined the tendencies of the future development of the relationship between science and production. He emphasizes in this connection that the application of mechanical and chemical laws makes a machine able to do work previously done by workers. Operations performed by workers turn more and more into mechanical operations, and at a definite stage they are replaced by a mechanism. The workman's own labour-power depreciates. Labour is not so involved in the process of production. Rather, it becomes the labour of a man who deals with the process of production from the position of controller or supervisor. A workman places a natural process, which he has transformed into an industrial process, between him and inorganic nature, which is to be mastered. In the future, instead of being the main agent of the process of production, a workman will take his stand next to it.

In the future society of automatized production, the automatized system of machinery will turn into a kind of experimental set-

ting for the correction and confirmation of theoretical conclusions. Human creative capacity will become a direct productive force. Universal labour in the context of culture will dominate over material production as well as over the sociality of cooperative labour. A confrontation of these two types of sociality in the sphere of both material and spiritual production is the most significant moment of the modern scientific-technological revolution. Cooperation of people according to the principles of cooperative labour continues to dominate in contemporary society, but it is subjected to the powerful impact of universal labour.

Science becomes a direct productive force not so much in the sense of materializing its results in a system of machines, as in the development of the creative capacity of man as a universal productive force of society. The sense of that activity consists, first of all, not in the suggestion of new theories, but in making a new subject of scientific activity with new resources of creativity.

I have been speaking about cooperative labour, applying the term to the process of material production when analyzing the place of science in it. Indeed, this type of cooperation among people is the most characteristic feature of material production, and now it dominates in society in general. This kind of sociality is present inside science as well, when, for instance, we deal with a scientific theory or scientific formula without thinking about its origin or about its transformation into a new one, when we use it as the end result of somebody's activity. This kind of activity is necessary for the existence of science, but it does not make science; science becomes science only through creative activity. At the same time, creative activity is present in material production; but material production can manage without this kind of activity, it can do without any novelty, reproducing the same things in the same way and remaining mere material production.

A social picture of the existence of science in society which stands out against Marx's ideas of science helps us to understand the different standpoints of sociologists and historians of science

as social phenomena. The fact that science is separated from production and that science has to possess some independence in order to serve this production is the basis for theories of science in which scientific knowledge is considered as existing absolutely independently of society. At the same time, investigators realize another fact of the life of science: contemporary machine production is impossible without science and its results converted to machinery. They see science as functioning in society in accordance with the laws of production, as submitted to the ends of production, and this consideration serves as a social basis for a contrary opinion about a complete dependence of scientific ideas on society.

In the first case, scientific ideas are detached from society and from the activity directed at their creation; in the second case, they completely merge with social relations, with human activity. I think that Marx's ideas on science help us to question the traditional understanding of sociality in science, and to show the timeliness of the problem of a different type of sociality in science – sociality in the context of civilization.

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NOTES

[1] Among Soviet appreciations of internalism and externalism see e.g. S. R. Mikulinsky, 'The Methodological Problems of the History of Science', *Scientia* 110 (1975), 83–97.
[2] About the social characteristics of science see e.g. in Soviet literature V. G. Kelle, 'Science as a Component of the Social System', in *Methodological Problems of the Investigations in the History of Science*, Moscow 1982, pp. 11–28 (in Russian); L. A. Markova, 'Social Characteristics of Science and Case Studies', in *Abstracts of the 7th International Congress of Logic, Methodology and Philosophy of Science*, Vol. 6, Salzburg 1983, pp. 136–144; S. R. Mikulinsky, 'Some Remarks on the Analysis of the Concepts of the Development of Science', in *In Search of the Theory of Development of Science*, Moscow

1982, pp. 3–11 (in Russian); Y. A. Zinevich and V. G. Fedotova, 'The Role of Socio-cultural Factors in the Investigation of Science', *Voprosy Filosofii* 9 (1982), 67–77 (in Russian).

[3] The problem of creative thinking in science has led to lively discussion in the Soviet Union. See V. S. Bibler, *Thinking as Creativity*, Moscow 1975 (in Russian); B. S. Griaznof, *Logic, Rationality, Creativity*, Moscow 1982 (in Russian); S. R. Mikulinsky and M. G. Yaroshevsky (eds), *Scientific Discovery and Its Perception*, Moscow 1971 (in Russian).

[4] With regard to Marx's concept of science as considered in the works of Soviet authors see e.g. Bibler, *Op. cit.*; B. M. Kedrov and A. P. Ogurzov, *The Marxist Concept of the History of Science in the 19th Century: The Emergence and Essence of the Conception of K. Marx and F. Engels*, Moscow 1978 (in Russian); S. R. Mikulinsky and R. Richta (eds), *Socialism and Science*, Moscow 1981 (in Russian).

[5] K. Marx, *Capital*, Vol. I, Moscow 1978, pp. 340–341.

[6] K. Marx, *Capital*, Vol. III, *op. cit.*, p. 104.

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DETERMINANTS OF SCIENCE EVOLUTION IN THE 19TH AND 20TH CENTURIES

The main contention of the present paper can be summarized in the essential points of a working programme for our research group. These items play a heuristic role in preparing case studies of the determinants of science evolution in the 19th and 20th centuries.[1] Our programme is based on the following ideas:

(1) Science evolution is a complex process with several determinants in each type of scientific thinking. We investigate the new quality of the type of science evolution in the scientific-technical revolution.[2] Among the essential determinants we must distinguish conditions and laws.[3]

(2) We use the statistical evolutionary law concept (SELC), developed and founded upon case studies by our research group.[4] A statistical law (or system of laws) is a general, necessary and essential interrelation between the individual elements of a system. This law determines the possible behaviour of the system, which is necessarily realized as a tendency (dynamical aspect). The individual elements, on the other hand, realize possibilities in a conditionally accidental manner according to a probability distribution (stochastic distribution), and their transitions between various states take place with certain transition probabilities (probabilistic aspect). Probability is here understood in a general sense as a regularity in the realization of possibilities. A system of laws is statistical, if aspects of the laws are recognized as dynamical, stochastic and probabilistic, and if their internal unity is established.

(3) In this context, the conditions of existence of the first order are necessary and sufficient factors for the realization of a tendency representing a systematic possibility. The conditions of existence of the second order are the necessary and sufficient factors

Imre Hronszky, Márta Fehér and Balázs Dajka (eds), Scientific Knowledge Socialized, 139–155.

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for the incidental realization of elementary possibilities. If the system is divided into subsystems, then these orders correspond to the preconditions of this subdivision. Concomitant conditions are those factors that lead to random modifications of the results. Because each law exists in concrete conditions, the conditions are always a unity of general conditions of existence and of concrete conditions for action. This applies to conditions of each order. We call the manifestation of a law via conditions of action the working mechanism of laws. This mechanism can only be investigated if the internal structure of the law is understood as a methodological requirement that the formulation of the law should contain the conditions of different order, as well as the possibilities that are realized in a random manner, and their probability distribution.

(4) The history and philosophy of science shows a hierarchy of determinants, which is related to scientific revolutions as essential processes of science evolution, to several sciences, to disciplines and the work of scientists.

(5) The impact of society on scientific cognition and that of science on the development of society have their essential link in the personality of the scientist.[5] In some cases this link is forgotten because science evolution is regarded as a process with anonymous persons. The scientist's personality is not taken into account.[6] In other publications his personality is overestimated.[7] The best way to get the real proportions of these links in science evolution is through case studies.[8]

I. WHAT ABOUT DETERMINANTS?

Determinants of the development of science comprise the entirety of all influencing magnitudes. This requires us to take into account the hierarchy of determinants, so as to be able to distinguish essential from unessential factors and to understand the relationship be-

tween main tendencies and side effects. The point is the *conditions*, hence essential causes for effects, and we are concerned with the *laws* of the development of science. In this respect, it is necessary to take into account three *groups of factors* in their interaction. These are the development of society, the state of development of the science and the role of the scientist's personality. Social development does not lie simply in external factors acting on scientific development but constitutes a substantial part of science. Science can only be done if there is production, if elementary vital needs are satisfied. Whether scientists are aware of this or not, in terms of ideas it is society with its needs which gives commissions to science. Society utilizes economic, political, and ideological means for the stimulation of science and is the consumer of scientific results.

Intrascientific development is determined by the state of development of theory, depending on the spheres it explains, how far it is couched in mathematical or quantitative terms, what its foundations are in terms of world view, epistemology and methodology, and another determinant is the results of experiments, depending on the availability of instruments.

The scientist's personality, whether an idependent individual or a member of a collective, must be assumed to have a knowledge of scientific development, to be capable of creative work and to have skills in scientific method as well as the means for scientific work. Hence, the personality is an essential link in the dialectical relationship between science and society. The personality utilizes a multitude of economic, political and ideological means, so as to satisfy by means of its capacities and skills the short-term or long-term requirements which are expressed in the development of science. But the other factors are also not independent from each other. The utilization of political and ideological means cannot be separated from the role of political and philosophical theories. Also, experimental activity with its instruments depends on the development of industry.

However, as a result of these dialectical contradictions, it may happen that, even though society announces its requirements and provides appropriate means, the results attained through the development of science are not accepted by society as a consumer. There may certainly be a variety of reasons for this fact. The results may not be in conformity with the demands, or the utilization of the results calls for new investments. Perhaps their importance is not seen. Another case is when there are the scientific prerequisites to solve a problem, but the creative idea for the concrete solution is lacking. A task of our theory must be to determine more accurately these contradictions in the process of production and reproduction in the development of science.

II. CONDITIONS FOR THE DEVELOPMENT OF SCIENCE

Laws have their conditions. The conditions of the present-day development of science determine the working mechanism of the laws. They have certain new features. These conditions are: (1) the scientification of society and the socialization of science; (2) the politization and ideologization of the sciences; (3) the growing complexity of scientific tasks and the institutionalization of appropriate management bodies; (4) the technologization of the sciences and methodologies; (5) the transition from thinking about structures and processes to scientific development in terms of thinking.

(1) The social interest in the evaluation and in the utilization of scientific insights is growing. The development of science is grasped as an essential part of social development. This leads to interesting questions for the management of the development of science. Which lines of research are to be promoted above all? Which priorities exist in the allocation of funds? What is the relationship between cognition-oriented and practice-oriented research? Is the role of chance taken into account in the planning of

directions? Have the parameters been correctly determined in the planning of objects? The answer to these questions demands concrete analyses and prognoses. Science is challenged, on the one hand, as an immediate productive force in the development of technologies and, on the other hand, in its capacity of a cultural force as well as a humane and social force which effectively fashions social structures and promotes humanity through scientific and technical advance. This leads to a question which is certainly not easy to answer: Is whatever is scientifically possible and technically feasible, also socially necessary, economically feasible and can it be justified in humane terms? Discussions of this question which characterize the interrelation between science and society led to considerations about external and internal factors in the development of science.[9] One-sided approaches in the attitudes of internalism or externalism were criticized. Our point is to emphasize the dialectical relationships between these two groups of factors. They are established above all by the role of personality in the development of science.[10]

(2) Science is subjected to economic, political and ideological discussion as to the effectiveness and humanity of socio-economic systems. The national prestige of states with different social systems depends significantly on their scientific and technological achievements. The existence of weapons for mass destruction calls for scientific measures for the safeguard of peace. Attitudes in terms of world view are varying between pessimism and illusionary optimism. The point is to achieve real optimism which is aware of the difficulties and problems and of the contribution of science to social progress and peace.

(3) Complex scientific problems call for interdisciplinary, multidisciplinary and interinstitutional cooperation. We are concerned with complex research programmes with uniform objectives which can be attained in a disciplinary manner. To elaborate our research projects is a complicated scientific task. A high level of disciplinary work is prerequisite to the solution of complex problems.

Disciplinary mediocrity in the elaboration of complex problems is increased according to an exponential function to give rise to interdisciplinary mediocrity. In this context, collective work does not abolish individuality. Interdisciplinary research projects often fail because eminent scientists are not quite ready for, and capable of, cooperation, as they hardly think beyond their discipline and they regard the complex problem only in the specific aspects of their own work. The solution of complex tasks by teams of scientists passes through various stages. These range from mutual information about research results in the complex theme, which can often be characterized as the popularization of science, up to the determination of stages for formulating the research programme. Connected with this condition are approaches towards the methodology of interdisciplinary cooperation.

(4) Owing to the emerging comprehensive material and technological basis of research, recently there developed a new relationship between empirical experience and theory, between special and global theories. Let us point to nuclear technology and to the material bases for research in the field of high-energy physics, to space research, as well as to the development of biotechnologies. The application of information technologies also presupposes a comprehensive material and technological basis. The system of methods in science has become so mature that the question about a methodology is raised time and again.[11]

(5) Since the 19th century there has been imposing itself a heuristic utilization of the principle of development in the formation of evolutionary theories in all sciences, which was concomitant with the development of the philosophical theory of development. Now this process is followed by a scientific justification of the principle of development by such theories as, for instance, the theory of the self-organization of systems.[12]

III. FUNDAMENTAL LAWS

For the present development of science, four laws are fundamentally important: (1) the law of the alternation of evolutionary and revolutionary phases in the development of science; (2) the law of unequal experimental and theoretical development in the scientific disciplines; (3) the law of the integration of knowledge and growing complexity of scientific tasks with practical relevance; (4) the law of the dialectization of science, that is, the unity of its presentation in mathematical terms and of its humanization.

The law of the alternation of evolutionary and revolutionary phases in the development of science comprises the mechanism of development as an accumulation of experimental and theoretical knowledge in various disciplines. The accumulation of knowledge produces scientific revolutions whose inception represents a new quality in the development of science and demands the formation of fundamental ideas as well as their practical utilization. This has implications for the determinants of scientific development in evolutionary and revolutionary stages. Scientific revolutions are the turning-points of scientific development. Among them, there are the achievements of Copernicus, Galileo, Kepler and Newton in the foundation of physics, Darwin's theory of evolution, the uncovering of the fundamental laws of social development by Marx, the theory of relativity and quantum theory, and the discovery of the genetic code. Here we get an insight into the mechanisms of creative behaviour. Scientific revolutions are the essential processes of scientific advance.[13] They are not simply identical with the formation of new theories. Scientific revolutions comprise the change of the fundamental theoretical structure of a scientific discipline by changing the initial axioms and by determining anew the fundamental laws. This again in its turn has an influence on other sciences. Therefore, the internal theoretical development of a scientific discipline does not have the character of a scientific revolution. The initial axioms and fundamental laws of other sciences

must also be affected. Scientific revolutions bear essential implications on social practice through new principal solutions and through changing the picture of the world (change of paradigms).[14]

In investigating the system of the determinants of scientific development, the point is to incorporate the stages of specific theoretical development and the hierarchy of theories as a unity of global and local theories, of natural, technological and social theories, of positions in terms of world view, of epistemology and methodology, into the fundamental processes of theoretical development, namely into the scientific revolutions.

In the structure of determinants it is therefore necessary to bear in mind a rank order among scientific revolutions, disciplinary and technological developments, between the emergence of new theories and theoretical advance in partial fields. Apart from this rank order, there exists for each aspect a relationship of essential and unessential factors. Besides, at the different stages a specification of the determinants is going on, as well as their substantiation by changing main determinants.

Let us now begin with the structure of the determinants of scientific revolutions. In this context, there exist three lines of determination. The first comprises the theoretical deficit which acts as a demand for a scientific explanation. Such a theoretical deficit can prevail for a long time. It is stated through philosophical explanations, formulated as a world enigma and is effective as an orientation of scientific research if there exist chances for a theoretical and practical solution of the problem. A second main line concerns social demands. They can stimulate scientific research, but they cannot directly influence solutions of problems. They work as objective interests, as formulated needs and as direct control tasks with means and personnel. The conditions of scientific development as characterized above make it clear that the significance of social demands continues to grow. However, they always leave scope for the individual creativity of the scientists. This capacity can be used to solve problems if scientific problem solving can be

advanced by training, by an appropriate atmosphere and by suitable technical means, as well as by personal reserves. The third determining line corresponds to the scientist's personality. The personality of a scientist includes theoretical approaches as a background for new theories.

From the point of view of scientific revolutions, the personal determinants such as curiosity, prestige-mongering and so on are of subordinate significance, the existence of such personalities being decisive.

Now, if we take theoretical development which is incorporated into and subordinated to the scientific revolutions, then we come to a subdivision of these main determinants. Let us begin with the sequence: idea, hypothesis, research programme, and theory. In finding useful ideas, the decisive factors are the researcher's flair, his knowledge of the problems and his capability to wonder. Then the idea must be elaborated into a hypothesis, which presupposes the ability to formulate a scientifically justified assumption about an unknown fact, an assumption which can be verified experimentally and theoretically. Thus a research programme is formulated which can be worked off in experimental and theoretical terms, and then social support for this research project becomes the appropriate main determinant. To get the programme accepted, it is necessary to find sponsors who are in a position to refute other arguments. In this context, it is necessary to take into account the impeding role of monopolized opinions. If work on the research programme has led to a theory, then there occurs the problem of social evaluation and utilization.

Main determinants for the general acceptance of a theory are its incorporation into training, its social utilization and the social prestige of its advocates. The process of the imposition of the theory is at the same time connected with the internal fashioning of the theory, its maturity. As evaluation criteria for a theory, H. Hertz indicates accuracy, truth and suitability. Accuracy requires taking into account logical rules to eliminate logical inconsisten-

cies. Truth can be established by empirical investigation. Suitability is connected to simplicity and beauty. The fashioning of a theory obviously depends on other main determinants than those required for its formation or its imposition. Decisive for social evaluation and utilization are then social values and ideas of values, whereas practical utility is in the foreground. The law about the alternation of evolutionary and revolutionary phases comprises the phase of the emergence of a theory with its conditions, the scientific revolution, the maturity of the theory, social evaluation and social utilization.

The law of the unequal experimental and theoretical development of scientific disciplines expresses that there are specific contributions by each scientific discipline at a certain developmental stage to the development of science which are dependent on the concrete state of experimental and theoretical research in that discipline at a certain stage. If classical physics had determined the ideal of exact science until the 19th century then linguistic and literary sciences passed to be an expression of humanism. Psychology showed the interpenetration of insights from natural sciences and from social sciences above all in the 20th century. The theory of relativity specified the view of time and space, and molecular biology grasped the relationship between information and evolution. The statistical manner of thinking was essentially influenced by quantum theory. The unity of theoretical and experimental activity is realized in the social cognitive process in which the integration of specialized activities takes place. But this is not sufficient. Experimental activity always includes theoretical activity, and theoretical activity must take into account the results of experiments and observations. It is stressed that nothing is better for practice than a good theory. However, it is possible to state that a bad theory will perish, but a successful experiment will remain. Time and again, new attempts at interpretation are based on the same experiment. Just let us think for instance of the decisive role of the Michelson experiment in the discussion about the ether, or

let us remember the photo-electric effect, the Compton effect, or the diffraction of electron beams on crystals in the development of quantum theory. A good theory is confirmed by experimental results and can be utilized in practice.

Experimental activity and observation provide a multitude of data. Nowadays, electronic data-processing helps store them. However, their evaluation presupposes that they be essential data, that is, such as determine the character of the investigated process. In order to find out what is essential, theoretical notions are necessary, which explain hypothetically possible experimental results and observations. Such hypotheses must be obtained from the previous theories and experiments by creative achievements. The misgivings of some scientists towards the theoretical aspect of scientific development by searching for data are therefore understandable. Certainly only a determined quantity of data reveals the essence in the appearance, or allows some indications about regularities. But the theoretical analysis of data, that is, their explanation by a theory is an important step of scientific development which can lead to further purposeful experiments.

The unequal development of disciplines manifested itself in the history of the sciences. In the formation of physics, experiments and the utilization of mathematical apparatus had a decisive role. Chemistry and biology developed as sciences through the experimental foundation of their theories and the better utilization of mathematical means. In social sciences, mathematical modelling is also used today. The processes of theory formation are not to be identified with their presentation in mathematical terms. The theory passes through various stages of development, from the qualitative expounding explanation of processes via their quantitative measurement up to their formalization. A theory's use depends on the cognition of laws contained in it which also implies the conditions for the existence of the law involved. Sometimes such laws can be grasped as qualitative rules, other times they can already be presented in mathematical equations. Certainly it would be incor-

rect to underestimate non-formalized qualitative analysis in the development of science. Mathematics as a science requires the formalization of the conceivable structures. Hence, the heuristic function of mathematics is fulfilled in a twofold sense. One is through suggestions for mathematical thinking in a wider sense, and the other is the mathematical presentation of theoretical notions, which can lead to uninterpreted expressions, as Dirac's electron theory showed with its theoretical prediction of the positron, or can uncover contradictions in theory, and then their meaning is to be elucidated. The point in the presentation in mathematical terms is not the possibility of mastering major sets of data and of quantitative relationships, that is, the point is not the function of presentation allocated to mathematics; the salient point is rather the heuristic function of formalization for the construction of theories and for theoretically guided experimental activity.

Experimental activity has various forms. Essentially, an experiment is an objective analysis of reality which is organized by man. The essential aspects analyzed in experiments are theoretically synthesized. Natural sciences have developed the experimantal method extensively. This method is also utilized for the foundations of psychology in terms of natural sciences. Tests are used to investigate behaviour patterns and capabilities of man.[15]

If tests are considered as a way of experimental verification of the existence or the absence of already recognized socially relevant patterns of behaviour in the individual, then they are to be integrated into the relationships between experiment and theory which are reviewed here. Tests take place as experiments in model cases which certainly represent essential conditions but are not identical with reality. In this respect, the suitability of the person for the test must be taken into account as much as the suitability of the model cases for simulation. One-sided aspects of certain tests must be compensated by a system of tests. Above all, tests have certain theoretical prerequisites which are again included into their evaluation. Among these are the previous insights into the theory

of the personality and the regularities of social behaviour, and from these insights the socially relevant patterns of behaviour are derived. As with every method, in the case of tests, too, the criticism of methods is essential, if their results and their implementation are connected with decisions about people.

The social sciences have certain specific aspects with regard to the experimental method. Certainly, in philosophical and social scientific thinking a decisive role is played today by thought experiments and by the investigation of model cases which enrich the exchange of views in the fashioning of social relationships through theoretical generalization. In the case of experiments in social subfields it is necessary to take into account the phases of the preparation of the experiment, that is, the elaboration of the question to be examined, and the mental preparation of the experiment by determining the constant and variable factors. Moreover, in the social field two other factors play a role: the complex of variable factors and the duration of the experiment. Thus it is not possible to keep any amount of parameters constant, since we are concerned with human beings, with the expenditure of living labour and with personal development. In order to arrive at analyzed essential moments by means of the experiment despite this fact, thorough theoretical work is necessary which determines the regular relationships between the variable parameters. This again has repercussions on the duration of experiments designed to obtain genuine experimental results as a basis for theoretical considerations and for practical changes.

The law of the integration of knowledge and the growing complexity of scientific tasks with a practical relevance expresses the interpenetration of sciences in the solution of significant social tasks. Thus, there emerges a new connection between disciplinary fundamental knowledge and the solution of complex problems. The point is to obtain a more rational utilization of energy, to open up new sources of energy and effectively utilize traditional energy carriers; we are concerned with the utilization and substi-

tution of raw materials, as well as with the setting up of rational cycles of material; with the fashioning of the environment along humane lines; with healthy nutrition; with improving national health; and with the development of personality.

Hence, it is necessary to ensure the acquisition of disciplinary fundamental knowledge, in order to solve complex problems. The point is to acquire complex knowledge, which can be utilized. In this context, the disciplines cannot be subdivided into complexes. This is shown by experience with complex research institutions and with aims of instruction based upon it. There are no optimizers of life processes, but there are indeed biologists, behavioural scientists, geo-scientists, economists and others who deal with such problems. The dialectical structure of the materially unified world demands disciplinary research into the regularities of lower and higher-developed forms of motion, of the complexes of forms of motion, and into the structure and behaviour of systems. Despite the existence of integrative forms of science, there emerge complex solutions only through a scientifically justified combination of partial insights which are elaborated in a disciplinary manner. Physics affects the problem range of energy and of raw materials. Chemistry has importance for production technologies, but also for nutrition, health and fashion. One could establish many points of reference. But above all, it is necessary to understand man as a social being in his individual manifestation as a unity of natural and social, of emotional and rational, of conscious, subconscious and unconscious factors.

There are two contrary tendencies opposed to the main tendency of this law. They affect not only the necessary development of special knowledge, but also the possible degeneration of research by collectors of data without any generalization or by speculation without an empirical basis. The integration of special knowledge is promoted by the solution of complex tasks. Natural, technological, cultural and social factors must be understood in their interaction and priorities. Any recognized physical or chemical law has a

social importance if it is utilized in the construction of machines and so on. The previously relatively isolated laws explored must, for this purpose, be viewed in connection with other laws in order to produce a new machine, a new medicine, a new household appliance, and so on. The multivalence of physical and chemical insights is sublated here, so as to incorporate them into certain relationships of integration which are necessary for man. Thus, there emerges a socially necessary integration of specialized knowledge. In this context, the point in question is not only the scientific organization of collectives to solve significant scientific problems, which can be of a disciplinary and of an interdisciplinary nature, but we are also concerned with each individual scientist who must be prepared for the implications arising from the law about the tendencies of the integrative interrelation of specialized knowledge. This has importance for education and further education, for the continuous specialization of the scientist, for his own attitude towards specialization and integration, and for the social evaluation of special insights or of cognitive results due to integration. Therefore, already in the education of the specialist, integrative tendencies must become visible. The scientist must be fully aware of the possible relationships between adjacent sciences, of analogies, of philosophical generalization and so on. Only if he himself sees tendencies towards integration, will he be not only ready for, but also capable of, performing interdisciplinary work.

The law of the dialectization of sciences as the unity of their humanization and mathematical presentation comprises the interrelation between two opposite tendencies.[16] The mathematical presentation of sciences with its tendencies towards axiomatization in the formation of theories, with its application of mathematical models and with the utilization of electronic data processing is an essential direction in the development of science. This mathematical presentation serves to develop technologies. Technologies are means for the domination of man over his natural, social and psychical environment and over himself. They are the

transformation of insights into regularities of the natural, social and consciousness-related processes and into their working mechanisms, into rules and methods for the domination of man. By means of technologies, man achieves a great gain in liberty. Hence, the development of technologies is part of the humanization of the sciences. Alternative solutions to scientific and technical advance limit the creative abilities of man to problems already solved, retard the development of science and technology and subtract from the revolutionary power of the development of the productive forces. The humanization of the sciences manifests itself above all in the complexity of tasks which demand interdisciplinary work. With disciplinary insights, by using all mathematical possibilities, the complexes of man's needs are to be scientifically penetrated. At the same time, man is opposed to the presentation of sciences in mathematical terms. Thus, there are reservations against the application of data processing in medicine because of the fear that the patient is no longer considered as a man of flesh and blood. One could also put forth more reservations against the 'computerization' of man. They would be justified if the permeation of sciences by mathematics were not concomitant with the humanization of sciences. Wherever man is left out, antihumane behaviour must be combatted. But that criticism does not apply to the development of science. Criticism must be directed against antihuman social conditions which subordinate human interests to the mere craving for profit of governing circles. This critique must be directed against the behaviour of those scientists who are not aware of their human responsibility and who act against it. Science can orient social action but cannot replace it. In order to obtain instructions for action which are justified in scientific terms, it is necessary to exhaust all possibilities of the development of science.

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GÜNTHER KRÖBER

PRODUCTIVE NEEDS AS DRIVING FORCES OF THE DEVELOPMENT OF SCIENCE

At least since the 2nd International Congress on the History of Science in London in 1931, the problem of the driving forces of the development of science has run like a red thread through discussions on the theory of science. Yet, the interest in the solution of this problem is not only a theoretical one. Under the present conditions, a theoretical solution of the problem is also expected to offer the possibility of deriving recommendations for science policy, for the management, planning and organization of science, for the effective stimulation of the development of science and thus of scientific-technological progress and its direction into channels which will serve social progress and the securing of the future of mankind.

I proceed on the assumption that neither the internalist nor the externalist conceptions of the development of science have any chance at all of being regarded as serious solutions to the problem. The internalist approach reduces science to a mere system of knowledge, to a summary of scientific statements, theories, hypotheses, etc. From this point of view, the development of science can only be regarded as the dynamics of theories, as an advancing intellectual problem-solving process. On the other hand, the externalist approach is equally inadequate because it presumes the one-sided, direct, and linear dependence of cognitive structures and processes as well as of the contents of scientific theories on social structures and processes, and on social needs. From this angle the development of science has to be viewed as a mere passive result of social conditions and developments. Thus the controversy of internalism and externalism is in fact a pseudo-problem.[1] I think this is generally accepted today.

Imre Hronszky, Márta Fehér and Balázs Dajka (eds), Scientific Knowledge Socialized, 157–164.
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But it also would take us no further if we accepted that internal *as well as* external scientific factors have played a role in the historical development of science and are still playing a role today. The mere 'as well as' does not offer a constructive solution as long as the relation of both groups of factors and their mutual interaction remain unclear. Only when this relation is defined will it be worth subjecting the respective solutions to the theoretical and/or historical test. For the evolutionistic way of explaining the development of science, which is preferred by many proponents of non-Marxist conceptions from Popper to Toulmin today, this test proves that, though the influence of social conditions on the development of science cannot be denied, it is more or less a secondary factor. The main driving force here is the correlation between the variations of scientific ideas and their selective stabilization. In this model, social factors, social conditions and needs play the role of selective factors which appear only when several variations of ideas or mature scientific theories are already contained in the pool of ideas or theories. It is obvious that this general model of explanation, in whatever forms it might be contained in the works of Popper, Kuhn, Toulmin, the Starnberg group or Feyerabend, cannot satisfactorily cover the phenomenon of the development of science and in particular, it is not able to explain the historical process of the ascending of scientific knowledge to objective truth of successively higher order as well as the growing practical efficiency of science in the course of its historical development.[2]

I consider as more promising the attempt to describe science from the beginning as a social phenomenon, as a socio-historical process, as a specific kind of social activity. This approach allows one to determine the social character of science from three different aspects, viz. from (1) the social preconditions of science, (2) the social character of the process itself, and (3) the social results of scientific work. If we examine these three aspects, it is possible, in my opinion, to draw up a complex theory of the development of science with special regard to the relation between science and

society. In the following, I shall, however, present only a few considerations in connection with the first aspect.

The interpretation of science as a specific kind of social activity prevents the understanding of science as a mere intellectual matter which is not bound to any material and social preconditions. The idea that knowledge creates knowledge just as money creates interest, or that science reproduces like people (with regard to the 'sum' of knowledge taken over from the previous generations) was a dominating theory from the 19th century through the first half of the 20th century; even Friedrich Engels held that view in the forties of the past century.[3] The fact that knowledge is growing exponentially seemed to confirm this idea. But it does not take into consideration that new knowledge does not merely develop out of the knowledge which is already 'there', but that it is born in the course of scientific work – a kind of work which requires qualified scientists as well as appropriate material and technological conditions, financing, etc. This seems trivial and it certainly is. As long as scientific work requires no more than the means to secure the social welfare of a relatively small number of scientists, and relatively simple material and technological means (e.g. to make scientific experiments), scientific work is in fact almost independent of external conditions. Under such conditions science seems to develop merely according to its internal logic; the individual curiosity of the scientist seems to be the decisive driving force of the development of science. This impression was finally destroyed around the middle of the present century, when the number of persons working in science and the amount of financial and other expenditure for the running of science in some of the developed countries assumed proportions which, when extrapolated into the future, meant that problems of an unprecedented nature and level would be encountered in the coming decades. One of the tasks of science is to help solve the problem of the resources of society, but today's science is more and more facing the problem of its own limited resources. But the amount of resources that a society

can use for the maintenance and development of its scientific and technological potential depends decisively on the level of productivity of that society.

Thus we can set up the following thesis: The *productivity level of a society*, particularly the level of the development of its material productive forces, determines the possibility of the development of science. This thesis is valid for the present situation as well as for the historical development of science. Science has historically developed in the process of the division of labour as a specific social activity, as a specialized social working process, to the extent that the level of the development of the social productive forces allowed a labour productivity which was high enough to produce more than was required for the maintenance of work itself. The higher the level of the productive forces, the better the material preconditions and the more favourable the social conditions for the running and the development of science.

The dependence of science on the level of development of society has yet another aspect. The level of the *social productive forces determines* not only the possibility, but also the *necessity of science*. Provided that there is a certain scientific-technological level of production, it will always bear in itself the need for new scientific-technological solutions and their technological application. The higher the scientific-technological level of production becomes, the more distinctly this need shows itself. The dialectic of this relation is expressed in the fact that the scientific-technological level of production, which itself is a result of scientific-technological progress, becomes the precondition for the further progress of science and technology in that it offers the possibility of really utilizing the new results of scientific-technological research in production. The higher the scientific-technological level of production already is, the sooner production will be in a position to absorb new scientific-technological achievements and put them into practice. The need of production for new scientific-technological solutions and the ability to really absorb and utilize them

belongs to the most effective stimuli of scientific and technological research.

However at this point we should avoid a misunderstanding. Though the possibility and the necessity of science are determined by the productivity level of society, this does not mean that the contents of concrete scientific discoveries or of new scientific theories or ideas can be derived from the level and need of the development of the respective production. This would be to approach the complex process of the development of science, which is completely characterized by non-linearities, from a vulgarizing point of view and explain it as a process of linear causality. Of course, this approach cannot be taken seriously. The importance of pointing out the dependence of scientific progress on the level and the needs of social production lies in the fact that this level and these needs determine the main orientations of research and thus the main directions of scientific-technological progress. What kind of individual solution, what kind of discovery, what kind of new scientific ideas and theories will or might be the result of this oriented research is mainly determined by the respective levels of development of science and technology.

Hence, there are two main determinants which are essential for the process of the development of science: (1) the level of development of the social productive forces and the needs of society for production and extended reproduction of its living and developing conditions, and (2) the level of development of science itself and the immanent needs of its further development. These determinants are not independent of each other. Therefore the actual problem is to find out their relationship, that is, the nature of the mutual correlation. In my paper at the Bucharest Congress in 1981, I made a suggestion on this problem using the term *'productive need'*.[4]

This term is based on the assumption that a need of a society, be it an economic, technological, social or cultural one, is not necessarily a *real* driving force of the development of science. A need

alone does not move anything, not even in science. If it is to cause research activities on a larger scale, a need has to be so intense that it either occurs as the need of the whole society, or that it is at least borne by the interests of those who own the means of production and thus dispose of the means of research. In order to become a real driving force of the development of science, the need has to be socially or at least economically significant. It will become productive for the advance of science only when its satisfaction corresponds with the basic interests of those social forces who are prepared and are in a position to promote and initiate the necessary research activities. The intensity of these interests influences the search for possibilities to satisfy them. This search is based on scientific research.

In this context I speak about *productive needs* as the *real driving forces* of the development of science. Not any social need proves to be productive in this sense. In order to become productive as a real driving force of scientific-technological progress, a social need has to fulfill the following conditions:

(1) It has to develop with certain preconditions: within the framework of the given level of production, and with reference to the given standard of science and technology.

(2) It has to be able to effectively regulate the conditions of scientific work; that is, it has to be so intense as to cause the improvement of those scientific subjects and fields which are essential for its satisfaction.

(3) A social need usually exists in a common form, as the need for food, housing, clothing, communication, security, etc. But in order to become productive for scientific-technological progress, it has to be concrete. It must be the case that (a) it can be formulated as a concrete technological or scientific problem; (b) the prevailing standard of science allows work on it; (c) the prevailing scientific-technological standard of production permits practical use of the new achievements in production.

Finally, I would like to make three remarks on the above-mentioned characteristics of productive needs.

First, the term 'productive need' comprises a whole complex of correlations between social and internal scientific factors and situations which might change in the process. Therefore, it goes without saying that, depending on the prevailing historical situation, the level of social development, the level of cognition in the respective scientific branch, the cognitive interests and the personality structure of the scientist, etc., the real correlation of these conditions may take on quite a different shape. The theoretical consequence following from this is that in the history of science the driving forces and criteria for the development of science are not fixed, but they are constantly changing, undergoing development, and that this evolution of the criteria and driving forces of evolution depends on the level of development of science and society.

Secondly, it is not significant for the productivity of a need whether this need is an individual one, a need of a group of people or of an entire society, a theoretical or a practical need, etc. Even the individual need of a scientist to go more deeply into the research of a certain theory, to explore new horizons of cognition, which is not dictated by any practical or production interest, may be a productive need. In this case, the precondition that the need be determined by the given level of development of science and be formulated and explored as a concrete scientific problem can be taken for granted, whereas it is not guaranteed that society attaches enough importance to it and grants social support for the respective research activities. The difficulty of identifying the theoretical needs of science as productive social needs is the basic reason for the legitimation pressure exerted on planned large-scale research activities in all existing societies today. In the basic research which belongs to the vanguard of scientific research today and aims at finding new scientific solutions in advance in order to be prepared to meet the future needs of social development, this

legitimation pressure must not be too strong. Otherwise it will prevent research activities for the simple reason that there is no immediate social need on which that research is based. Therefore, it is generally recommended that science policy treat the needs and problems of basic research as productive social needs.

Thirdly, from the above we can draw the general conclusion for science policy that, under present-day conditions, priorities in scientific research should be established, with due consideration for the following three points: the basic needs of social development as far as they can be formulated as concrete scientific problems, and as the given level of development of science allows work on them; the conditions for the reproduction of the national economy and thus the amount of social resources available for the development of science and technology; and the given standard of the development of science itself, including the needs and problems of basic research.

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[2] See G. Kröber, 'Zu Problemen der Entwicklung in wissenschaftstheoretischen Konzeptionen der Gegenwart', *NTM, Schriftenreihe für Geschichte der Naturwissenschaften und Medizin* 21 (1984) 1, 1–15.

[3] See F. Engels, 'Umrisse zu einer Kritik der Nationalökonomie', in *MEW*, Vol. I, Berlin 1968, p. 521.

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WOLFGANG KROHN

SOCIAL CHANGE AND EPISTEMIC THOUGHT

(Reflections on the origins of the experimental method)

I have chosen two quotes to head this lecture, representing the poles between which my interpretation is situated: one is from Engels, the other from Kant:

> Both science and philosophy have completely neglected the influence of man's activities upon his thinking – they recognize only nature on the one side, and thought on the other. But it's precisely *the transformation of nature by man*, not nature alone, which is the most essential and immediate foundation for human thought. To the degree to which man learned to transform nature, man's intelligence has grown.[1]
>
> Therefore, objective validity and necessary, general validity (for everyone) are complementary concepts.[2]

My intention is to do justice to both these quotes, to the pragmaticism of Engels as well as to Kant's emphasis on the general validity of knowledge. My historical point of reference will be an interpretation of the famous dictum of Francis Bacon: "Knowledge is power."[3]

With regard to science I take the dictum to have two distinct meanings: a political one, which concerns the effects of knowledge, and an epistemological one, which concerns the generation of knowledge. I will be interested mainly in the epistemological meaning, which relates to a particular claim: that a 'violent' dealing with nature generates knowledge about nature. In other words: the sentence is intended as a justification of the experimental method. In this it is opposed to medieval natural philosophy, which – basing itself on Aristotle – would have denied the relevance of such a procedure for the acquisition of knowledge about nature. My goal will be to explain how it became possible that experience which is gained as a result of (experimental) activities is

Imre Hronszky, Márta Fehér and Balázs Dajka (eds), Scientific Knowledge Socialized, 165–178.
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accepted as knowledge about nature – accepted both in the cognitive and the evaluative sense.

To answer this question it does not suffice to determine a shift in the convictions of Bacon's contemporaries, or forerunners, as important as they might be. It is also not enough to uncover the social causes of such changes of conviction, although these are also important. One can avoid the genetic fallacy which sociological analysis has often been criticized for only if one is willing to take the social emergence of new epistemological claims of truth seriously.

Admittedly, such claims create a significant problem for a sociological explanation of the development of science. On the one hand they can no longer be held to be 'a priori' in a Kantian sense; on the other hand they cannot be classed together with other cultural convictions, the meaning and obligatory force of which can only be understood relative to the symbolic world of a given society.

In the philosophical section of this paper I will try to outline a conceptual model for the social emergence of these claims, located somewhere between their sociologized trivialization[4] and their aprioristic treatment. The central idea of such a conceptual model was anticipated in Hegel's dialectic. As a principle for the structuring of knowledge, the dialectic is based upon the ability of the intellect *to make one's own actions the object of reflection*. This reflection is the origin of epistemic claims.[5]

REFLECTION AND EPISTEMIC CLAIMS

In analyzing this dialectical process I will draw upon ideas from the genetic epistemology of Jean Piaget.[6]

A somewhat simplified definition of the reflective process in terms of Piaget's theory would be: Reflection is an intellectual activity the object of which is not nature but human actions. Re-

flection is therefore a process that depends upon experience, but the experience is not of objects but of our dealing with objects. Accordingly, the knowledge achieved by reflection is not objective knowledge, but subjective knowledge. 'Subjective knowledge' means, basically, that we come to know the intellectual structures that we implicitly employ when we interact with nature. This experience has an important consequence: it does not only make these intellectual structures transparent, but – by the same token – enlarges them. Expressed in Kantian terms one could say: reflection is analytic because it clarifies operations that we already use; and it is synthetic because precisely through this analysis it extends our intellectual operative space.[7]

The next step in which I follow Piaget is in what he has termed the 'decentering' process. It means the better understanding of how our knowledge in some area is a construct of our intellectual faculties. The decentering process is at once an increase in the *'relativism'* and a *'justification'* of knowledge: *relativism* – because the alleged objectivity appears to depend on subjective construction; *justification* – because the operations leading to experience can be explained.

The third and last step leads us back to the problem of epistemic claims: Epistemic claims proceed from our ability to make the operative connections between the knowing subject and the known object, the object of thought. The more explicity these connections are reflected upon, the better such claims are redeemed. Scientific claims are neither a particular form of social prejudice, nor are they a priori elements of a universal epistemological structure.

It is clear that this model for understanding epistemic claims needs not only detailed elaboration but is also lacking in important aspects. But as I cannot convey here more than a rough idea, I will turn to the historical part of my presentation. Seen as a social and historical process, the transition from action to reflection

is not restricted to individuals but can occur over different periods of time.

The historical development I am going to describe now follows a simple scheme.

(a) I will first briefly describe the increase in the number of crafts in the late Middle Ages. These crafts represent the new activities which were basic for the emergence of new epistemological ideas.

(b) Then I will look at the technical literature of that time and of the Renaissance period. I will give a few examples of early reflection on the new body of knowledge.

(c) Then I will point to the new type of technology-oriented innovative literature in which the methods used to obtain new knowledge are already used deliberately and the idea of research has already begun to play a role. I will refer to different kinds of justification of knowledge and research, depending on the social environment of the author.

(d) Finally I will come to Bacon whom I consider to be the first to attempt to give the new epistemic claims of modern science full expression.

(a) The only detail I want to mention concerning the first point is the strong increase in new ways of dealing with nature, which can be observed in the increase of crafts and guilds in the late Middle Ages. In the 12th century there were approximately 12 guilds and crafts to be found in the towns. Over the next 150 years this increased, for example, to about 150 in Venice, 75 in Florence, and over 50 in Nuremberg.

(b) I will also be brief with the analysis of the technically oriented literature of the time. Early medieval texts were written already in the 9th century. A continuous growth began somewhere between 1250 and 1350.[8]

The texts document a rapid development and dissemination of new techniques and technologies, but they contain no evidence that this knowledge or its generation resulted in any way in social or intellectual problems. The knowledge gained was seen as be-

longing to the lower mechanical arts which were held to have no philosophical value. It was presented in a pragmatic style ("If you want to make..., then take...") with frequent use of the vocative ("Oh brother, do this and that..."). Rare exceptions do show, however, that reflection on this kind of knowledge had already begun: In the prooemium of the Heraclius ms. the author states: "for you ... brother ... I have written down this ... if you follow it, *utendo vera probabis* (you will find it true in your practice)".[9]

(c) A striking change can be seen as the authors of such treatises came into contact with humanism. The close association between art and humanism in the 15th century (which is represented by such names as Filippo Brunelleschi, Mariano Taccola, Leone Batista Alberti, Leonardo da Vinci, and Albrecht Dürer) induced several authors to reflect on the newness and the importance of their knowledge.

Depending on the social background of the author one finds two strategies of justification: If he was a craftsman or superior artisan he had to grapple with the guilds, which were opposed to new knowledge. If the author was a scholar or a humanist with a university education, he had to deal with the representatives of traditional learning and with the widespread contempt for the crafts. Common to both strategies is the playing down of the newness of this new knowledge. The craftsmen often emphasized that the innovations, despite their newness, would be useful; the academics pointed out that the knowledge embodied in the handicrafts could be taken up into the scholarly canon despite its origins. The most widespread argument was: this knowledge only seems new, but it was well known to antiquity. Only rarely did these reflections lead to theoretical considerations.

A few examples illustrate the different strategies of this period. Albrecht Dürer's book *Treatise on Human Proportions* (1528) is a handbook intended to provide the artist with natural standards of beauty. In the dedication of the book we read: "I don't doubt that many will attempt to criticize me for my endeavor to write

and teach despite my being uneducated, of little understanding and not gifted with much skill..."[10] His book should enable artists "to work with that art, in which there is truth".[11] Truth, he explains, is "founded in and demonstrated by geometry".[12]

Much earlier Leone Batista Alberti wrote his famous book on architecture *De re aedificatoria* (1485). It was the most important work on architecture after Vitruv. Alberti followed the opposite strategy to that of Dürer and raised the architect to the level of a well-educated scholar. "He should behave as someone who is occupied with the study of the sciences (in *studiis litterarum*). ... He should equip himself with the knowledge and practice of the best of all the arts, and become an independent expert."[13] Most important among the various disciplines are mathematics and painting.

Georg Agricola, in his *De re metallica* (1556) still argued in a similar way: "Everybody has the opinion, mining is a contingent and dirty job and, in general, a business that does not demand art and science but manual labor. But when my mind wanders through its parts it appears to me to be of quite a different nature. The miner needs to possess not only a great deal of experience but furthermore he has to have knowledge of many arts and sciences – first of all philosophy, for an understanding of the origins, causes and qualities of the objects beneath the earth's surface then medicine, astronomy, arithmetic, geometry and law."[14]

The last quotation is from Vannoccio Biringuccio's *Pirotechnica* (1540). Here the relationship between new knowledge, new methods and new goals is expressed with a high degree of self-confidence. The quote is not from a prominent place of the work, but from the preface of the 5th book: "As you have seen, I have hitherto demonstrated to you the methods for bringing to their proper and pure bodies all metals of any kind of ore that you have found and mined. If these did not now serve human needs all these operations with so much expense and labour would have been profitless and in vain. Therefore I recognize that it is necessary in

speaking to touch upon their operations and to teach you to make use of the metals according to the works needed."[15]

Most of these attempts to justify new knowledge and research are eclectic: they make use of humanist arguments together with scholastic-Christian and utilitarian arguments. It is only at the second half of the 16th and the beginning of the 17th centuries that one finds a conceptually thought-out argument depicting the fundamental differences between the old and the new knowledge, describing the contrast in interests and values that lie behind them and the basic methodological distinction between them.

The three most important authors to oppose the Aristotelian-scholastic type of knowledge as well as the revival of Platonism were Galileo, Descartes and Bacon. Despite significant differences in other respects they shared the awareness that the new integration of practice and knowledge could not be reconciled with traditional conceptions of the sciences. They realized that the success of the new sciences depended not only on the acquisition of new empirical knowledge, but on the creation of a new concept of knowledge and nature. Since it was Bacon who gave more precise and consistent formulations in this conflict than the other authors I will focus on him and analyze his conception of knowledge and truth.

(d) Bacon's expression for power is *'potentia'*, in the sense of 'ability' or 'capacity'. Conceptually he goes back to the Aristotelian concept of *'dinamis'*, which was not, for Aristotle, an active principle, and which bore no relation to power. The reworking of *'potentia'* by Bacon into an active principle is connected with human action: the goal of the new science is a knowledge by which one "will be capable of all manner of works" *(omnis opera potentia)* in contrast with the "felicitous contemplation" *(felicitas contemplativa)* of classical philosophy.[16] For Bacon causes are related to knowledge just as rules are to action. The equivalence between the knowledge of causes and the ability to produce something can be understood in both directions: Not only are our actions more

manageable as a result of our knowledge of the laws of nature, but the laws of nature can be understood better when our point of departure is not the *observation* of nature, but the *vexationes artis*, "nature under constraint and vexed",[17] the changes produced in nature by application of the rules employed in the crafts. The use of the word *'vexatio'* (abuse) is interesting here, since Bacon's designation of craft operations as an abuse of nature reveals that his language is still thoroughly Aristotelian. This makes his turning from Aristotle that much more noticeable, as his claim that a condition for the understanding of nature is our interfering with it is irreconcilable with the Aristotelian concept of knowledge. An important pattern of the new epistemic claim is now established: If the goal of science is to attain knowledge for mastery over things, the means is to experiment. A similar conflict between Aristotelian language and anti-Aristotelian thought can be found in Bacon's idea of *natural law*. He is among the first authors in early modern times to use this term. But he relates it to the traditional 'form'. Form, however, he defines contrary to Aristotle: "From the discovery of forms therefore results truth in speculation and freedom in operation" *(contemplatio vera et operatio libera)*.[18] In his explanation of the concept of law Bacon departs completely from the Aristotelian tradition. According to Bacon, laws have to be investigated with a view to the type of *praeceptum* (doctrine), *directio* (direction), *deductio* (guidance) one needs to produce something.[19] Such laws, Bacon continues, must be reliable, free (or general) and employable, by which Bacon rejects the means traditional natural philosophy suggested for obtaining empirical knowledge. Statements which contain the words 'more' or 'less', or 'usually' violate the principle of reliability; the requirement of generality is violated by the usually arbitrary experimental conditions in alchemy; and the requirement of employability is transgressed against by those laws in which the scholastic *faculties* are named as causes. The new concept of knowledge Bacon developed comprises three aspects:

– The goal of understanding is the knowledge of causes which serve as rules for the production of something.
– This goal of understanding has a consequence that knowledge can not be obtained by observation alone but, in particular, by dealing practically with the external world.
– Progress in knowledge is progress in the freedom of operative activity.

All these ideas were explicitly obtained by Bacon from reflection upon craft activities and by abstracting from them those elements that lead to knowledge. Aristotle and the scholastic tradition are of the contrary opinion on all three points.

Bacon's new epistemology is supplemented by his cognitive theory or, better, by his criticism of the tools of cognition. Bacon is aware of the fact that the new goals and means of knowledge call for a new theory of cognition. Here again he is in direct opposition to Aristotelian phenomenalism and descriptionism. Of central importance for Bacon is that an unaided understanding of nature is impossible. According to him, man, as a biological, social and individual being, is poorly suited to perceive the facts of nature. This being the case the new science presupposes our bringing to light the peculiar functioning of human reason and man's senses, and his socially-induced distortions and individual fallacies, and it is to compensate for them both in terms of method and by the use of instruments. This criticism of our cognitive tools is for Bacon a precondition for overcoming the scientifically fruitless dichotomy between empiricism and rationalism, and of arriving at a reliable combination of an understanding of and operation with nature. Mankind had devoted insufficient attention to the control and improvement of its tools of cognition and had therefore developed either – trusting these tools – empiristic dogmas or – distrusting them – rationalistic ones. Bacon suggests regarding our cognitive capacities as tools and instruments requiring maintenance, supervision and correction. For Bacon the *intellectus humanus* is a "false mirror, which, receiving rays irregularly, distorts and discol-

ours the nature of things by mingling its own nature with it".[20] This criticism is not only directed against Aristotelianism, but also against the Renaissance philosophers, who claimed that there was a correspondence between the microcosm and the macrocosm, between human and cosmic proportions. Bacon cuts the ground away from under such arguments: he suspects that between the reason underlying the world, and that from which man derives his standards for the world there is a basic disharmony. Bacon undermined the *analogia entis*, the assumed continuity of all levels of existence and knowledge, which was central to the world views of the ancient world, the Middle Ages and the Renaissance. What idea or concept of nature was he prepared to substitute?

As is well known, Bacon loved to emphasize the close relationship between his idea of nature and the practice of craftsmen and merchants. More than that, I want to show that in a fairly strict sense he modelled his concept of nature after this practice. I will consider two points: first, the reduction of nature to materials to be used; secondly, the enlargement of nature from the given world to the technologically possible world. With regard to the ends of the craftsman and merchant nature consists, in the final analysis, of materials, which are not to be ordered according to the categories of beauty or of correspondence, but according to 'storage' principles. Similarly for the theoretician, "the empirical investigation of nature should conceive nature as something resembling a storeroom or a warehouse, which is not intended to please one or as a place for one to stay or to live in, but as a place that one enters only occasionally when one needs something for the theoretician's work".[21] It is clear that Bacon's ideology critique didn't protect him from establishing a new ideology. But it is just as clear that in Bacon the transformation of the operative patterns of the craftsmen and the merchants into an epistemological postulate had reached a tangible result.

There is a second and even more important aspect of this new concept of nature. It resembles more the practice of the engineer

than that of the craftsman: nature does not only include what is present to the senses or what reason derives but also that which can be produced by employing our knowledge of the laws of nature – that is, technology. He defines as the goal of knowledge the discovery of "what nature does, or can be brought to do".[22] Although Bacon maintains the distinction between artificial and natural movement (or between nature in itself and nature under constraint), he also maintains that in research distinctions of this kind are questions of practicability and purpose, not of principle.[23] This concept of possible nature connects the operative with the conceptual observation of nature, going beyond the phenomenal limitation of the classical concept of nature. Nature is no longer defined as the 'given' or the 'immediate'; it is much more the domain of the attainable – the laws of nature are the rules of production. Bacon's 'facts of nature' are always facts in the original Latin sense of the word – *facere*, to make – things that can be brought about by acting, not sense impressions. Bringing into being something that may not yet exist but is possible is included in the *concept* of nature. Nature is not the objectively given but the objectively possible. The goal of our understanding of nature is no longer the discovery and ordering of facts (this is only a means to an end) but the construction of facts according to laws that stake out the domain of the possible.

With this conception of the scientific knowledge of nature Bacon transformed the new forms of interaction with nature that had arisen in the Middle Ages into conscious abstract structures. In closing it must be mentioned that Bacon did not stand alone with this concept of nature: a gradual weakening of the Aristotelian conception had already begun with the artist-engineers of the first half of the 15th century. Leonardo da Vinci had postulated that the investigator into the nature of things *(specolatore della cose)* has to investigate the domain between the impossible and the possible.[24] And in the title of his work *The New Science*, Ni-

cola Tartaglia sought the attention of the speculative-mathematical artillerist.[25] Among Bacon's first followers was Descartes. In his *Discourses* there is a definition of natural science which, despite their contrast in other respects, is almost identical with a formulation of Bacon's: nature, Descartes writes, "is the discovery of everything in the world that is or can be".[26]

SUMMARY

We began here with the contention that in a sociological analysis of the development of science one had to depict not only the origin and function of empirical knowledge, but also the epistemic claims and criteria of validity on the basis of which this knowledge is founded and claimed as 'true'. A sociological analysis which neglects this is either functionalistic or relativistic. In a functionalist account validity claims are reduced to a psychic accompaniment of social development, in a relativistic account they become ideological cultural expressions. In both cases science as a particular human activity is lost sight of.

I have attempted to show, however, that epistemic claims are social facts sui generis. They emerge when certain types of social activity are made the object of intellectual reflection.

I went on to show that the philosophy of Bacon can be understood in connection with the social origin of validity claims which are fundamental for modern science. The type of social activity that led to these claims came about through the emergence of a large number of new craft and commercial occupations that began to be superimposed upon the feudal-agrarian way of life of the Middle Ages. These occupations required a new way of dealing with nature and provided therefore a new foundation for human thought – in the sense in which Engels speaks of this in the quote

I have chosen to head this essay. But it is only in the philosophy of Bacon that this thought found expression as the new operational form of science.

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NOTES

[1] F. Engels, *Dialektik der Natur*, *MEW*, Vol. 20, p. 448 (English translation by W. K.).

[2] I. Kant, *Prolegomena zu einer jeglichen Metaphysik*, § 19.

[3] As far as I know the dictum 'knowledge is power' was not used by Bacon in *these* words. Bacon's own words are much more careful: "Human knowledge and human power meet in one." (*Novum Organum*, III, *The Works of Francis Bacon*, Vol. IV, ed. Spedding, Ellis and Heath, p. 47.)

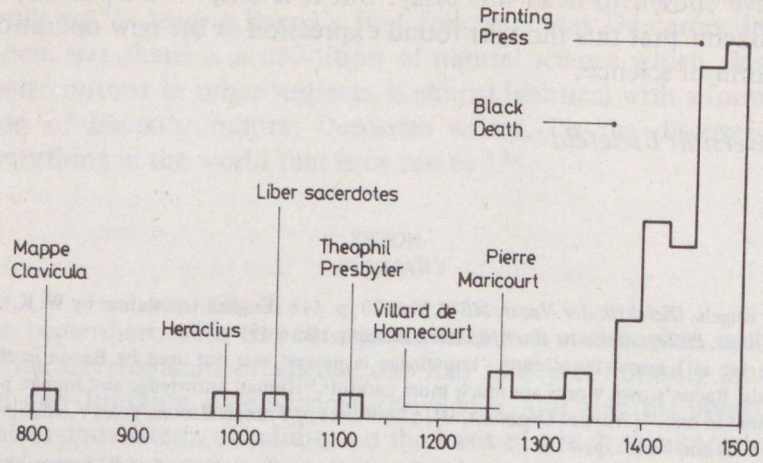
[4] An example of this trivialization is the programmatic statement of B. Barnes and S. Shapin: "to relax completely about natural knowledge and set it alongside technique, art, music, or literature as simply (sic!) a part of our culture." (*Natural Order*, London 1979, p. 9.) Is it easier to understand science in terms of any of the other areas than to understand science? And is culture more simple than its parts?

[5] Hegel's classical study is his *Phänomenologie des Geistes*, 1807.

[6] I have two general reasons for utilizing Piaget instead of Hegel: first, in Piaget intellectual action always relates to concrete action or practice, which Hegel tends to disregard; secondly, Piaget has studied the relationship between practical and intellectual operations empirically. It should be noticed, though, that I do not refer to or rely on specific assumptions of Piaget's developmental psychology.

[7] Unfortunately there is no text by Piaget in which he discusses in a systematic manner the problem of epistemic claims and criteria of validity. The most important texts, upon which my presentation is based, are J. Piaget, *Die Entwicklung des Erkennens*, 3 Vols, Stuttgart 1975; E. Beth and J. Piaget, *Mathematical Epistemology and Psychology*, Dordrecht, 1966; J. Piaget *et al.*, *Recherches sur l'abstraction refléchissante*, 2 Vols, Paris 1977. On the connection between Piaget and Hegel cf. P. Damerow, 'Handlung und Erkenntnis in der genetischen Erkenntnistheorie Piagets und in der Hegelschen Logik' in P. Furth (ed.), *Arbeit und Reflexion. Zur materialistischen Theorie der Dialektik – Perspektiven der Hegelschen 'Logik'*, Köln 1980; T. Kesselring, *Entwicklung und Widerspruch. Ein Vergleich zwischen Piagets genetischer Erkenntnistheorie und Hegels Dialektik*, Frankfurt 1981.

[8] The following diagram includes all technically oriented treatises (mostly in the fields of arts, chemistry, engineering and mining) which are rather independent of each other and not only copies of each other or older texts. Excluded are encyclopedias and pure picture books without technical expertises.



[9] Heraclius, *Braumüller*, ed. Ilg, Wien 1873, p. 27.
[10] A. Dürer, *Schriften und Briefe*, ed. Ullmann, Leipzig 1978, p. 222 (English translation by W. K.).
[11] *Ibid.*, p. 225.
[12] *Ibid.*, p. 229.
[13] L. B. Alberti, *De re aedificatoria*, Vol. I, ed. Orlandis, Milano 1966 (English translation by W. K.).
[14] G. Agricola, *Zwölf Bücher vom Berg- und Hüttenwesen*, ed. Schiffner, 1928 (repr. München 1977), p. 1 (English translation by W. K.).
[15] V. Biringuccio, *Pirotechnica*, Boston 1942, p. 207.
[16] F. Bacon, *Novum Organum*, I, p. 144 = IV, p. 32.
[17] *Ibid.*, p. 140 = IV, p. 29.
[18] *Ibid.*, p. 228 = IV, p. 120.
[19] *Ibid.*, p. 229 = IV, p. 121.
[20] *Ibid.*, p. 163 = IV, p. 54.
[21] F. Bacon, *Parasceve*, IV, p. 255.
[22] F. Bacon, *Novum Organum*, V, p. 236 = IV, p. 126.
[23] F. Bacon, *Parasceve*, IV, p. 253.
[24] Leonardo da Vinci, *Codex Atlanticus* 119 v. a., 337, r. 6.
[25] N. Tartaglia, *Quesiti et inventioni diverse*, Venecia 1546, p. 98.
[26] R. Descartes, *The Philosophical Works*, Vol. I, ed. Haldane and Ross, Cambridge 1967 (1911[1]), pp. 119, 121.

SYBILLE KRÄMER-FRIEDRICH

THE 'UNIVERSAL THINKING MACHINE', OR ON THE GENESIS OF SCHEMATIZED REASONING IN THE 17TH CENTURY

INTRODUCTION

The aim of this paper is to establish the thesis that the 17th-century paradigm of technical action gains meaning not only in the case of the so-called factual truths *(Tatsachenwahrheiten)* of the natural sciences, but also in the case of the truths of reason *(Vernunftwahrheiten)* for mathematics and logic and for the rationalistic concept of reason in general. In support of this statement, I will consider the genesis of a regulative idea: the regulative idea that reasoning can in principle be mechanized. This idea can be expressed in the metaphor of the 'universal thinking machine'.

I would like to identify the way this idea arose through a systematic reconstruction of four distinct but partly overlapping stages in the historical development of the scientific and philosophical discussion of the 17th century. These stages are as follows:

(1) the assumption of representative symbolism;
(2) the possibility of working out calculi for mathematical and logical operations;
(3) the assumption of a *'mathesis universalis'*;
(4) the assumption of a *'scientia generalis'* and an *'ars characteristica'*.

Imre Hronszky, Márta Fehér and Balázs Dajka (eds), Scientific Knowledge Socialized, 179–191.
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1. THE ASSUMPTION OF REPRESENTATIVE SYMBOLISM OR LANGUAGE AS A CODE

1.1. The transition from the 16th to the 17th century was marked by a radical shift in the semantic pattern, that is, in the manner in which signs were conceived to have meaning. Signs were no longer taken to designate their designata by virtue of being similar to them, but rather by virtue of pure conventional correspondence. This transition from symbolism based on similarity to symbolism based on representation was part of a comprehensive change in the form of thought orienting Western culture.[1] In the 15th and 16th centuries, the idea of similarity had a universal function. It was assumed to be an overall model of the connection between objects: what is similar to something is connected to it.[2] It was a semantic pattern: the meaning of a sign was based on the similarity between the sign and its designatum.[3] It was also a form of knowledge: knowledge as the interpretation of signs.[4] And, finally, it was a form of magical praxis: objects may be affected through the manipulation of their signs.[5] These various manifestations of the general function of similarity can only be mentioned here and not proved. What is essential for us now is that the universal idea of similarity became overshadowed by the idea of representation. This is especially true in the case of the semantic pattern. I will now try to explain the transition to the understanding of signs as being only representative, as, for instance, in Leibniz's concept of representation, a view already anticipated by the conventionalistic semantics of John Locke and Thomas Hobbes.[6]

1.2. For Leibniz, the term 'representation'[7] originally had the meaning of a one-to-one correspondence between the elements of two sets.[8] Although there is no figurative similarity between the circle and the hyperbola – this is an example Leibniz used – on the basis of a constant law of correlation, it is possible to represent a point on the circle by a point on the hyperbola.[9] The essential step Leibniz made was to transfer the notion of representation as

one-to-one correspondence as in mathematical functions, to the semantic plane, and there take it to be the relation between a sign and its designatum.[10] Signs represent states of affairs in so far as they isomorphically correspond to them. This representation, however, is based on convention. Leibniz illustrates this in the relation of ciphers to numbers, letters to geometrical lines, words to ideas and words to things.[11] Similarity is not completely banished from this semantic pattern, but its locus changes. It no longer exists between the individual sign and its designatum, but rather takes the form of an analogy between the structures of the signs and the structures of the designata; in this way model construction becomes possible.[12]

1.3. That representative symbolism should provide the foundation for the semantics of signs in general implies that systems of signs or languages attain the properties of a code. This is clear even from the following consideration. As a result of the arbitrary character of the alphabet of signs *(Zeichenalphabet)*, the same state of affairs can be expressed by various systems of signs. In this way the isomorphism between signs and states of affairs is reproduced in the isomorphism between various analogous sign systems. Due to this isomorphic relation, every given system of signs can, in principle, be translated into another. For example, a system of signs having the property of a machine language can be brought into one-to-one correspondence with the states of a machine. Leibniz himself drew this conclusion in his construction of a four-species calculation machine, insofar as he allowed the ciphers of the decimal system to correspond to the placement of the teeth of the gears.[13] Moreover, with the help of the ciphers O and 1, he developed the first binary translation of the decimal system, as is used in computer calculations today.[14]

2. THE POSSIBILITY OF WORKING OUT CALCULI FOR MATHEMATICAL AND LOGICAL OPERATIONS OR THE POSSIBILITY OF MECHANIZING SYMBOL OPERATIONS

2.1. What distinguishes 17th-century European development in mathematics and logic is that at that time it became possible to work out calculi for the operations of mathematics and logic themselves.[15] This development was prepared for by a change in the everyday practice of reckoning in 14th- and 15th-century Europe.[16] The Roman system of ciphers, which required the use of stones placed on a counting-board, was supplanted by the decimal system. This system allows a purely symbolic calculation using number signs written on paper. This arithmetic calculus, which employs the Hindu–Arabic system of ciphers with their algorithmic calculations, came to be seen as the ideal realization of a calculus and provided the model for all attempts to calculate in higher mathematics during the 17th century.

2.2. The renewal of algebraic notation by the French mathematician François Viète is the first stage on the way towards working out a calculus for higher mathematics. Viète introduces letters not only for the unknown, but also for the known numerical coefficients.[17] Thus it becomes possible to formulate rules of algebra with universal validity. Unlike the ciphers, the letters are no longer signs for single numbers, but rather signs for the whole class of numbers satisfying a given equation by substitution.[18] The rules which refer to the transformation of equations can in this way be written down in a formal language. This means that their validity is independent of the numerical values entering into the calculation. Algebra becomes the transformation of series of signs according to rules which have no relation to the meaning of the signs.

2.3. Viète interprets the algebraic symbols as restricted to arithmetical quantities. In Descartes' analytic geometry,[19] however, the interpretation of the algebraic symbols is extended to geometrical quantities. Descartes' fundamental thought consists in representing

geometrical relations by algebraic relations.[20] Renouncing the ancient principle of homogeneity, the fundamental operations of arithmetic can be interpreted as operations with lines, and equations of whatever degree can be interpreted as geometrical figures, so that the properties of geometric structures can be derived from the properties of algebraic equations. The algebraic calculus becomes a tool for solving geometrical problems.

2.4. The calculus of geometrical operation which is analogous to arithmetic is applied by Newton and Leibniz to the analysis of the infinitesimal.[21] The infinitesimal calculus in the form of the Leibnizian differential and integral calculation[22] proceeds from two fundamental thoughts. The first thought, related to differential calculation, consists in representing the function of a curve at any particular point by the function of the tangent belonging to it. Thus one can construct the properties of the function of the curve at any particular point from the properties of the corresponding tangents. The second thought consists in specifying, on the basis of a suitable system of sign rules for calculating the function of the tangent (the differential quotient), what the formal analogues are of the rules for the basic kinds of calculation. This system of rules forms the real 'calculus'.

2.5. It will only be mentioned in passing that Leibniz, referring to a calculus for concepts worked out by the German philosopher and logician Joachim Jungius,[23] made several attempts to develop a logical calculus.[24] This, however, cannot be dealt with here.

2.6. Instead, attention may be directed to one implication of constructing calculi. It consists in being able, in principle, to mechanize the operations in the calculi. This becomes clear from the following considerations: (a) There is a one-to-one correspondence between a sphere of objects and a symbolic alphabet. This correspondence makes it possible to interpret the results of the operations with the symbolic alphabet as statements about the sphere of objects. (b) In order to carry out symbolic operations, rules which bear no relation to the meaning of the symbols are stated. Thus

the formation and transformation of the series of symbols becomes a schematized operation. (c) Because of the one-to-one correspondence, it is possible to solve problems in the sphere of objects by schematic operations in the symbolic sphere. Consequently, the rules of the calculus become algorithms. An algorithm here is a prescription for the way given input data are to be transformed into desired output data in a finite and unequivocally determined number of steps. (d) A schematic operation using symbols according to rules which have no relation to the interpretation of the symbols and which have the function of an algorithm can, in principle, be organized as a mechanical procedure, that is, such an operation can be simulated by a machine.

3. THE ASSUMPTION OF THE *'MATHESIS UNIVERSALIS'* OR ALGEBRA AS A CALCULUS OF QUANTITY

3.1. In the context sketched here a calculus becomes a mechanical procedure which allows the mechanical solution of a certain class of mathematical problems. The next step consists in universalizing the calculus in order to establish the truth value of scientific statements. That is, it is to be developed into a universal procedure of acquiring quantifiable knowledge. This intention may be illustrated by the Cartesian project of the *'mathesis universalis'*.[25]

3.2. Descartes' *'mathesis universalis'* rests on two epistemological assumptions. (a) Knowledge attains certitude by proceeding in steps ordered according to a method which follows from the prescriptions of a unified and universal system of rules.[26] (b) This unified and universal method determines what can become an object of knowledge at all.[27] Only that which can be derived from self-evident statements by the application of the method can be known.[28] In Descartes' conception, the requirement of using reason methodically is ideally met by mathematics[29] – a mathematics, however, as it is to be found in symbolic algebra and in ana-

lytic geometry, where algebraic operations can be interpreted both arithmetically and geometrically.[30] Thus algebra becomes a language in which proportions – relations of quantities – may be expressed independently of their being realized either numerically or figuratively. The rules of algebra become universal procedures having validity for arithmetic and geometry. In contrast to arithmetic and geometry, algebra becomes a higher-order discipline to which other disciplines of mathematics are subordinated.[31] Descartes calls this discipline *'mathesis universalis'*.[32] It was claimed to be a universal science of proportions which provides procedures valid for all spheres of objects having to do with quantity, be it in the form of *'multitudo'* or *'magnitudo'*.[33]

4. THE ASSUMPTION OF THE *'SCIENTIA GENERALIS'* AND THE *'ARS CHARACTERISTICA'* OR THE CALCULUS OF QUALITY AS THE 'UNIVERSAL THINKING MACHINE'

4.1. Whereas the purpose of the Cartesian *'mathesis universalis'* was to work out a universal calculus for deriving and testing quantifiable statements, for Leibniz the goal became that of deriving and testing true statements in general. Leibniz regards his *'scientia generalis'* as a calculus of quality.[34] The insight implicit in Descartes' conception of algebra, that the formalization of epistemic procedures in calculi depends on the introduction of a formalized language, becomes explicit with Leibniz. His *'scientia generalis'* needs an artificial language.[35] This language is to be constructed so that every concept is analytically reducible to some basic concepts,[36] so that, in turn, all complex concepts may be formed by combining these basic ones. To these basic concepts an alphabet of signs should be related in a one-to-one correspondence. Leibniz calls this artificial system the *'calculus ratiocinator'*, and works out a system of rules for it to transform the series of signs.[37] These rules fulfill the criterion of being independent of the mean-

ing of the signs. The *'scientia generalis'* has to perform two tasks:[38] (a) it is an *'ars inveniendi'* insofar as still unknown statements can be generated by exhausting the formal possibilities for combining the basic signs; (b) it is an *'ars iudicandi'* insofar as the truth of already known statements can be demonstrated by analyzing and reducing them to identical judgements.

4.2. The Leibnizian project of a *'scientia generalis'* implies that a machine can be constructed with which new statements can be demonstrated. Leibniz himself indicates this when he speaks about the possibility of producing a combinatory calculus with a machine. It has already been mentioned that a calculus can be mechanically simulated insofar as its rules take the form of an algorithm, that is, insofar as its operations can be described as an effective procedure which is to be performed in a finite number of steps. Hence the two tasks of the *'scientia generalis'* may be described as follows: (a) given an axiomatic system A, there will be an algorithm according to which all possible inferences can be derived mechanically within this system; (b) given an axiomatic system A and a theorem T, there will be an algorithm whereby it can be mechanically decided if T can be derived within A or not.

These assumptions characterize what may be called 'the universal thinking machine'. I choose this metaphor as a way of characterizing the assumption that all acts of knowledge can be carried out according to algorithms. Such an algorithmic operation can always, to use our present terminology, be written down as a computer programme.

4.3. In the mathematical-logical discussions of the 20th century, the concept of algorithm is defined in terms of computability, enumerability and decidability.[40] In certain respects, recursive enumerability can be interpreted as specifying the *'ars inveniendi'*, and decidability as specifying the *'ars iudicandi'*.[41] Defining algorithms in this way, however, shows that decidability and enumerability are not valid for every possible calculus. Thus the impossibility of a universal thinking machine has been demonstrated. But the his-

tory of the Leibnizian programme is not our theme here. I would rather turn to conclusions which may be drawn from the above sketch of the idea that reasoning can be schematized.

5. CONCLUSIONS

5.1. The 17th-century paradigm of technical action is valid not only for the natural sciences, but also for *mathematics* and *logic*. With the beginning of modern times, two strategies for testing the truth of statements emerge. On the one hand, the examination of the so-called 'factual truths' in the natural sciences has to be based on experimentation. On the other hand, the examination of the so-called 'truths of reason' in mathematics and logic has to be based on axiomatic-deductive derivation. Customarily, it is the new role of confirmation by experiment in the natural sciences (under the paradigm of reproducible technical operations) which is interpreted as the new element in the development of modern science. This is to be understood in contrast to the axiomatic-deductive reasoning which has been given its classical form by the Greeks and which remained relatively unchanged up to the transition from the 19th to the 20th century. However, our explication gives a different picture. The logic of technical procedures gives its shape to 17th-century mathematics and logic in the form of a mechanizable calculus. The deductive method based on the ideal of mechanical deducibility through the pure syntactic transformation of series of symbols is the new element in the conception of a deductive process, which was not conceptualized by the ancient Greeks. Nevertheless, in contrast to David Hilbert's later attempts to work out a complete axiomatic calculus, we cannot overlook the fact that the conception of a 'calculus without interpretation', just as a 'sign without meaning', is not to be found in the 17th century. The symbols of a 17th-century calculus were always thought of as representing states of affairs given independently of

the signs of the calculus. The technical goal of a 17-century calculus consists precisely in simplifying the operations relating to complicated states of affairs by making them operations in calculi. This is to be done in such a way that, in principle, anyone at any time can perform any given operation in the same way, insofar as he or she has mastered the language of signs and its corresponding rules.

5.2. The rationalistic concept of reason is oriented towards the ideal of reasoning which can be *mechanized.* The guiding idea for rationalism, the theory of knowledge of the 17th century, was the methodical use of reason. While this idea was tied to the assumption of a universal scientific language, which could be formalized, and to the assumption of a universal method of reasoning, which might be formulated according to algorithms, the paradigm of a mechanizable operation was also valid for the rationalistic concept of reason in general. A statement is rational if, in principle, it can be inferred even by a machine from other true statements. The truth of a proposition is then guaranteed by the correctness of its derivation. Technical producibility becomes the criterion of modern rationality. This is made possible by a radical shift in the semantic pattern, as a result of which signs are no longer encoded ciphers for the objects they refer to, but rather come to represent these objects according to convention. The modern epistemological ideal presupposes representative symbolism. Knowledge is no longer oriented towards a hermeneutical model of the interpretation of signs as it was especially in the Natural Philosophy of the 15th and 16th centuries, but, rather, it is oriented towards the model of forming and transforming signs by employing algorithms.

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NOTES

[1] See M. Foucault, *Die Ordnung der Dinge*, trans. U. Köppen, Frankfurt 1971, Ch. 2.
[2] For example T. Campanella, *Realis philosophia*, Frankfurt 1623, cit. Foucault, *Op. cit.*, p. 48.
[3] For example T. Paracelsus, 'Die 9 Bücher der Natura rerum', in *Sämtliche Werke*, ed. K. Sudhoff, München–Berlin 1923–33, Vol. 11, p. 393, cit. Foucault, *Op. cit.*, p. 57.
[4] B. de Viginère, *Traité des chiffres*, Paris 1587, cit. Foucault, *Op. cit.*, p. 70.
[5] For example T. Campanella, *De sensu rerum et magia*, Frankfurt 1620, cit. Foucault, *Op. cit.*, p. 64.
[6] J. Locke, *An Essay Concerning Human Understanding*, New York 1959, Vol. II, Book III; *Hobbes' Leviathan*, reprinted from the edition of 1651 with an essay by the late W. G. Pogson Smith, Oxford 1965, Ch. IV.
[7] Sometimes Leibniz uses the term 'expression' instead of 'representation': "Une chose exprime une autre (dans mon langage) lors qu'il a un rapport constant et réglé entre ce qui se peut dire de l'une et de l'autre. C'est ainsi qu'une projection de perspective exprime son géométral." (G. W. Leibniz, *Discours de Methaphysique et Correspondance avec Arnauld*, ed. G. Le Roy, Paris 1957, p. 180 ff.)
[8] See Y. Belaval, *Leibniz critique de Descartes*, Paris 1960, p. 148.
[9] G. W. Leibniz, *Opuscules et fragments inédits*, ed. L. Couturat, Paris 1903, p. 15; G. W. Leibniz, *Neue Abhandlungen über den menschlichen Verstand*, trans., intr. and notes by E. Cassirer, Leipzig 1915, VIII § 13.
[10] See A. Gurwitsch, *Leibniz*, Berlin–New York 1974, p. 37.
[11] G. W. Leibniz, 'Initium institutionum iuris perpetui', in *Mitteilungen aus Leibnizens ungedruckten Schriften*, ed. G. Mollat, Leipzig 1893, p. 4; for further examples see *Die philosophischen Schriften von Gottfried Wilhelm Leibniz*, ed. C. J. Gerhardt, Berlin 1875–1890, Vol. VII, p. 205.
[12] G. W. Leibniz, 'Dialog über die Verknüpfung zwischen Dingen und Worten', *ibid.*, p. 190 ff.
[13] See W. Jordan, 'Die Leibniz'sche Rechenmaschine', *Zeitschrift für Vermessungwesen* 26 (1987), 283–315.
[14] G. W. Leibniz, 'Explication de l'arithmetique binaire', in G. W. Leibniz, *Mathematische Schriften*, ed. C. I. Gerhardt, London–Berlin–Halle 1850–1863, Vol. VII, p. 223 ff.
[15] O. Becker, *Grundlagen der Mathematik in geschichtlicher Entwicklung*, München 1954, p. 130.
[16] K. Menninger, *Zahlwort und Ziffer*, Göttingen 1979[3], Vol. II, p. 256 ff.
[17] F. Viète, 'In artem analyticem isagoge' (Tours 1591), in *Opera mathematica*, ed. Schooten, Leiden 1646 (reprint: Hildesheim 1970).
[18] Viète distinguishes between *'logistica numerosa'* and *'logistica speciosa'*: "Logistica numerosa est per numeros, Speciosa quae per species seu rerum formas exhibetur, ut pote per Alphabetica elementa. Logistices speciosae canonice praeceptae sunt quatuor, ut numerosae ...". (*Ibid.*, p. 5.)

[19] R. Descartes, 'Geometrie', in *Oeuvres*, ed. Adam and Tannery, Paris 1965–75, Vol. VI.

[20] See E. R. Grosholz, 'Descartes' Unification of Algebra and Geometry', in *Descartes*, ed. S. Gaukroger, Sussex 1980, p. 156 ff.

[21] C. B. Boyer, *The History of the Calculus*, New York 1959, Ch. IV; J. O. Fleckenstein, *Der Prioritätenstreit zwischen Leibniz und Newton*, Basel–Stuttgart 1956.

[22] G. W. Leibniz, 'Neue Methoden der Maxima und Minima sowie der Tangenten ... und eine darauf bezügliche Rechnungsart', in *Acta Eruditorum* 1684, trans. G. Kowalski; G. W. Leibniz, 'Rechtfertigung der Infinitesimalrechnung durch den gewöhnlichen algebraischen Kalkül', in *Hauptschriften zur Grundlegung der Philosophie*, trans. A. Buchenau, Vol. I, Leipzig 1904, p. 101 ff.; see also J. E. Hofmann, *Die Entwicklungsgeschichte der Leibnizschen Mathematik während des Aufenthaltes in Paris 1672–1676*, München 1949, p. 118 ff.

[23] H. Kangro, 'Heuretica (Erfindungskunst) und Begriffskalkül – ist der Inhalt der Leibnizhandschrift Phil VII C139r–145r Joachim Jungius zuzuschreiben?', *Sudhoffs Archiv* 25 (1968) 1, 48 ff.

[24] See G. W. Leibniz, *Opuscules et fragments inédits, op. cit.*, p. 239.

[25] For different interpretations of Descartes' *mathesis universalis*, see Hans Werner Arndt, *Methodo scientifica pertractatum. Mos geometricus und Kalkülbegriff in der philosophischen Theorienbildung des 17. und 18. Jahrhunderts*, Berlin–New York 1971, p. 30 ff.; L. J. Beck, *The Method of the Regulae*, Oxford 1952; J. Laporte, *Le Rationalisme de Descartes*, Paris 1945; J. Mittelstrass, 'Die Idee einer Mathesis Universalis bei Descartes', *Perspektiven Philosophie Neues Jahrbuch* 4 (1978), 177 ff.; J. A. Schuster, 'Descartes Mathesis Universalis 1619–1628', in *Descartes, op. cit.*, p. 41 ff.

[26] See R. Descartes, 'Regulae ad Directionem Ingenii', in *Oeuvres*, *op. cit.*, Vol. X, Rule IV, pp. 374–375.

[27] See Rule VIII, *ibid.*, p. 398.

[28] See Rule III, *ibid.*, p. 369 ff.

[29] See Rule II, *ibid.*, p. 363 ff.

[30] See Rule IV, *ibid.*, p. 373.

[31] See Beck, *Op. cit.*, p. 199; Laporte, *Op. cit.*, p. 8 ff.

[32] "Quod attentius consideranti tandem innotuit, illa omnia tantum, in quibus aliquis ordo vel mensura examinatur, ad Mathesim referri, nec interesse utrum in numeris, vel figuris, vel altris, vel lonis, aliove quvois objecto talis mensura quaerenda sit; ac proinde generalem quandam esse de ere scientiam, quae id omne, explicet, quod circa ordinem et mensuram nulli speciali materiae addictam quaeri potest, eademque, non alcititio vocabulo, sed jam veterato atque usu recepto, Mathesim universalem nominari, quoniam in hac continetur illud omne propter quod aliae scientiae et Mathematicae partes appeluntur." ('Regulae ...', in *Oeuvres, op. cit.*, Vol X, Rule IV, p. 378.)

[33] Rule XIV, *ibid.*, p. 451 ff.

[34] G. W. Leibniz, 'De Synthesi et Analysi: Universali seu Arte inveniendi et iudicandi', in *Die philosophischen Schriften, op. cit.*, Vol. VII, pp. 297–298; G. W. Leibniz, 'Calculus alternativus', in *Opuscules et fragments inédits, op. cit.*, p. 556.

[35] G. W. Leibniz, 'Dissertatio de Arte combinatoria', in *Die philosophischen Schriften, op. cit.*, Vol. IV, Berlin 1880, p. 72 ff.
[36] G. W. Leibniz, 'De Organo sive Arte Magna cogitandi', in *Opuscules et fragments inédits, op. cit.*, p. 430.
[37] See G. W. Leibniz, *Die philosophischen Schriften, op. cit.*, Vol. VII, p. 194 ff.
[38] G. W. Leibniz, 'Ad Specimen Calculi universalis addenda', *ibid.*, p. 221 ff.
[39] G. W. Leibniz, 'Specimen Calculi Universalis seu Arte inveniendi et iudicandi', *ibid.*, pp. 292–298.
[40] See A. Church, 'An Unsolvable Problem of Elementary Number Theory', *American J. of Math.* 58 (1936), 345 ff.; 'A Note on the Entscheidungsproblem', *J. of Symbolic Logic* 1 (1936), 40 ff. and 101 ff.; St. C. Kleene, 'Recursive Predicates and Quantifiers', *Transactions of the American Math. Soc.* 53 (1943), 41 ff.; E. L. Post, 'Formal Reductions of the General Combinatorial Decision Problem', *American J. of Math.* 65 (1943), 197 ff.; A. M. Turing, 'On Computable Numbers with an Application to the Entscheidungsproblem', *Proc. London Math. Soc.* 42 (1936), 230 ff.

GIDEON FREUDENTHAL

TOWARDS A SOCIAL HISTORY OF NEWTONIAN MECHANICS. BORIS HESSEN AND HENRYK GROSSMANN REVISITED

In the present paper I shall argue for a social history of science, different from an 'externalist' approach in that it explains the cognitive content of science, too, and different from the approach of the 'sociology of scientific knowledge' in that it upholds the scientific claim to objective knowledge. I shall present my argument in a discussion of the papers of Henryk Grossmann and Boris Hessen which are both concerned with the scientific revolution of the 17th century.

Henryk Grossmann's paper, 'Die gesellschaftlichen Grundlagen der mechanistischen Philosophie und die Manufaktur' (1935) has not been translated into English, nor taken notice of. Boris Hessen's paper, 'The Social and Economic Roots of Newton's *Principia*' (1931) is notoriously ill-reputed. Having been discussed for many years, the conclusion now seems clear: "No one who is seriously engaged in the study of Newton finds his [Hessen's] account useful as an illumination of the *Principia*."[1]

Hessen's argument, "science develops out of production", is interpreted to maintain "that the focus of scientific interest of 17th century England were responses to the technological needs of merging merchant capitalism and manufacture".[2] This "Hessen-thesis" suffers from "the naivety of crude forms of externalism".[3] This crude externalism seems to me rather to characterize the interpretation of Hessen's essay than the essay itself. I shall in the following discuss the basic questions of Hessen's paper and shall suggest a different interpretation, which not only avoids the crude

Imre Hronszky, Márta Fehér and Balázs Dajka (eds), Scientific Knowledge Socialized, 193–212.
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forms of externalism but also makes the concept 'externalism' as well as its complementary concept 'internalism' superfluous.

Hessen's essay has five chapters. I shall dispense with a discussion of Capter 1, 'Marxist Theory of the Historical Process' and Chapter 5, 'The Machine-Wreckers of Newton's Epoch and the Present-day Wreckers of Productive Forces'. The second chapter, 'The Economics, Physics, and Technology of Newton's Period', has received the greatest amount of attention. Hessen sums up the result of this investigation as follows:

> This rough outline of the contents of the 'Principia' exhibits the complete coincidence of the physical thematics of the period, which arose out of the needs of economics and technique, with the main contents of the 'Principia' ... [4]

Hessen therefore called the *Principia* a "survey of mechanics of the earth and the heavenly bodies".

I shall leave this assertion uninterpreted for the moment and turn to Chapter 4 entitled: 'Engels' Conception of Energy and Newton's Lack of the Law of Conservation of Energy.'

Hessen's summary reads: "Thermodynamics not only received an impulse to its development from the steam machine, but, in fact, developed by the study of that machine."[5]

Taking these two propositions of Hessen's together, we obtain a thesis that ascribes to technology a three-fold significance for the development of science. First of all, the socio-economic development expressed in and conditioned by the state of technology determines a social interest in technology; these economic-technological problems thus constitute an 'impulse' for the development of science, and technology becomes the subject matter of scientific study. As the subject of scientific study – this is the second important function of technology for science – the functioning of the machine determines the spectrum of specific questions that can be investigated.

Finally, a third function of technology for the development of science must be considered. *Whether* certain questions can be answered, and *how* they can be answered also depends on the state of technology both as the subject matter of science and as an instrument of scientific research.

These three functions are of course not independent of one another. But the development of certain areas of technology was connected with their role in the social process of production quite independently of their later function in science. Hessen's thesis on the importance of socio-economic interests should thus be seen in two different temporal aspects: first of all, the development of practical mechanics itself, which later became a prerequisite for science, as a reservoir of objects for study and of instruments for scientific work, and secondly, the engagement of science in studying questions of technology. Both aspects are connected by the technological literature: the development of technology follows and entails an advancement of knowledge, and in the 16th century a large number of technological treatises appear, generalizing and systematizing a well-advanced knowledge. Hence looking for a synthesis between the craftsman, who did much but had no ideas, and the philosopher, who thought much and did little, misses the point.[6]

On this interpretation, the Hessen Thesis turns out to be complementary to the study conducted independently by Henryk Grossmann, 'Die gesellschaftlichen Grundlagen der mechanistischen Philosophie und die Manufaktur'.[7] Grossmann argues that since the time of Leonardo da Vinci mechanics had developed through the study of machines. The best example of such a connection is of course Huygens's *Horologium Oscillatorium* of 1675. But I want to point out the more general aspects as well. Grossmann believed that practical mechanics had provided fundamental theoretical prerequisites for theoretical mechanics. It had reduced the infinite multiplicity of forms of motion to a uniform driving force or engine by hitching different instruments to the same engine[8]

and thus made possible a unified concept of driving force; which was first discussed in the technological literature; secondly it had equated the mechanics of the earth with that of the celestial bodies by constructing mechanical planetaria[9] long before Newtonian mechanics unified both areas with the universally applicable three laws of motion. This thesis applies both to particular insights as well as to general patterns of interpretation, e.g. the interpretation of the world system according to the model of a mechanical clock.

The Hessen–Grossmann Thesis on the relationship between economic-technological development and the development of science has thus three components:

(1) Economic-technological interests were the reason why practical mechanics reached a stage of development in certain areas which must be considered as a prerequisite for the development of scientific mechanics. This applies both to machines whose functioning could be studied and to the scientific instruments.

(2) Practical mechanics and the corresponding literature developed paradigms of interpretation, which theoretical mechanics took up.

(3) Economic-technological problems provide an impulse to deal with certain physical problems.

We can thus see that the usual criticism of the 'externalist' thesis does not refute the Hessen–Grossmann Thesis. For, against the externalist thesis it has been pointed out[10] that technological interests alone did not determine research. Electricity, for instance, was studied intensively although no economic interests were tied to this subject. I believe, however, that the Hessen–Grossmann Thesis is not only *not* refuted by this argument, but that, it can even explain why Newton's *Principia* is a survey of the mechanics of the earth and the heavenly bodies but not of electricity, too.

The former were the areas in which practical mechanics had attained such a stage of development that its scientific analysis could succeed. This was not the case for electricity nor, for example, in chemistry; Newton and others took an interest in both

these fields, but since practical production and the theoretical tradition dependent on it had provided no prerequisites for the scientific treatment of them, the necessary prerequisites had first to be created in scientific work itself. If the thesis that the development of the study of electricity was dependent on the development of appropriate instruments (J. Heilbron) is correct, then electricity and similar examples do not provide any argument against the Hessen–Grossmann Thesis; rather, they suggest that we formulate the thesis even more generally. For, if the knowledge that can be gained depends on the instruments available then it is clear that the instruments applied – whether they are taken directly from social production or produced in the process of scientific work (this possibility, too, depends of course on the general state of technology) – are nothing external to theoretical work. On the contrary, they constitute precisely the basis of cognitive development.

Let me now consider the question, which of the three aspects of the Hessen–Grossmann Thesis can at all be tested by an examination of the *Principia* itself.

It is clear from the start that we must be able to recognize the area of socio-economically important technological problems in the physical questions treated. This was done by Hessen and his results were supplemented by Merton and others.

The first and second components of the Hessen–Grossmann Thesis, namely the question to what extent Newton's insights were conditioned by the state of technology, has received much less attention. Moreover, most of the issues cannot be examined directly in Newton's *Principia.* For instance, it is clear that Newton had to rely on the astronomical data of Flamsteed and others. The technological and the cognitive prerequisites for the work of the astronomers cannot be studied in Newton's book.

The situation is somewhat different when one asks to what extent technology as an object to be studied was a prerequisite for Newton's mechanics. Here, too, it would be short-sighted to de-

mand biographical evidence or descriptions of experiments performed.

But especially if Newton did not himself study machines but rather relied on the studies of others who dealt with practical mechanical problems, then one might easily expect his appropriation of traditional knowledge to show traces of the practical questions, the terminology, and the theories of the artisans and mechanics.

And this is indeed the case; Newton uses e.g. for the simple machines the common terminology of the technical literature, speaking of 'increasing force by reducing speed' and the like. Modern studies comparing technical and scientific literature are still a rarity; I am quite sure that studies of this kind would show that early scientific terminology and conceptions build upon the technological literature.[11]

A further argument in support of this thesis is the following: If mechanics had not also been a theory of technology, then it would also be anything but obvious that theoretical mechanics can explain the functioning of machines. But even in the *Principia*, that is, at a time when no applied physics had been developed or put to use, Newton illustrated his laws of motion by applying them to explain the laws of the five simple machines, as well as such machines as "clocks and such like instruments made up from a combination of wheels".[12] My point here is not the fact that mechanics accounts for the function of machines but rather the fact that Newton was confident that it would. This conviction is complementary to his view as expressed in the preface to the *Principia* that rational mechanics is a mathematically exact theory of the functioning of machines.

So much for the Hessen–Grossmann Thesis. Before I turn to Hessen's other thesis, which concerns the relationship between Newton's work and the English Revolution, I would like to outline briefly some of the far-reaching consequences which can be drawn from this first complex of theses.

We can interpret the relationship of theoretical to practical mechanics as follows: The subject matter of theoretical mechanics of the earth was not simply nature, but rather a machine constructed by man. The machine functions according to laws of nature, it is a natural object. It is also the embodiment of human knowledge and human purposes. It is of importance that one cannot construct a machine such as a water mill or a clock without possessing a rather advanced knowledge of the laws of nature, but one can construct it without knowing Newtonian mechanics, which explains the functioning of the machine much better than its constructor could have done. But the machine builders of Newton's time did not draw their knowledge from nowhere. Many generations of specialists had inherited machines and the knowledge of how to build them from the respective previous generations and had increased this knowledge by more or less systematic investigations and embodied it in the construction of improved machines.

One can even ask whether the transmission and development of knowledge in general cannot be much better interpreted and comprehended if one investigates in each case the dependence of transmission, application, and development of knowledge in its dependence on its material media, whether these be instruments or means of representation.

If one can furthermore explain the origin of new knowledge from the application of traditional, continually improved means of investigation as well as through the investigation of objectified knowledge – be it in the technical apparatus or in material representations – then a theory can be conceived that interprets the transmission and development of scientific knowledge in a manner not fundamentally different from the transmission and development of social means of production in general and which thus comprehends science as a particular element in the system of social labour: Hessen's dictum, 'science develops out of production' could thus be supplemented by saying that science shares essential common characteristics of material production.[13]

The third chapter of Hessen's paper bears the title 'The Class Struggle during the English Revolution and Newton's Philosophical Outlook'.

The *Principia*, Hessen says, is also a 'conception of the universe', and he undertakes to explain how Newton's compromise between materialism and religion expressed the views of his class, which supported the compromise of 1688, and also how Newton's concept of inert matter and of absolute space, in which material bodies can be absolutely at rest, conditions his introduction of God into the system:

> It is clear that such a conception of the modality of movement must inevitably lead to an introduction of an extrinsic motive force, and with Newton this role is filled by God.[14]

Two questions must be distinguished here. The first concerns the relationship between Newton's philosophical views and the sociopolitical ideologies of his time; the second is whether Newton's ideological commitment also had significance for the scientific content of his physics. The first question has been studied closely in recent years – especially by James and Margaret Jacob.[15] These investigations have to a large extent confirmed Hessen's judgement about Newton's philosophical views and have provided rich material on the use of his work to bolster the ideological positions of the factions supporting the compromise of 1688. But Hessen's thesis goes a step farther: he maintains that ideological commitments led to Newton's conception of matter and of motion. Hessen did not provide the necessary evidence of a connection between Newton's ideological positions and his concepts of matter and space. The influence of the philosophical interpretation of Newtonian concepts on the ideologies of the time has been amply demonstrated by the Jacobs. Nevertheless, to demonstrate the significance of the ideological position for the cognitive content of science, one has to show that Newton's ideological commitments had an influence on the scientific theory itself and not only on its

philosophical interpretation. I would like, therefore, to report briefly the results of a study that I published in 1982,[16] in which I think have provided evidence for this claim. Let us then take up Newton's *Principia.*

The first Definition in Newton's *Principia* is the definition of 'mass'. It reads,

> The quantity of matter is the measure of the same arising from its density and bulk conjointly.[17]

Ernst Mach rightly saw a circle in this definition, "since we can define density only as the mass of unit volume".[18]

Such a seemingly trivial mistake by Newton must, I think, prompt the question whether it is possible to justify this definition in such a way that no circle is involved; and the possibility is easy to see if we take Newton's specific version of atomism into account. When Newton undertook to explain why certain materials have a greater specific gravity than others, he wrote that the differing specific gravities of bodies prove the existence of a vacuum. A denser body is proportionately fuller than a less dense body. Newton's argument is thus that the ultimate particles of all materials are absolutely dense and of equal volume; only the composition of bodies differs, that is, the relation between filled and empty volumes or the relation between the number of equal particles of matter and the amount of vacuum per unit of volume. Newton's definition of the quantity of matter follows consistently from this consideration. The mass of a body is determined by its density – that is, by the number of particles per unit of volume – and by its bulk – that is, by the number of standard units of volume, multiplied by the mass of one standard particle.

A second example can characterize Newton's atomistic theory more precisely and show its influence on his physics.

Newton, as is well known, uses two experiments to prove the existence of absolute space. First of all he cites an experiment that

can actually be carried out – with a rotating bucket of water. A bucket filled with water is hung up on a twisted cord. The water and the bucket are at rest relative to one another. When the bucket is released, it begins to rotate as the cord unwinds, and it moves relatively to the water, which remains at rest. The surface of the water remains smooth. The motion is gradually imparted to the water until the water is moving just as fast as the bucket, that is, until it is again at rest relative to the bucket. At the same time the surface of the water takes on a concave shape, that is, the water receding from the axis of rotation climbs up the side of its container. It reaches its highest mark when the water is at rest relative to the bucket:

> This ascent of the water shows its endeavor to recede from the axis of its motion; and the true and absolute circular motion of the water, which is here directly contrary to the relative, becomes known, and may be measured by this endeavor.[19]

The experiment proves – so Newton believed – that there is a difference between relative and absolute motion, and an absolute motion is a motion relative to empty space. This latter interpretation is supported with a thought experiment: Two balls connected by a string rotate around their common center of gravity in otherwise empty space. The balls are at rest in relation to one another, but the tension in the string (the effect of centrifugal forces) shows their motion with respect to empty, immovable, absolute space.

Again, it was Ernst Mach who criticized Newton's conclusions. Mach doubted that centrifugal forces would in fact appear if space were really empty; he also questioned whether they would really not appear if – in our world – the heaven of fixed stars were rotated around a body at rest.

The point here is that Newton did not suspect any difference between the two examples, that he presupposed that a phenomenon occurring on a rotating body in our world system would also

occur even if there were only two bodies rotating in otherwise empty space. I propose therefore the thesis that Newton assumed that physical phenomena can not only be traced back to the 'properties of things', but also that these properties can be attributed unchanged to each and every particle in empty space.

Let me briefly substantiate this assertion. Consider Newton's characterization of the *one property* which cannot be formulated as the property of a *single* body: gravitation, which can only be formulated as *mutual gravitation* and thus presupposes the existence of at least two bodies.

In his 'Regula Philosophandi' Newton discusses the *universal* properties of the smallest parts of bodies, that is, those properties which fulfill two conditions:
(1) That they "admit neither intensification nor remission of degrees" and
(2) that they "are found to belong to all bodies within reach of our experiments".[20]

Newton then lists the qualities that meet these conditions, and these are extension, hardness, unpenetrability, mobility and (force of) inertia. Gravitation is not mentioned at first.

In a hand-written addendum to his personal copy of the first edition Newton later remarked:

> Gravity is a quality of all bodies on which experiments can be conducted, and, existing in individual bodies in proportion to the quantity of matter, it cannot be intensified or remitted, and consequently by Hypothesis III [= regula III – G. F.] it is a property of all bodies.

Gravity, then, meets the requirements of a 'universal property'. In the third edition of the *Principia* Newton however wrote:

> Not that I affirm gravity to be essential to bodies. By their *vis insita* I mean nothing but their force of inertia. This is immutable. Their gravity is diminished as they recede from the earth.[21]

The argument seems to be nonsensical; for it is not the 'property' gravity but rather its effect, the acceleration of the gravitating bodies, which diminishes with distance, that is, the formula for gravitation is still valid: $G=\frac{m \cdot m'}{r^2}$. But Newton's argument in the third edition does make sense if we consider the reason given for the introduction of the criterion for a universal poperty: "and such (qualities) as are not liable to diminution can never be quite taken away."[22]

Now, if essential properties are those which can be attributed to a single particle in empty space, then Newton is right in saying that gravitation is not an essential property, since a single particle does not gravitate. Inertia, on the other hand, is an essential property, since in the lack of powers in empty space the single particle would preserve its state of motion.

Summing up, we can say that fundamental concepts of Newtonian mechanics depend on his interpretation of the analytic–synthetic method as maintaining that phenomena should be traced back not merely to the properties of things or to simple laws but rather to those properties which belong to every body even as a single body independent of all others.

Is this view peculiar to Newton or was it shared by others? And how can it be justified?

Francis Bacon dealt with the origins of the analytic–synthetic method in his *Novum Organum*:

> Now the human understanding is infected by the sight of what takes place in the mechanical arts, in which the alteration of bodies proceeds chiefly by composition or separation, and so imagines that something similar goes on in the universal nature of things. From this source has flowed the fiction of elements, and of their concourse for the formation of natural bodies.[23]

Now I do not want to maintain here that the analytic–synthetic method was simply borrowed from 'mechanical arts', nor do I want to review the extensive literature on the development of this

method in various sciences under various interpretations. I merely want to stress two points:

(1) That Newton's explanation of the method indeed agrees with the artisan tradition of 'compounding' and 'separating'. For in practical mechanics it must be assumed practically that the properties of elements are independent of the system constructed by combining them.

(2) That not only Bacon but also many other scientists of the 17th century interpreted the method on the model of artisan procedures.

But the analytic–synthetic method was supposed to be not just the method of mechanical arts and physics, but also the universal scientific method. Thomas Hobbes, for instance, introduced the method into his general methodology:

> For everything is best understood by its constitutive parts *(ex quibus rebus constituitur)*. For as in a watch, or some such small engine, the matter, figure, and motion of the wheels cannot well be known, except it be taken insunder and viewed in parts.[24]

Newton's implicit presupposition is introduced by Hobbes explicitly, both in natural philosophy[25] and in social philosophy. In *De Cive* Hobbes writes: "so to make a more curious search into the rights of states and duties of subjects, it is necessary, I say, not to take them insunder, but yet that they be so considered as if they were dissolved."[26]

Now the purpose of Hobbes's procedure is pretty obvious: Hobbes's theory that the state originates in a contract of individual men with the sovereign, depends on the presupposition that before entering into the contract all men could live on their own and possessed all properties necessary to make a contract possible but not compelling. They must be able to live self-sufficiently and have freedom and reason. As a consequence, Hobbes must (as he points out himself):

Consider men as if but even now sprung out of the earth and suddenly, like mushrooms, come to full maturity, without all kind of engagement to each other.[27]

But is there any real connection between the analytic–synthetic method as applied in natural and in social philosophy? One connection between natural and social philosophy lies in the fact that they have an object in common: Man. For as a member of society man is subject matter for social philosophy; as a living creature he is subject matter for natural philosophy. But some properties of human beings are even the special subject of mechanics, such as the ability of humans – just like gravity or the impact of a moving body – to set an unmoved body in motion. This ability of humans also makes a connection to practical mechanics, in which Bacon suspected the origins of the analytic–synthetic method and of atomism. For the transmission machines of the time could be driven by water power or some other force of nature but also just as well by humans or animals. The concept of 'force' had to apply to all kinds of power sources. Even without going into the often assumed anthropomorphic sources of the concept of force, we can ascertain an immediate connection between the analysis of human motions and that of general mechanical motions, that is, between physiology and mechanics. However, the question of the ability of humans to move has not only physical and physiological aspects; it also has moral and socio-political implications. For, the capacity of humans for arbitrary motion – and not like inanimate objects having merely the ability to be moved – was seen as the basis of their freedom. And the freedom of humans is, as we have seen, also one of the presuppositions of all contract theories of society.

To understand Newton's position we must first consider the position of Descartes to which Newton refers. With his argument, *Cogito ergo sum* Descartes began a new chapter not only in epistemology but also in physiology. For, the 'soul', which was traditionally thought of as endowed with powers of sensation, motion, and thought *(facultates sentiendi, movendi et intelligendi)*, was

now ascribed only the property of understanding, no longer the ability to move the body. This is, on the one hand, a revolution in physiology – as Descartes argues at the beginning of his *La description du corps humain*; on the other hand, it means that the willful motion of humans can no longer serve as a proof of their freedom. This position, which Leibniz also took up, is attacked by Newton. In an argument drafted for the discussion with Leibniz and oriented on Descartes, Newton defines: Phenomena I call whatever can be perceived, either things external which become known by the five senses, or things internal which we contemplate in our minds by thinking. As fire is hot and water is wet, and gold is heavy, and sun is light, I am and I think.[28] In the same connection Newton appealed to the "daily experience of all mankind",

> every man finding in himself a power of seeing with his eyes and moving his body by his will.[29]

Human freedom is thus guaranteed by the fact that everyone can move his body by his will. But Hobbes's and Leibniz's argument that the will itself is determined by external motives also had to be refuted. The counter argument is that the human will could act spontaneously even if there were no motives at all and the alternatives were consequently indifferent: Samuel Clarke, representing Newton in the debate with Leibniz wrote:

> But intelligent beings are agents; not passive, in being moved by motives, as a balance is by weights; but they have active powers and do move themselves, sometimes upon the view of strong motives, sometimes upon weak ones, and sometimes where things are absolutely indifferent.[30]

Now both Clarke and Leibniz agree that absolutely indifferent situations can be realized only if we place an active principle, a will, in empty space. The discussion of human freedom is thus carried out in the form of a discussion of whether God could act in empty space.

It is not necessary to take up the additional arguments for and against this concept of freedom; in our context it is only important to note that the concept of freedom which is grounded here demands that the will could act spontaneously in empty space. Only then is it possible to attribute this freedom to the will as an essential property. The spontaneity of the free will demands, however, that matter can be moved and that it is different from the active principle. The determination of the essential qualities of the active principle, of the will, thus conditions the determination of the essential qualities of the passive principle: passivity is thus ascribed to *matter* as an essential quality.

It is appropriate here to draw some conclusions from and make some explanations about this reconstruction of the Newtonian interpretation of the analytic–synthetic method:

(1) First, I would like to point out that this interpretation applies only to the notion that qualities like inertia, etc. can be attributed to objects as *essential properties*. It does not necessarily concern the question of how these qualities are defined. The alternative given at the time between a concept of inertia as *'inclinatio ad quietem'* and the Newtonian concept of 'conservation of the state of motion', for instance, is not explained by this reconstruction.

(2) Furthermore, to complete the demonstration of the philosophical-physiological-ideological conditioning of Newton's concept of matter and the concepts dependent on it (density, absolute space, etc.), it would be necessary to show that an alternative foundation of dynamics would have been possible in the 17th century. I believe I have provided such a demonstration in the study mentioned earlier: I have shown there that Leibniz grounded his dynamics on the concept of the conservation of *vis viva* in systems of bodies and that one can derive the same laws of motion in his system as in Newton's. It can also be shown that Leibniz explicitly relates all physical propositions to systems and criticizes the concept of ultimate element. It can furthermore be shown that Leibniz rejected the English contract theories of society as well as

the corresponding concept of freedom. Finally, the differences touched on here can be traced back to differing social relations.

To put it briefly, it can, I believe, be shown that we are dealing with differing philosophical systems each of which is conditioned by social relations; within each of these differing foundations of dynamics are possible, which are however of equal value as physical theories.

(3) From these remarks it should not be concluded that socially conditioned philosophical systems determine immediately basic assumptions of physics. On the contrary, I have shown in the discussion of the *Cogito*-arguments of Descartes and Newton how many theoretical presuppositions of a special nature – for instance physiological notions – go into this supposedly 'immediate experience'. Experience can neither be reduced to supposedly immediate sense perception nor to rigid, pre-given paradigms of interpretation. The point is, however, that the paradigm of interpretation that I have discussed includes the experiences of practical and theoretical mechanics as well as of physiological theories with their moral and social philosophical presuppositions and consequences. There is of course no necessity that the paradigms of interpretation are always as rich as in this case, but there is also no reason to believe that this case is exceptional. The problems discussed here also played an important part, for instance, in the discovery of the principle of the conservation of energy, which was made in physiology and was connected with discussions about 'vital forces' and physicalist materialism in general.

In conclusion I should explain briefly the conception of the development of science touched on before and to compare it with earlier attempts at socio-historical interpretation and with the criticism of such attempts.

Critics of socio-historical interpretations of science – for instance, Koyré – have, I think, rightly pointed out that such interpretations do not affect the 'scientific chain of thought'. I hope I have succeeded in showing that this need not always be the case.

As is well known, there have already been a number of attempts to give a so-called 'externalist' explanation for the process of theory formation. I mention only the sociology of knowledge, Borkenau, Sohn-Rethel and the 'strong programme' in Edinburgh. However different these approaches are, they all suffer from at least one of three basic failings: they explain only very general characteristics of theories, in relation to which any specific theory is indifferent; they do not indicate how a supposedly pre-given paradigm of interpretation is connected with the formation of particular concepts; and finally, they cannot explain the possibility of acquiring objective knowledge.

When Zilsel – to mention one of the most serious attempts of this kind – explains the origin of an experimental-mathematical or experimental-logical method in terms of social history,[31] his explanation applies to a characteristic of all theories of modern science, whether in physics, chemistry, biology, or any other science. The differences between the methods of the different disciplines as well as within one discipline both at one time and at successive points in time are not considered. The usefulness of this thesis for the investigation of any specific problem is correspondingly small. It is therefore not surprising that Zilsel was able to remain a convinced empiricist.

The third failing mentioned above – which especially characterizes the various forms of sociology of knowledge – lies in the conclusion that scientific knowledge is sociomorphic. When one explains the concept of a scientific law, of mathematics or logic as the result of a reflection of social relations, one stumbles into the quandaries of the traditional question of epistemology: how the subjective thought forms are to correspond to the laws of objective reality. It is not enough to claim that theories are generally underdetermined by experience and hence that different theories can in principle account for the same range of phenomena;[32] it has to be *shown in concreto* that in a specific time such different theories were or could have been conceptualized. It is only then that

the concept of an ideology-laden theory representing objective knowledge makes sense.

None of these objections can, I believe, be raised against the conception I have outlined. If one considers scientific work as a specific form of social labour and derives the acquisition both of particular knowledge and of paradigms of interpretation from actions dealing with structured objects within the framework of social relations, then relativism is excluded from the start. For whatever experiences contribute to the formation of a paradigm of interpretation, the paradigm must be able successfully to structure experience. Even if one assumes the possibility of a very strong influence on paradigms of interpretation by social and political ideologies, as I have tried to show for Newton, the interpretation must, nonetheless, account for physical experience. Ideology-laden concepts and theories can be tested and developed further just as can any other concept.

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[29] A. Koyré and I. B. Cohen, 'Newton and the Leibniz–Clarke Correspondence,' *Arch. Int. Hist. Sci.* 15 (1962).
[30] *The Leibniz–Clarke Correspondence*, ed. Alexander, 1956, Clarke's 4th Reply, §§ 1 and 2.
[31] Cf. E. Zilsel, *Die sozialen Ursprünge der neuzeitlichen Wissenschaft*, ed. Krohn, 1976.
[32] Cf. D. Bloor, 'Durkheim and Mauss Revisited: Classification and the Sociology of Knowledge', *Stud. Hist. Phil. Sci.* 13 (1982) 4. .

HUBERT LAITKO

ON THE EMERGENCE OF SCIENTIFIC DISCIPLINES

The study of the emergence processes of disciplines has become one of the preferred topics in Marxist science research during the last decade. It has always had some interaction with other trends in science research, but it has been not very strongly interrelated with them. Following an international trend (e.g. Geblewicz *et al.* [eds] 1973; Kotarbinski *et al.* [eds] 1976; Scriba 1979; for a biographical survey see Guntau 1982b, pp. 99–111), historians of science in the GDR, in collaboration with specialists from various other areas have completed an investigation programme on discipline emergence processes. Beginning with a symposium, 'Zur Herausbildung wissenschaftlicher Disziplinen', held in December 1982 at Wilhelm Pieck University, Rostock, more than fifty case studies were elaborated and prepared for publication (in 1985; cf. Fahrenbach and Köster 1983). The set of studies was divided into four groups: natural sciences, human sciences, technical or technological sciences, and social sciences. We recommended for the single independent investigations, undertaken by individual authors in different institutional contexts, a conceptual framework and a – deliberately loosely constructed – research strategy (Guntau 1982a; Laitko 1982). While our descriptive concepts were accepted or criticized, but at least perceived by the majority of authors, the various authors nevertheless differed significantly in their methodological approaches. This is a fact which reduces the value of the resulting sample of case studies for comparative analysis, but the divergence of approaches drew our attention to many features of the processes of discipline formation and confronted us with unsolved problems of adapting the categories of Marxist science research to this field of investigation.

Imre Hronszky, Márta Fehér and Balázs Dajka (eds), Scientific Knowledge Socialized, 213–223.
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Marxists have conceptualized science from the very beginning as a social phenomenon – in a period when other philosophies saw science as a cumulative result of independent individual genius or as a body of self-sufficient and self-developing ideas (Marx 1844, p. 531). During the first decades of a specialized Marxist science research, the pioneers of this area (Hessen, Bernal, Harig and others) concentrated their effort on dependencies and interactions between science on the one hand, and the productive forces and modes of production, the ideological and political superstructure, and the cultural system of society on the other. The research methods developed in the course of this research trend were mainly appropriate for detecting global relations of the above-mentioned kind. The specific social relations of knowledge production, on the contrary, for a long time remained (with a few exceptions) outside the scope of systematic investigation – state of affairs which elucidates the fact that scientific disciplines as social systems of a medium level (between science as a totality and the individual scientists and their microgroups) have been reflected in Marxist science research only at a relatively late stage. Meanwhile, they were always carefully investigated as systems of developing knowledge with their own concepts, methods, and world pictures. Scientific disciplines seem to be very suitable subjects for studies of the highly complex mediation between global characteristics of society and the processes of knowledge production including the behaviour of their actors – a mediation that forbids (apart from rare and therefore methodically valuable exceptions) any immediate and rigid correlation between the two aspects of society.

It is not intended here to sum up the results of the case study programme before their complete publication. Nevertheless, we shall try to formulate three general features of the problem as guidelines for further research. The results of the project are to be regarded as preliminary. We tend to the conclusion that – before possibly starting a new and more precise comparative study – it might be favourable to improve the conceptual base and the meth-

odological equipment by profound and extended investigations of a small number of selected discipline genesis processes, just as Guntau did in the case of geology (Guntau 1984).

I

Scientific disciplines (or specialities; here no distinction is made between the meaning of the two terms) may be understood as systems of research activities with a stable orientation towards a given field of investigation (Laitko 1978). They can expand or reduce their areas and change their boundaries with neighbouring disciplines, but there must always remain some kind of continuity with the primary field. We regard a discipline as established if at least the following three components are given:
(1) a system of research activities, the participants of which communicate completely, and exchange ideas, methods and results, and so form a demarcated community in science;
(2) a mechanism that holds this system stable over time;
(3) a set of institutions that socially warrants the functioning of that mechanism and so the persistence of the whole (Whitley 1975).

The formation of these three components represents the main content of the discipline emergence process. When they are established we may evaluate the genesis as completed and from that moment speak of the further development of the discipline on its own base. It is obviously impossible to identify the birth of a discipline with any single event in the history of science (Guntau 1984, p. 12); rather than try, it seems far more plausible to consider the emergence of a discipline as a complex and qualitatively peculiar process without sharp chronological boundaries. But if we go into details we find a tremendous variability among the single processes of discipline emergence; there is no regular step-by-step procedure but a wide range of possible sets of institutions

able to secure the existence of a discipline, sets which vary remarkably from country to country. Furthermore, in the course of discipline emergence processes we did not detect any typical differences between the four groups of sciences.

II

The beginning of a discipline genesis process may be caused by internal requirements as well as by external ones or by the coincidence of both of them. The degree of accessibility to external influences is varying, as the former Starnberg group has already demonstrated (Böhme *et al.* 1977). For instance, Scholz found in the genesis processes of chemical disciplines short but important periods, characterized by a full concentration on their own theoretical, methodical and institutional emancipation and a nearly complete lack of interaction with other areas (Scholz 1983, p. 94). Therefore the idea that science develops according to changing social goals should be complemented by the hypothesis that science can accept only certain social needs, dependent on the given conditions, and react to them by producing adequate intellectual means to satisfy them successfully. New disciplines, whether or not they are initiated by practical social needs, must always start from the body of existing knowledge and cannot be established beside it as a totally independent enterprise. All new branches of science grow out "of prior research or out of the extension of an established body of scientific and/or technical knowledge" (Mulkay in Lemaine *et al.* [eds] 1976, p. 2). This affords an effective mechanism securing the unity of science in all its variability and protecting it from the peril of dissolving into isolated pragmatic enterprises. In their well-known study, Krohn and Schäfer carefully traced the foregoing development in organic and general chemistry and in other branches of natural science as an indispensable requirement for the establishment of agricultural chemistry,

an effective scientific alternative to the concept of the so-called 'rational husbandry' (Krohn and Schäfer 1976, pp. 32–36). In a similar way, our project analyzes the relations of social requirements and the foregoing development of knowledge for the emergence of geology, psychiatry and several technical disciplines. These last, including technical mechanics, mechanical engineering, technical thermodynamics, etc. offer especially striking examples of the importance of the above-mentioned dependency; for a long time many practical skills and practical experience were available in the respective areas of industry, but they could only be incorporated into the body of scientific knowledge when some fundamental natural sciences were mature enough to analyze the sophisticated technological reality with their conceptual means (Albert *et al.* 1982; Buchheim 1980; Ivanov and Češev 1977; Simonenko 1981).

Another kind of external social influence is the incorporation of ideas and experience from social practice and general culture into the conceptual framework of the emerging discipline. The use of natural language is unavoidable before the new universe of scientific concepts is established. Natural language is a very flexible mediator of ideas between science and other spheres of society. Especially in cases where an area of pre-scientific everyday or professional experience comes under investigation, the first conceptual equipment of the new discipline often contains a lot of terms taken from practical experience and from general intellectual – mainly philosophical – culture reconstructed and adapted to the accepted scientific approach. We do not have in mind here a passive influence, but the deliberate assimilation of everyday, professional and philosophical knowledge. This aspect seems to be important – to swamp science with practical experience that actually cannot be integrated into the body of scientific knowledge would be more a hindrance than a help to scientific progress. While professional experience originates from a particular branch of social activity, philosophical ideas are a kind of general abstract from the totality

of social life, and in order to be a creative element in the system of culture it is always favourable, if not indispensable, for a scientific discipline not only to demarcate itself from, but also to connect itself with philosophy – an insight clearly formulated in an early work of F. Engels (Engels 1844, p. 551). Guntau has pointed out that the emerging geology acquired many experiences and words from the miners' language – for instance (in German) *'Gänge'*, *'Klüfte'*, *'Streichendes'*, *'Hängendes'*, *'Schärfen'* and so on; at the same time he stresses the decisive importance of the fundamental philosophical idea of evolution for the constitution of the conceptual framework of geology (Guntau 1984, pp. 73–74). Similar features are to be seen in other disciplines.

A third kind of possible external influence is correlated with the construction of empirical observational and experimental procedures for the new field. A lot of 'know how' (how to perform various procedures) comes from social practice, from craftsmanship and industry. The emergence of new specialities is often closely related to the invention of new equipment. For instance, the invention of the eye speculum by Helmholtz and its implementation by the school of A. v. Graefe were of crucial importance for the separation of ophthalmology from surgery and its subsequent development on its own foundation as a medical speciality (Fahrenbach 1978, pp. 126–129). During the last century, it became usual that advanced research technology used the most advanced methods offered by the existing level of the productive forces of society. The limiting factor for the emergence of new specialities in such cases is often the system of productive forces, as may be seen from the emergence of disciplines like semi-conductor physics or microelectronics. This is a trivial but fundamental relation by which science is integrated into the general development of society.

The three above-mentioned simple kinds of external influences combine to form very sophisticated patterns, for they are mediated by the internal social processes in the disciplinary communi-

ties, by cross-disciplinary communication (new disciplines are often born in the strong interaction of some pre-existing ones), by the perception of the importance of the growing discipline in science policy and the respective distribution of resources – manpower and funds.

The whole picture is further complicated by the fact that there exist well-defined channels of influence which have also some diffuse social impact on science, acting partially subconsciously as a kind of background effect.

III

Emerging disciplines are usually obliged to utilize the facilities afforded by their institutional environment. The use of pre-existent institutions may be more or less convenient in the first steps taken by the new inhabitant of the scientific world, but later on it is of vital importance for the young discipline to establish its own institutions. It is well known, for instance, how the foundation of the *Zeitschrift für physikalische Chemie* by Ostwald helped to concentrate disciplinary communication among physical chemists and thereby consolidate the disciplinary community, and how the establishment of Technical High Schools at the same level as the traditional universities sponsored the emergence of technical specialities. Scientific institutions are not only internal social forms of scientific activity, but also a materialized expression of the attitude of society towards science.

National differences in the institutional systems of science reflect differences in the social conditions of the development of science. It seems that disciplines usually emerge in different countries almost simultaneously or, better, that disciplines as genuine international systems of scientific activities are built up of national components that are influenced by different social conditions, traditional as well as contemporary (Dolby 1976, pp. 67–

72; Guntau 1984, pp. 37–61). It seems a rare event that a system should come to maturity in only one country, while the others have simply to imitate the model it affords.

Processes of discipline emergence become irreversible due to an extended selection process which includes the making of decisions, especially decisions about the establishment of institutions. At the frontiers of research dynamic systems of scientific activities always appear, demarcated from their environment for cognitive reasons, and existing informally as invisible colleges or formally as project organizations; and later, these systems dissolve again into their scientific surroundings. Structures of this kind are candidates for possible disciplines.

The critical point in the discrimination of disciplines and protodisciplinary transitory structures seems to be the existence of an institutionalized and, to a certain degree, standardized scientific training for students as a necessary component of every discipline, at least in the form of textbooks, lecture cycles, experimental exercises, examinations and so on. This aspect of discipline emergence is often underestimated – as it was in most of the studies in our project as well – because we are accustomed to prefer to see teaching as an activity of knowledge distribution. But it is at the same time the reproduction of disciplinary research, combined with selected results and methods of other disciplines, and it turns to act as an instrument for assimilating newcomers: so the discipline attains an internal reproduction cycle (or feedback) that warrants its stable existence over time by solving the problem of generation change in scientific manpower (Blauberg *et al.* 1978; Mirskij 1980).

In such a cycle, a double control is in action: the accepted way of training determines to a considerable extent the subsequent disciplinary research possibilities; the community of researchers tries to influence the curriculum for students in such a way that it suits as perfectly as possible their ideas about the desirable future of the discipline. The formation and institutionalization of their own re-

production cycles in an indispensable part of the emergence of disciplines, and their genesis cannot be regarded as finished until this component is included.

The poor abstract given in this paper may suffice to show that social impacts touch discipline emergence processes not only at their periphery but at their centre, and that it is impossible to draw any unequivocal demarcation line between cognitive and social aspects as they are always dialectically intertwined. It therefore might not be a very fruitful approach to conceive of discipline genesis as a genuine cognitive process influenced by a social environment. We will probably come nearer to the truth through investigating the emergence of a discipline as the formation of a new stable and self-reproducing system of scientitic activities the cognitive and social aspects of which are only abstract moments of a far more complex reality.

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TREVOR J. PINCH

THE EXTERNALIZATION OF OBSERVATION: AN EXAMPLE FROM MODERN PHYSICS

INTRODUCTION

The sociology of scientific knowledge has recently seen the emergence of several empirical studies of modern scientific practice. These studies, whether of episodes of scientific dispute[1] or of individual laboratories,[2] are characterized by their detailed accounts of the 'nuts and bolts' of scientific activity. Such work often displays the kind of sensitivity to the technical content of the science studied as is common in the history of science (indeed, some of this work has been referred to by historians as 'history of science' in all but name).[3] Philosophers of science, too, are beginning to turn their attention to detailed analyses of scientific episodes in hope of obtaining a philosophy of science more commensurate with actual scientific practice.[4] These new studies promise rich dividends in terms of our understanding of the production of scientific knowledge.

The theoretical background of the new sociological work on science has been provided by Barnes, Bloor, Collins, Mulkay, Knorr-Cetina, and Latour and Woolgar.[5] Notable are their attempts to formulate a sociology of scientific knowledge which treats the 'truth' and 'falsity' of scientific beliefs as an actors' achievement, rather than as a matter of epistemological maxim, or as an outcome produced by the voice of Nature. In this paper I attempt to provide a framework whereby detailed empirical studies, informed by this tradition, can be compared. The comparison centers upon the notion of observation in science. The study drawn upon here – that of solar-neutrino observation – is only one of four cases in modern physics which I have researched.[6] The observational pro-

Imre Hronszky, Márta Fehér and Balázs Dajka (eds), Scientific Knowledge Socialized, 225–244.
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cess in this case, however, illustrates general features typical of all the other cases.[7]

SOLAR-NEUTRINO OBSERVATION

Solar neutrinos are produced as a by-product of the nuclear fusion reactions thought to occur in the core of the sun. The detection of solar neutrinos would confirm one of the basic tenets of stellar-evolution theory – that stars derive their energy from nuclear fusion. All other observations of stars are based upon the detection of electromagnetic radiation. However, such radiation is produced at the surface while neutrinos have the advantage of being produced in the deep interior. Because neutrinos interact so little with matter (weak interaction only), they pass through the sun's outer layers and arrive at the earth eight minutes after they are produced by nuclear fusion in the core of the sun. The small amount of interaction displayed by neutrinos makes them ideal as a probe. The drawback is, they are exceedingly difficult to detect.

In 1964 it was shown that neutrinos of a rather high energy, from one branch of the pp-chain in the sun, might be detectable.[8] An experiment was constructed to try and detect these neutrinos, and in 1967 the first results were obtained. These results seemed to indicate that the expected flux of neutrinos was not present and subsequent observations over a decade have continued to indicate a discrepancy between prediction and experimental result. This disrepancy, known as 'the solar-neutrino problem', constitutes one of the outstanding problems of nuclear astrophysics today.

As a way of examining the observational situation, I will describe how solar neutrinos are observed (or not observed) in this experiment. Because neutrinos are (or, until recently, were thought to be) massless and chargeless particles which interact only via the weak interaction, an experimenter cannot in any straightforward

way 'see' solar neutrinos. The presence or absence of neutrinos can only indirectly be revealed with the aid of a sophisticated measuring instrument. In this case, the apparatus is rather bizarre: it consists of a 100,000-gallon tank of perchloroethylene (C_2Cl_4 – better known as dry-cleaning fluid), located a mile under the earth in a vacant mineshaft. The C_2Cl_4 contains an isotope of chlorine (Cl^{37}) with which neutrinos can interact. As a result of the interaction ($Cl^{37} + \nu \rightarrow Ar^{37} + e^-$) a radioactive isotope of argon, Ar^{37}, is formed. The presence of Ar^{37} in the tank can be used as evidence of the passage of neutrinos. The tank must be located deep under the earth's surface in order to shield it from cosmic rays which also trigger the reaction.[9]

As with many observations in modern physics, the entities to be observed – in this case solar neutrinos – can only be detected by their interaction with other entities. We can say that case Ar^{37} serves as a surrogate for solar neutrinos. Rather than observe solar neutrinos the experimenter looks for the presence or absence of Ar^{37} atoms. Does this therefore mean that the experimenter actually observes Ar^{37} atoms? No, in this case it does not mean this because the presence or absence of Ar^{37} is only, in turn, indicated indirectly. In practice what happens is that after a period of time (typically one month) the accumulated Ar^{37} atoms are extracted from the tank of cleaning fluid by sweeping it with helium gas (Ar^{36} or Ar^{38} is also added to the tank and acts as an argon carrier). The argon is collected on a supercooled charcoal trap and placed in a tiny Geiger counter where it decays with the emission of Auger electrons of characteristic energy. It is these electrons which the Geiger counter registers. Is it therefore the sound of Geiger-counter 'clicks' which are observed in this experiment? No, again further experimental work must be carried out. Some clicks will arise from quite other sources, for example the background radiation in the counter. In order to separate the signal from the background, sophisticated electronics and data analysis have to be employed. For instance, the energy and pulse-rise time of each

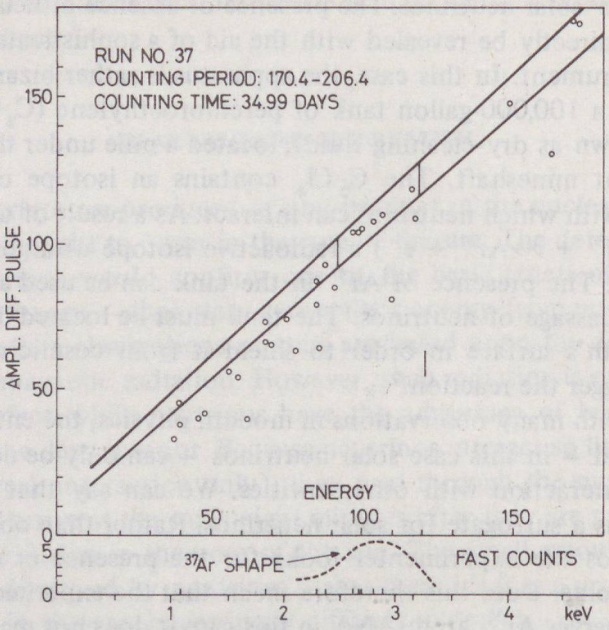

Figure 1

count are recorded and the counter is surrounded by anticoincidence devices. The 'raw data' for one experimental run are shown in Figure 1. Counts or 'splodges' within the window are deemed to be signal and those outside are deemed to be 'noise'.

It is the visual inspection of plots similar to those shown in Figure 1, which provides the information from which the number of Ar^{37} decay products detected can be derived. This in turn can be used to calculate the neutrino-induced events. Of course, the translation of graphical information into a meaningful observation of solar neutrinos is far from being a straightforward process. It depends upon a detailed knowledge of the physics of the detection process, an understanding of the working of the apparatus, and a body of statistical theory.

EXTERNALIZATION OF OBSERVATION

It can be seen that the process of observation just described is highly mediated by experimental manipulations, practices, and processes of interpretation. The 'splodges' observed (and here we are ignoring all the observational issues associated with the observing agent being able to identify 'splodges' in the first place) can be given a meaning in terms of solar neutrinos only by reference to the other stages of detection. It seems that the process of observation has become refined in such a way that what we might call 'primitive sense perceptions', or the output of an 'inscription device', forms only the last part of a chain of inference.[10] As a way of describing the minimal role given to this last stage of observation, where the observing agent actually 'sees' something, I shall refer to the process of observation in science as the 'externalization of observation'. To use a biological metaphor: it seems that, in scientific observations, our internal biological receptors have become externalized. It is as if the instruments of ordinary observation, our eyes and ears, get projected or externalized via the process of scientific observation.[11] In a very important sense the scientist's observations reside *in the apparatus* rather than in the immediate visual or audible fields.[12] Corresponding with this characteristic of scientific observation, one finds that arguments over observations center not so much on the reliability of scientists' sense perceptions, but rather on the reliability of the *practices and assumptions* that went into the observation process. What matters is not what the experimenter 'saw', but rather such things as: How carefully were the practices followed? How good were the arguments? How competent was the statistical analysis? How good were the computer programmes? and so on.

By this process of externalization, observation becomes a question of studying a chain of surrogate phenomena via a series of manipulations and interpretations, and this highlights the fundamental ambiguity concerning *just what has been observed*. For in-

stance, in the solar-neutrino example, has the experimenter observed solar neutrinos, or Ar^{37} atoms, or Ar^{37} decay products, or Geiger-counter 'clicks', or 'splodges' on a graph? This ambiguity can be reflected in the production of different observational reports of the experiment. For instance, if we look at scientific papers in which the results of the solar-neutrino experiment are given, we can find the following observational report:[13]

> The upper limit of the product of the neutrino flux and the cross-sections for all sources of neutrinos was 3×10^{-36} sec^{-1} per Cl^{37} atom.

The result, in this case, is given in terms of the number of neutrino-induced events in the target (this is the product of the fluxes of neutrinos and the cross sections for their interactions with Cl^{37}). However, other reports are possible. For instance, the following report can be found:

> The average ^{37}Ar production rate ... is 0.32 ± 0.08 ^{37}Ar atoms per day.[14]

In this case the report is of the number of Ar^{37} atoms formed rather than the number of neutrino-induced events. In some cases one can find more than one observational report given in the same paper.

In principle, one can imagine a whole range of different observational reports. For instance, the results of the experiment could be reported as: 'splodges on a graph were observed'. Alternatively they could be reported as: 'solar neutrinos were observed'. Of course the reports would also contain quantitative information, but for our purposes the numbers are irrelevant.

The difference between the possible observational reports of the experiment can be characterized by their *degree of externality*. As a way of illustrating this, let us take three hypothetical reports from the solar-neutrino experiment. We will assume the reports are as follows:

(a) Splodges on a graph were observed.
(b) Ar^{37} atoms were observed.
(c) Solar neutrinos were observed.

These reports can be said to exhibit progressively increased externality in the transition from (a) to (c), in the sense that more and more of the observational situation must be encompassed in order to produce each report. For example, report (a), which is most 'proximal' to the observing agent, refers to only a very small part of the observational process – namely, the interpretation of splodges on a graph. Report (b) is more 'distal' and encompasses more of the observational situation, since it deals in addition with the interpretation of that part of the observational process whereby Ar^{37} atoms are extracted and counted. Report (c) is even more 'distal' and includes not only the detector and associated equipment, but also the core of the sun and the intervening passage of neutrinos from the core to the detector. It may at first seem slightly odd that the sun is included as part of the observational situation, but there is nothing unusual in including sources. After all, if we were talking about experiments in which decays from atomic systems were observed we would think it odd to exclude the atom itself from the observational situation.

EVIDENTIAL CONTEXT AND SIGNIFICANCE OF OBSERVATIONS

When talking about observation in science one must always be aware that observations are usually made for some purpose, for example, to test a theory or another observation. One way of describing this is to refer to the evidential context of observations. The evidential context can include such things as a body of knowledge, a theory, law or hypothesis, a classificatory scheme or one class of objects, a set of observations or one observation. A variety

of relationships is possible. For example, perhaps the observational report is similar, different, or better (in terms of errors) than other observations; perhaps it provides evidence for the existence or non-existence of a class of objects; or perhaps it confirms or refutes a hypothesis, law or theory.

Observational reports can be said to be 'relevant' only in terms of the evidential significance that can be ascribed to them. However, there is one very interesting property associated with evidential significance. This is that *observational reports may take on significance in a variety of different evidential contexts.* This can be illustrated by reference to the solar-neutrino example. When the results of the solar-neutrino experiment first became known, the neutrino-induced event rate reported was put into two different evidential contexts. One context (and this was the primary one) was a test of the pp-chain (and the significance of the results were that they indicated the absence of the expected neutrinos). However, the results were also put in the context of a test of another cycle of nuclear reactions which can occur in stars – the CNO-cycle. The significance of the results in this context was that they definitely ruled out the CNO-cycle in the sun.[15]

The variety of evidential contexts in which observational reports can take on significance is related to the externalization of observation. It seems that the more externalized the observational report, the more the evidential context in which the observations will take on significance is specified. If we return to the hypothetical observational reports in our solar-neutrino example, then we can see that reports (a) to (c) exhibit not only increased externality, but also increased specification of the evidential context. For example, report (a) of 'splodges on a graph' could take on significance in a virtually infinite number of evidential contexts. No scientist would make such a report alone because it could mean anything. Report (b), of Ar^{37} atoms, does severely reduce the possible contexts, but it still leaves open many possibilities. For instance, one might be interested in the formation of Ar^{37} as part of a study

of the properties of Ar^{37} per se, or one might be interested in the purity of cleaning fluid with regard to Ar^{37} contamination when stored underground, or one may be interested in cosmic-ray interactions (which also produce Ar^{37}). Report (c), of solar neutrinos, makes it clear that the relevant evidential context is neutrinos produced in the sun. However, as we have seen already, although more highly specified than reports (a) and (b), there is still more than one context in which report (c) may take on significance. Indeed, if increased specificity were required, this could be obtained with yet another observational report of even greater externality. For example, the following report would suffice:

(d) The pp reactions were observed.

In this report a new observational entity, the pp-reactions, is introduced. This report is of greater externality than all the previous ones and also specifies the evidential context to a greater extent. It can be seen that the increase in externality acts as a kind of 'focussing device' – it focusses attention on one specific evidential context.

We will return to the evidential context and significance of observational reports below. It forms an important part of the analysis, especially when it comes to understanding observational disputes in science. As we shall see, such disputes can be understood in terms of attempts to change the evidential context and significance of observational reports.

THEORY-LADENNESS OF OBSERVATIONS

The problem of the theory-ladenness of observations has been extensively discussed in the philosophy of science. What I want to suggest here is that the issues raised can fruitfully be reformulated within the analysis of externality.

The attempt to distinguish between theoretical terms and observational terms within logical empiricist philosophies of science can be seen to be at the core of the problem of the theory-ladenness of observations.[16] If observational terms are theory-laden then the separation between observational and theoretical terms breaks down. An example is provided by Hanson in his discussion of Galileo's observations of 'craters' on the moon's surface. He writes:

> Galileo often studied the Moon. It is pitted with holes and discontinuities; but to say of these they are craters – to say that the lunar surface is craterous – is to infuse theoretical astronomy into one's observations ... To speak of a concavity as a crater is to commit oneself to its origin, to say that its creation was quick, violent, explosive...[17]

The term 'crater' then is not purely an observational term but has embedded in it theoretical notions. Achinstein, in an extension of Hanson's discussion, points out that the problem is that terms are used by scientists sometimes in a theoretical way and sometimes in an observational way.[18] I suggest that the analysis of externality provides a way of comparing scientists' terms which takes us beyond the sterile characterization of terms as either theoretical or observational. For instance, different observational reports on the chain of externality can be said to be 'observational' in the sense that they refer to what scientists claim to observe. They are, however, also 'theory-laden' in the sense that some assumptions have to be employed in compiling such reports. Even the most base (lowest externality) of observational reports (e.g., counting splodges on a graph) requires assumptions (e.g., concerning the separability of individual splodges).

The term 'theory-laden' is, however, not directly equivalent to the term 'externality'. As mentioned earlier, externality is defined in terms of the amount of observational situation which must be encompassed in compiling the report. The more external the report the more assumptions about the observational situation must be included. For example, when making an observational report of 'solar neutrinos', that is one of high externality, assumptions must

be made about the interaction of neutrinos with Cl^{37} (this will involve, for instance, weak-interaction theory – a highly formalized theory); about the chemical properties of argon (for instance, that argon ions will readily become free atoms) and about the counting of Ar^{37} decays (e.g. that Ar^{37} decays can unambiguously be separated from other pulses in the counter). There are many other assumptions contained within the complicated web of practice and inference which constitutes the observational situation in this case. Many such assumptions will be embodied in the craft practices of operating this particular piece of experimental equipment. For example, the tank must be cleaned in a certain way to avoid contamination. We can say that reports of high externality are heavily theory-laden, but we are then using 'theory' in a very special way. As has been emphasized, high externality reports often incorporate assumptions about the working of the apparatus at lower degrees of externality. These assumptions need not be theoretical in any traditional sense (e.g., general, systematic, formalized knowledge). For instance, the assumption that shot blasting is the best way of cleaning the tank is embedded in an observational report of 'solar neutrinos', but it constitutes an aspect more of 'craft-practice-ladenness' than theory-ladenness. The differing degrees of externality of observational reports do not correspond in any straightforward way with differing degrees of theory-ladenness when 'theory' is taken to mean systematic formalized knowledge.[19]

OBSERVATIONAL DISPUTES

The fruitfulness of the analysis developed thus far can be seen in cases where observational reports come into dispute. It is important to look at such disputes, because it is during such episodes that the taken-for-granted practices of science often come out into the open. There have now been a number of excellent sociological studies of controversies over experimental claims in science.

My intention here is not to add to these studies, but rather to recast aspects of their findings in terms of the analysis developed above.[20] What these studies have shown is that, during observational disputes, challenges to the background assumptions which have gone into the observation are made. Because the results are only as good as the assumptions used to get them, to doubt the assumptions is, in effect, to doubt the results. And, as we have seen, there are plenty of candidate assumptions which can be called into question.

Philosophers of science are more than familiar with this property of scientific observation through the Duhem–Quine thesis.[21] Since observational statements depend on a network of assumptions, the logical force of a recalcitrant observational statement can always be removed by a change elsewhere in the network. Essentially, the sociological studies describe scientists' attempts to remove the force of recalcitrant observations by challenging background assumptions in the observational process. In most scientific practice, background assumptions are routinely accepted. For example, every time a laboratory balance is used to weigh something (we are in a poorly-funded lab which cannot afford electronic balances!) we take it for granted that the air pressure acting on each side of the balance pan is the same. Although this means that all reports of weighings have such built-in assumptions, one would not expect this to be important, or even to be noted. It is taken as part of the *ceteris paribus* clause of experimental claims. It is only during a controversy over an observation that such background assumptions come into the foreground.

This process of challenging background assumptions can be illustrated by drawing once more upon the solar-neutrino case. When the solar-neutrino results were first reported, many attempts were made to see what had gone wrong with the observation – it had, after all, come out against one of the mainstay theories of astrophysics. One response to the results was to challenge the taken-for-granted assumption in the observation process that weak-inter-

action theory could be used to calculate the capture of neutrinos by Cl^{37}. Bandyopadhyay proposed a modified weak-interaction theory which indicated a much smaller cross section for this capture – thus explaining the failure to detect the expected neutrino flux.[22]

One way of understanding challenges to assumptions made in the observation process is to see such challenges as *attempts to change the evidential context of the observational report.* For example, if Bandyopadhyay's challenge to the solar-neutrino results had been sustained, then the appropriate evidential context for interpreting the results would have been weak-interaction theory rather than solar neutrinos. The results would still have been significant because they would have indicated that the standard theory of weak interactions needed to be replaced by Bandyopadhyay's own theory. However, the context would have been changed. Another response to the solar-neutrino results was to question the argon chemistry. Jacobs published a paper in *Nature* suggesting that perchloroethylene could in certain circumstances form polymers, thus trapping Ar^{37} ions in the tank and preventing them from forming neutral atoms, and hence from being extracted.[23] If Jacobs was correct, then the evidential context of the results would have been switched away from solar neutrinos to argon chemistry.[24]

Although I have mentioned only two such challenges, a variety of background assumptions in the solar-neutrino observational process have been questioned over the years.[25] In this particular case none of the challenges has been successful, and the experimenter has been able to maintain that the appropriate evidential context for the results is the testing of solar-neutrino flux predictions.

I would now like to consider what happens to the externality of reports during the course of a challenge to background assumptions. It will readily be seen that *a successful challenge has the effect of changing the externality of the observational report.* For

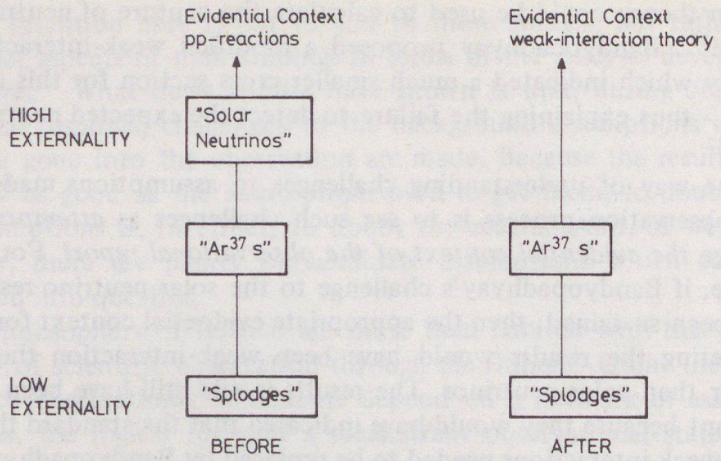

Figure 2. The potential change in externality and evidential context of Bandyopadhyay's challenge

example, in the solar-neutrino case, a successful challenge to assumptions about weak-interaction theory means that observational reports of 'solar neutrinos' can no longer be sustained. This is because it now will not be legitimate to extrapolate information from the observation of Ar^{37} atoms to draw conclusions about solar neutrinos, since, with a different weak-interaction theory, solar neutrinos cannot be expected to produce Ar^{37} atoms in this experiment. Or, to put it another way, Ar^{37} will no longer be an appropiate surrogate for solar neutrinos. However, a change to an observational report of lower externality might not raise any difficulties. For example, observational reports which refer to Ar^{37} atoms and splodges on a graph will still be valid: it is just that these cannot now be translated into reports on solar neutrinos. The chain of inference will have been broken (see Figure 2).[26]

If we look at the challenge to the argon chemistry we can see that a similar switch in the externality of the report is entailed. Because Ar^{37} atoms cannot be extracted from the tank, reports which refer to 'solar neutrinos' or 'Ar^{37} atoms' will no longer be

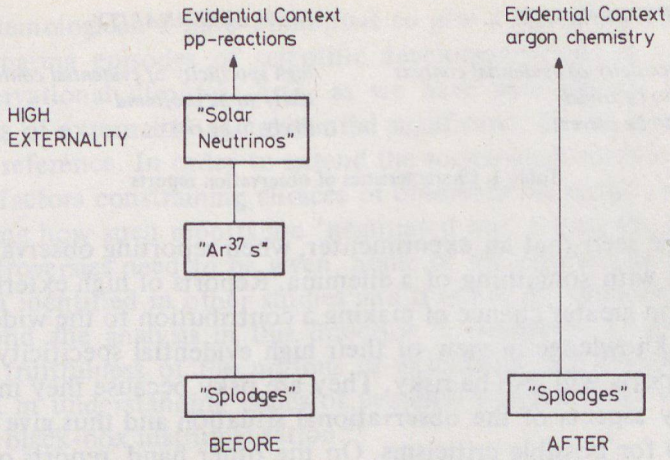

Figure 3. The potential change in externality and evidential context of Jacobs' challenge

correct. However, a report with lower externality can still be sustanied. Thus, a report of 'splodges on a graph' can still stand; but splodges cannot be translated into reports of Ar^{37} atoms or solar neutrinos. Splodges are no longer an appropriate surrogate for Ar^{37} atoms. The chain has again been broken, but in this case at one step lower externality than in the weak-interaction challenge (see Figure 3). Again, the effect of the challenge is to leave a residue of correct observational reports, but these are now at a lower level of externality. Hence, Figures 2 and 3 show that both challenges reduce the externality of observational reports and change the evidential context.

THE EXPERIMENTER'S DILEMMA

If observation is highly externalized and a number of different reports with different levels of externality are available from any single experiment, the question immediately arises as to how an experimenter decides which report to give. The characteristics of reports of high and low externality are summarized in Table 1.

LOW EXTERNALITY	*HIGH EXTERNALITY*
low specificity of evidential context	*high specificity of evidential context*
likely to be trivial	*likely to be profound*
likely to be correct	*likely to be risky*

Table 1. Characteristics of observation reports

It can be seen that an experimenter, when reporting observations, is faced with something of a dilemma. Reports of high externality will stand greater chance of making a contribution to the wide corpus of knowledge in view of their high evidential specificity, but such reports will also be risky. They are risky because they involve so many aspects of the observational situation and thus give more grounds for possible criticisms. On the other hand, reports of low externality are less risky and more likely to gain acceptability – however, they sacrifice profundity. They are relevant to so many evidential contexts that for all intents and purposes they are useless.

The dilemma for the experimenter is the choice between whether to go for a safe low-level report which stands little chance of being challenged, or to be more bold and risk a high-level report which emphasizes the profundity of the observations for some specific evidential context. Interestingly enough, these options, when recast in terms of epistemology can be seen as the traditional ones which philosophers of science have emphasized.[27] We can say that a report of low externality corresponds to maximum veracity, while one of high externality corresponds to maximum theoretical significance. There is a tension between the need for profundity and the need for veracity. The observational reports of maximum veracity are likely to be the most trivial while the most profound reports run the risk of being shown to be false.

My concern here has not been with epistemology.[28] The purpose in developing the analysis of *externality* and *evidential significance of observational reports* has been more descriptive than

epistemological. I have attempted to give a means of empirically comparing episodes of scientific development, and in particular observational disputes. And, as we have seen already, the concepts of externality and evidential significance can be given empirical reference. In order to extend the sociological analysis some of the factors constraining choices of observational report, and influencing how such reports are 'negotiated out' during the course of controversies need to be given. Some of the relevant factors have been identified in other studies and it is not my intention here to extend the analysis in this direction.[29] Elsewhere I have shown the fruitfulness of the notions of externality and evidential context in understanding issues of personal warrantability in science and black-box instrumentation.[30]

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NOTES

[1] See for example the studies by Collins, Harvey, Pickering, Pinch, and Travis collected in H. M. Collins (ed.), *Knowledge and Controversy, Social Studies of Science*, Vol. 11 (1981), 1–158.

[2] See for instance B. Latour and S. Woolgar, *Laboratory Life* (London and Beverly Hills: Sage, 1979) and K. D. Knorr-Cetina, *The Manufacture of Knowledge: An Essay on the Constructivist and Contextual Nature of Science* (Oxford: Pergamon Press, 1981).

[3] See M. Rudwick, 'Report of Meeting "New Perspectives in the History and Sociology of Scientific Knowledge"', *Society for Social Studies of Science Newsletter*, Vol. 5 (1980), 43; and S. Shapin, 'History of Science and its Sociological Reconstructions', *History of Science*, Vol. 20 (1982), 157–211.

[4] T. Nickles, 'ERISS and Internal Sociology of Science', paper presented to the Sixth Annual Meeting of the Society for Social Studies of Science (Atlanta, Georgia, 5–7 November 1981); D. Shapere, 'The Concept of Observation in Science and in Philosophy', *Philosophy of Science*, Vol. 49 (1982), 458–515; D. Gooding, 'The Making of Meaning: Experimental Realism and the Reference of Scientific Terms', paper presented to the joint meeting of the British Society for the Philosophy of Science and the British Society for the History of Science on 'The Acceptance and Rejection of Scientific Theories' (Leicester, 28–30 March 1983); and I. Hacking, *Representing and Intervening* (Cambridge: Cambridge University Press, 1983).

[5] See B. Barnes, *Scientific Knowledge and Sociological Theory* (London: Routledge and Kegan Paul, 1974); D. Bloor, *Knowledge and Social Imagery* (London: Routledge and Kegan Paul, 1976); H. M. Collins, *Changing Order* (London and Beverly Hills: Sage 1985); M. Mulkay, *Science and the Sociology of Knowledge* (London: George Allen and Unwin, 1979); K. D. Knorr-Cetina, *Op. cit.*, note 2; and B. Latour and S. Woolgar, *Op. cit.*, note 2.

[6] The other three cases researched are: measurements of solar oblateness, measurements of global solar oscillations, and the detection of high fluxes of gravitational radiation. Nearly all the scientists who have made contributions to the debate have been interviewed. For some of the results of the study see T. J. Pinch, 'Theoreticians and the Production of Experimental Anomaly: The Case of Solar Neutrinos', in K. Knorr, R. Krohn and R. Whitley (eds), *The Social Process of Scientific Investigation, Sociology of the Sciences Yearbook*, Vol. 4 (Dordrecht: Reidel, 1980), pp. 77–106; Pinch, 'The Sun-Set: On the Presentation of Certainty in Scientific Life', *Social Studies of Science*, Vol. 11 (1981), 131–158; Pinch, 'Towards an Analysis of Scientific Observation: The Externality and Evidential Significance of Observational Reports in Physics', *Social Studies of Science*, Vol. 15 (1985); Pinch, 'Theory Testing in Science – The Case of Solar Neutrinos: Do Crucial Experiments Test Theories or Theorists?', *Philosophy of the Social Sciences* (1985); Pinch, *Confronting Nature* (Dordrecht: Reidel, 1985).

[7] All the cases are, however, of astrophysical phenomena observed from earth. How far this analysis extends to other areas and other epochs is an open question. Stevan Shapin has suggested to me that similar processes can be seen at work in the debate between Boyle and Hobbes over the working of Boyle's air pump – see S. Schaffer and S. Shapin, 'Leviathan and the Air Pump: Hobbes, Boyle and the Politics of Experiment', paper presented to the joint meeting of the British Society for the Philosophy of Science and the British Society for the History of Science on 'The Acceptance and Rejection of Scientific Theories' (Leicester, 28–30 March 1983). David Gooding, drawing upon his studies of Faraday, has developed ideas which are in some ways similar to those of the present paper – see his 'Empiricism in Practice: Teleology, Economy and Observation in Faraday's Physics', *Isis*, Vol. 73 (1982), 46–67.

[8] The pp or proton-proton chain is thought to be the predominant series of nuclear reactions in the sun. The neutrinos detected in this experiment come predominantly from the reaction: $B^8 \rightarrow Be^8 + e^+ + \nu$.

[9] The experiment is located in a mineshaft in Lead, South Dakota.

[10] The importance of inscription devices in the analysis of science is emphasized by Bruno Latour: see Latour and Woolgar, *Op. cit.*, note 2, and B. Latour, 'Visualisation and Cognition: An Introduction to the Debates', paper presented to the STS Seminar (Paris, L'Ecole des Mines, 12–14 December 1983).

[11] I am grateful to Robert Cohen for pointing out the fruitfulness of this metaphor.

[12] This meshes well with Bachelard's description of observation as 'phenomenotechnique'. See G. Bachelard, *The Philosophy of No* (New York: Orion Press, 1972). For an extension of Bachelard's ideas to scientific instrumentation, see Latour and Woolgar, *Op. cit.*, note 2.

[13] R. Davis, D. S. Harmer and K. C. Hoffman, 'Search for Neutrinos from the Sun', *Physical Review Letters*, Vol. 20 (1968), 1205–1209, at 1205.

[14] R. Davis and J. Evans, 'Neutrinos from the Sun', in J. A. Eddy (ed.), *The New Solar Physics* (Boulder, Col.: Westview Press, 1978), pp. 35–58, at 43.

[15] See, for instance, J. N. Bahcall, N. A. Bahcall and G. Shaviv, 'Present Status of the Theoretical Predictions for the ^{37}Cl Experiment', *Physical Review Letters*, Vol. 20 (1968), 1209–1212.

[16] For further discussion of this problem, see R. E. Grandy (ed.), *Theories and Observation In Science* (Englewood Cliffs, NJ: Prentice Hall, 1973).

[17] N. R. Hanson, *Patterns of Discovery* (Cambridge: CUP, 1958), p. 56.

[18] P. Achinstein, *Concepts of Science: A Philosophical Analysis* (Baltimore, Md. and London: Johns Hopkins University Press, 1968), p. 181.

[19] It so happens that in the solar-neutrino case (and in the other three cases I have examined) increased externality corresponds with increased theory-ladenness. Also, increased externality seems to correspond with an increase in the number of conjectural theories. This, however, may be true only for astronomical-type observations. In general, astronomical sources are not subject to experimental control. For another discussion of degrees of theory-ladenness, see W. V. O. Quine, 'Grades of Theoreticity', in L. Foster and J. W. Swanson (eds), *Experience and Theory* (Boston, Mass.: University of Massachusetts Press, 1970), pp. 1–17.

[20] See, for example, the studies referred to in note 1.

[21] See S. G. Harding (ed.), *Can Theories Be Refuted? Essays On the Duhem–Quine Thesis* (Dordrecht: Reidel, 1976).

[22] P. Bandyopadhyay, 'Solar Neutrinos and the ^{37}Cl Neutrino Absorption Experiment', *Jornal of Physics*, A, Vol. 5 (1972), L19–L23.

[23] K. C. Jacobs, 'Chemistry of the Solar Neutrino Problem', *Nature*, Vol. 256 (1975), 560–561.

[24] In discussing these challenges, I am assuming that they produce a sufficient effect to change the context. Some challenges to background assumptions will, for instance, only have the effect of slightly changing the numerical result expressed in the observational report.

[25] Mostly it was assumptions in astrophysical theory which came under attack – in other words, the challenge was mounted at high externality, in the more conjectural theories.

[26] For a philosophical analysis of these causal observational chains, see K. F. Schaffner, 'Correspondence Rules', *Philosophy of Science*, Vol. 36 (1969), 280–290.

[27] Popper's philosophy, for instance, can be ragarded as placing the emphasis upon theoretical boldness, while inductivists and logical empiricists place greater emphasis upon the veracity of observational statements.

[28] Some epistemological remarks on this problem are made by Shapere, *Op. cit.*, note 4. He suggests that scientists should always give their 'strongest' description (maximum ex-

ternality in my terminology) and 'retreat' to other descriptions only when reason for doubt arises. The problem with this suggestion is that what counts as 'doubt' depends on the particular context of the experiment. Indeed, during experimental controversies everything can be doubted, including the competence of the experimenter. In any case, it seems that scientists do not always offer the highest externality observational reports – if this were the case, Davis would have expressed his results in terms of the pp-chain rather than a neutrino event rate.

[29] See, for instance, the studies referred to in note 1.

[30] See Pinch, 'Towards an Analysis of Scientific Observation', *Op. cit.*, note 6, and Pinch, *Confronting Nature*, *op. cit.*, note 6.

ANGUS R. H. GELLATLY

INFLUENCES ON THE CONCEPTION OF LOGIC AND MIND

INTRODUCTION

In this paper I shall bring together two sets of ideas concerned with different aspects of the relationship between logic and the mind. One set of ideas is found within recent work on cognitive psychology, or cognitive science; the other set is due to sociologists writing about logic from a relativist stance. To illustrate my argument I will make use of a number of by now quite well-known experiments on cross-cultural differences in cognition and on cognitive development. This choice is not arbitrary, for as has been recognized in the literature, the two fields of enquiry have for some time been converging onto theoretical and methodological issues of common concern (Laboratory of Comparative Human Cognition, 1979). One of those issues is the relationship between logic and mind, and the methodological problems that surround its investigation. My purpose will be to use the empirical examples to open up questions about the relationship and in particular to cast doubt upon the conception of it in which logic is held to be descriptive of mental processes.

Speculations about logic and mind have, of course, a long history, and the diversity of modern approaches to logic allows for all kinds of variations on the major positions. As a result one cannot readily provide a neat characterization of each and every view about logic and its relevance to mind. Yet for present purposes a dichotomy must be established between, on the one hand, the view of logic in which the validity of a step in reasoning is held always to be a matter of social convention and, on the other hand, those views of logic according to which the validity, or necessity,

Imre Hronszky, Márta Fehér and Balázs Dajka (eds), Scientific Knowledge Socialized, 245–263.
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of such steps is held to be determinable on the basis of self-evidently rational criteria. To put it in other terms, the distinction is between logic as a never-ending process of social invention and logic as discovery. The latter half of the distinction covers both the case where the supposed discovery is of pre-existent Platonic relationships and that where it is the discovery of remote consequences of linguistic conventions, such as exponents of the theory of analytical validity might hold. This view of logic as discovery of one sort or another is far and away the most prevalent one in our society. So much so that for many people the idea that the validity of a step in reasoning could be determined by social convention may be thoroughly repugnant. The desire to ratify our most basic achievements by reference to some set of objective standards that are external to society and ourselves is a deep one. The pervasiveness of the desire, as expressed in the discovery view of logic, is one of the influences referred to in my title. Another and related influence is the esteem in which logical thinking and abstract thought in general are held.

Because, on what may be called the standard view, logic is seen not as a product of social practices but as the achievement of individual minds, there is a temptation to take it for a blueprint of the actual workings of the mind. Once this is assumed, then formal logic itself can be called on to provide a rigorous-looking description of mental functioning. The temptation to go down this path is made all the stronger because of the added credibility that accrues to theories couched in abstract terms. The abstract tends to be considered not as one among many modes of discourse but as a distinctly basic mode. It has the power therefore to attract credibility to a theory that makes use of it.

The contention of this paper then is that conceptions of the relationship between logic and mind have been overly subject to the influence of the view of logic as discovery and description of pre-existing abstract relationships. In addition, a second influence has been the high standing of abstractly, or formally, stated theories.

RELATIVISM

In a recent exposition of relativism as it applies to logic, Barnes and Bloor (1982), writing in the context of the strong programme for the sociology of knowledge, note that an important line of argument by which rationalists oppose relativism is to assert that all cultures share a common core of true beliefs and rationally-justified patterns of inference. These beliefs and inferences supposedly are acknowledged everywhere because they rest on objective and universally recognized criteria of truth and rationality, the existence of which is held to refute relativism.

If correct, such a claim would indeed be the undoing of relativism. Barnes and Bloor, however, resist the claim. With respect to the putative rationally-justified patterns of inference, they point out that rationalists have rarely felt a need to offer empirical evidence of such universally recognized patterns. Even if such evidence were available, they continue, it would not help the rationalist case. This would be so because it can be argued that justifications of deductive inference always presuppose what they are intended to prove (e.g. Haack 1976). If, therefore, there are universally accepted inference patterns their acceptance cannot be justified rationally but only explained causally. One such form of explanation would be a nativist one: the disposition to accept the inference pattern would be a result of the way our brains are organized. But, maintain Barnes and Bloor, to explain acceptance of the pattern in biological terms is no less discomforting for the rationalist than to explain it in terms of conventional social practices. In either case acceptability is being provided with a causal explanation not a rational justification.

Although Barnes and Bloor want to block the appeal to biological organization, this is a move commonly made by rationalists at some point or other. For instance, Fodor (1975, 1980) argues that it would be impossible for an individual to develop logical powers not present at birth, so that therefore logic must be innate.

The evolution of innate dispositions to accept deductive patterns might even be seen as rationalism at the evolutionary level, where the patterns are somehow immanent in the world. In Piaget's theory, by contrast, it is the individual who in the course of his adaptation to the environment discovers the immanent logic for himself, rather than the species in the course of its adaptation. Either way, whether one opts for a nativist or an acquisitional account, logical competence is commonly explained in terms of the way the mind, or brain, operates. This belief that people are able to make valid inferences because their minds are appropriately structured can be referred to as the doctrine of mental logic. It is a doctrine that is often explicitly stated and even more frequently implicitly assumed.

Whether rationalists should be allowed to make appeal to an innate mental logic, or whether, as Barnes and Bloor argue, they should not remains undecided. Nonetheless, there is still a need to investigate the evidence pertaining to universal patterns of inference. For the relativist position to be secured, it will be necessary to demonstrate that the relevant empirical data are far from compatible with the notion of universally accepted inferences, and to explain how this ceases to be surprising once the conventional nature of logic is borne in mind (see Bloor 1983). In what follows, emphasis will be given to the view of logic as social convention. It will be demonstrated that examples of inference making are not self-evident but that their identification depends on the application of mutable criteria. The attributions of logicality on which all forms of the doctrine of mental logic rest are, it will be argued, reflections of the interests of those who make them.

MENTAL LOGIC

There is a long-standing division of opinion as to whether formal logic is descriptive of the thinking process or merely normative. It

is the first of these beliefs that is known as the doctrine of mental logic. Johnson-Laird (1983) suggests that all psychologists who have considered human beings capable of rational thought have subscribed to some or other version of the doctrine. It appears to express an idea that is readily available in the culture and which one may fall back on even unwittingly. An extreme version of it was propounded by Piaget, for whom cognitive development was essentially the child's construction for himself of a series of increasingly powerful logics. For those individuals who achieved it, the process concluded in the stage of formal operations, by which point the individual was supposed to have equipped himself with the full propositional logic. "Reasoning is nothing more than the propositional calculus itself" (Inhelder and Piaget 1958, p. 305). In the original theory this was taken to mean the ability to reason logically about any subject matter at all, since the formal-operations thinker supposedly dealt with logical relations rather than their concrete realizations. However, Piaget (1972) radically watered down this claim. He conceded that the formal-operations thinker might only reason logically in respect of matters which were of interest to him and in which he possessed some expertise.

The importance of this dilution of Piaget's position on formal operations is enormous, for it immediately raises the problem of what criteria need to be satisfied before an individual can be credited with a mental logic. If the logic only manifests itself in particular circumstances, how may those circumstances be identified? How, on any occasion, can it be known that a person is really reasoning rather than, for instance, remembering how things ought to be done? What is gained by the positing of a mental logic whose manifestations are so unreliable? As we shall see, these are crucial questions to answer. They also bear on what is the most commonly voiced objection to mental logic.

Henle (1962) remarks that the mere existence of errors in reasoning, of contradictions and fallacies, is frequently taken as conclusive evidence that logic cannot be descriptive of thinking pro-

cesses. She goes on to point out, however, that there are alternative means for coping with the problem of error. For instance, both Kant and Boole were of the opinion that errors resulted from interference with the laws of right reasoning, and that the laws were those of valid deductive inference. In contemporary jargon, this would be to postulate a competence–performance distinction where formal logic describes a model of competence. (Henle [1981] remarks that the distinction should be expanded to include cognitive processes but does not elaborate the point.)

In her discussion of errors, Henle provides examples of the kind of interference with reasoning which might give rise to them. She refers to data collected from American students who were given syllogisms, many of them difficult ones, to evaluate.

In many cases, Henle claims, the errors they make can be shown to result not from faulty reasoning but from factors such as the omission of a premiss, the slipping in of an additional premiss, or illicit conversion of premisses (e.g. restating particular premisses as universals from which the conclusion followed). In other words, subjects in these cases were reasoning validly but had set themselves to solve the wrong problems. The working of their mental logic could be seen if care was taken to reconstruct the actual course of their reasoning. Of course, this strategy is not possible for all errors since there is not always sufficient information in the subject's answer to allow reconstruction. However, Henle argues that such cases are sufficiently numerous to justify the inductive conclusion that all errors could in principle be explained in a similar fashion. We will return later to Henle's defence of mental logic and to her claim that errors in reasoning are seen not to occur if chains of reasoning are properly reconstructed. For the present, we turn to Johson-Laird's critique of the doctrine.

CRITIQUE OF MENTAL LOGIC

Johnson-Laird (1983) identifies six main problems with the doctrine of mental logic. Each is summarized below but for our purposes some are of greater interest than others.

(1) People commit errors in deductive reasoning. Attempts can be made to explain these away as Henle does, but it is difficult to assess such attempts.

(2) If there is a logic in the mind, one has to ask which logic it is. Modern logicians have formulated many different logics and, at least at present, there is no way of demonstrating which one might be represented in the mind. There seems presently no convincing reason to assume a particular one, as Piaget assumed the propositional calculus.

(3) Even if one could determine which logic was relevant, there would still be the problem of discovering its representation in the mind since any logic can be formulated in more than one way.

(4) If there is a mental logic where does it come from? Piaget gives an answer to this question but his theory has come in for heavy criticism (see below). The sole alternative currently on offer is Fodor's innatism, for which the only evidence is the weakness of its competitors.

(5) There is considerable evidence (see Johnson-Laird 1983) that the deductive reasoning of adults in our society is not immune to the content of logical problems. As Piaget was forced to recognize, there are no perfect formal-operations thinkers. But if cognitive development is not an advance towards full formal operations, what is it the development of and how are we to predict when logical competence will manifest itself?

(6) Logic tells us whether a conclusion follows validly from a set of premisses. However, any set of premisses implies an infinite number of valid conclusions and clearly people do not draw an infinite number of conclusions. The doctrine of mental logic tells us nothing about which conclusions will in fact be drawn. Johnson-

Laird (1983) formulates certain principles that could govern which inferences speakers actually make during conversation, but his formulation is independent of any requirements for a mental logic. It does, on the other hand, include Grice's (1975) maxim for discourse, according to which speakers do not state the obvious, and also a convention of not stating the trivial.

This last point, that logic cannot tell us which inferences *will* be drawn, also connects to the previously noted difficulty of knowing when logical competence will be displayed. For example, with some pairs of premisses it is very difficult to see what valid conclusion follows, as the following example of Johnson-Laird's demonstrates: *All of the bankers are athletes; None of the councillors are bankers.* Subjects given such premiss pairs tend to produce erroneous conclusions, and while it may be possible to show, *à la* Henle, that such errors can be given a rational reconstruction, this does not tell us why subjects failed to draw the valid conclusion: *Some of the athletes are not councillors*, in the first place. And since with easier premiss pairs subjects demonstrate their facility at solving syllogisms, it will not do to appeal to Piaget's revised theory that formal operations are available only for types of reasoning familiar to the subject.

We can see therefore that Johnson-Laird has raised some formidable difficulties for any version of the doctrine of mental logic. In the next section, we will consider some relevant empirical data on comparative cognition. Before moving on to examine the empirical evidence it may be helpful to recapitulate the three main points so far:

(1) Rationalists sometimes attempt to refute relativism by claiming that there is a core of universally accepted patterns of inference.

(2) This claim suggests the existence of some form of mental logic, whether innate or acquired.

(3) Henle tries to show that the notion of a mental logic can be sustained by arguing that reasoning is never faulty although it may at times operate on the wrong input.

COMPARATIVE COGNITION OF THE SYLLOGISM

Of the many comparisons that have been made between industrialized and traditional peoples, one of the most hotly debated has been that concerning whether or not the two groups share the same cognitive processes. A commitment to mental logic could be sustained in either case. An innatist like Fodor would presumably argue that people in all cultures must share the same processes of mental logic. Piaget, on the other hand, is free to suggest that traditional cultures may not promote mastery of formal operations, and that therefore there may be cultural differences in mental logic (Piaget 1966). Cross-cultural tests of many aspects of Piaget's theory have been carried out in recent years, but for present purposes I want to concentrate on studies that were not designed with the theory specifically in mind but that can be shown to have implications for any version of mental logic.

Scribner (1977) summarizes a number of experiments, starting with Luria's pioneering study of the Uzbeks, in which comparisons were made between non-literate and unschooled members of traditional societies, those with some degree of schooling, and control subjects in industrialized societies. In all cases, the subjects were required to reason about simple verbal syllogisms (e.g., *In the far north all bears are white. Novaya Zemlya is in the far north*. What colour are the bears there?). Similar results were obtained with a variety of non-literate populations. In all cases, non-literate and unschooled subjects performed at an extremely low level. By contrast, there was very little variation between schooled subjects in different cultures. As little as two or three years of schooling make for a dramatic improvement in performance, al-

though whether this is attributable to schooling per se or to the acquisition of literacy is not readily decidable.

When the performance of non-literates was examined in detail, all the kinds of errors that Henle found using more difficult syllogisms were present on a grand scale. Subjects tended to misremember premisses, to import personal and general knowledge, and to confuse quantifiers. But what distinguished them most markedly from schooled subjects was their habit of using imported knowledge to convert the problem from the hypothetical to the factual (e.g., I don't know. You should ask people who have been there and seen bears).

Scribner explains the performance of the non-literate subjects in a manner reminiscent of Henle. Casual observation of their daily life and conversation convince her that the people are capable of verbal reasoning, even involving hypotheticals. Like Henle, she claims that where it is possible to reconstruct a subject's reasoning it is always found to have been faultless. The low level of 'correct' responses she attributes to lack of familiarity with the 'genre' of language in which the syllogisms are posed. Problems of this kind require near-verbatim memorizing of premisses, attention to logical relations and disregard for content, and reasoning that is completely disembedded from real-life context and concerns. Because non-literates are not accustomed to practice their mental skills in this manner they do not show themselves to good effect, Scribner claims.

At first sight then, there may seem to be nothing here that need embarrass any version of mental logic. However, a number of points can be made.

First, the rationalist claim that all cultures share a common core of accepted patterns of inference has not been substantiated. Non-literates refuse to endorse all manner of, to our schooled minds, straightforward inferences. The fact that they spontaneously say things which we consider to be examples of the very same inference patterns does not mitigate this fact. They do not have a con-

cept of the *inference pattern* itself as valid and necessary. What has been reported is that in their thinking they sometimes take steps which we would also be inclined to take and would want to classify as conforming to particular patterns. Yet *which* steps are and are not agreeable to both groups is contingent on factors such as schooling and literacy.

These same objections could in principle have been raised against Henle's interpretation of her subjects' errors, but matters are that much more clear-cut with the cross-cultural comparison. What both Henle and Scribner do is to examine the behaviour of their subjects and to select certain aspects that they feel able to classify as logical. These can then be explained by appeal to a mental logic, while all other aspects of the behaviour are given alternative explanations or ignored altogether. Henle (1962) specifically points out that only deductive reasoning is governed by mental logic, not free association or fantasy or other kinds of thinking. Of course this makes her position impregnable since the only way of identifying examples of deductive thinking is by seeing if they conform to an acceptable inference pattern. The problem is the same as that of having formal operations that only sometimes come into play, as in the diluted form of Piaget's theory. In either case, a mental logic is attributed to the individual in order to explain behaviours whose only common feature is that they are judged to be in conformity with the logic. Where behaviour might be deemed to demonstrate illogicality it is simply dismissed as irrelevant. For example, when Henle's subjects, who certainly ought to have known better, illicitly convert premisses.

The thrust of the argument is that we attribute inferences, or logical behaviour, to other people as we judge appropriate. And what we judge to be an appropriate case will reflect our various interests and commitments. For example: Whenever I place a suitcase at the top of the stairs, my dog flattens himself on his stomach by the front door, apparently anxious not to miss out on any excursion. Has he made an inference? Does he have a mental

logic? Could his behaviour be explained on a simpler model like conditioning? The answers one gives will be determined by one's prior commitments, concerning in this case various features of human and canine cognition. The same point is made forcefully in the following example from the psychology of cognitive development.

THE DEVELOPMENT OF TRANSITIVITY

A transitive inference is one in which two pieces of information (A > B, B > C) are combined to yield a conclusion (A > C). Piaget (Inhelder and Piaget 1956) has argued that pre-operatiohal children, below the age of seven or eight, are unable to make such inferences because they lack the necessary mental logic. He demonstrated the deficiency in two ways. First, a child was presented with two sticks, A being longer than B. Then stick A was removed and B was shown together with a shorter stick C. The child was then asked how A compared with C but was typically unable to answer correctly. In the second, active, test a child was shown a tower of blocks on a table and was asked to build a second tower of the same height on the floor. A stick of the same length as the first tower from top to bottom was lying around, and the purpose of the experiment was to see if the child would make spontaneous use of it as a measuring device. Would he, in other words, construct a transitive sequence (A = B, B = C, A = C)? Or would he rely on unreliable by-eye comparisons of the heights of the towers?

While pre-operational children do usually fail both inference tasks, it could still be that they are capable of making transitive inferences under other conditions. Bryant and Trabasso (1971) argued that failure on the passive task might reflect failure to remember the premisses (A > B, B > C) rather than an inability to draw the correct conclusion. They gave children intensive training on the premisses of a series involving five sticks (A > B, B > C,

C > D, D > E). Only when the children had passed a test of memory for the premisses were they asked to make the crucial comparison between B and D, and it turned out that even four year olds performed well. Bryant and Trabasso therefore concluded that transitive inferences could be made a lot earlier than Piaget had suggested.

This conclusion did not, however, go uncriticized. Younniss and Furth (1973) objected to a number of features of Bryant and Trabasso's study. In particular, they suggested that during the training period children formed an ordered mental image of the series of sticks and read off the comparison of B and D from it. Such a procedure, they argued, involves only a 'sub-logical' inference. True *operative* transitivity, such as is called for in the tower-construction task, is not available to the pre-operational child. In other words, supporters of Piaget's theory countered the claim of Bryant and Trabasso by seeking to establish a definition of inference making that would rule out the offending case.

Trabasso (1977) responded to this attempted narrowing of the definition of inference making by trying to show that it would exclude more than was desirable. Far from denying that young children used imagery to reach their conclusions, he presented evidence that this was a typical strategy for adults also. He argued that the rules of logic do not provide an adequate model of inference making. If the inferences of the children were to be judged inadmissible, so too would have to be many apparent instances of inference making by adults. Both children and adults were said to display an 'implicit' sense of transitivity in their ability to construct an ordered mental image during the training on the premisses. Furthermore, Bryant and Kopytnyska (1976) were able to demonstrate a degree of operative transitivity even in four year olds. They showed that children who failed on the tower-construction test would sometimes make spontaneous use of a measuring stick to compare the depths of two holes drilled in blocks of wood. That is, when the temptation to make visual comparisons was excluded

some children revealed a spontaneous understanding of measurement and transitivity.

Between them, therefore, Bryant and Trabasso have argued that the restricted definition of transitive inference fails to capture the psychological reality of how inferences are made, that it excludes too much, and that anyway it fails to draw an absolute distinction between pre-operational and supposedly more advanced children.

A further defence of the Piagetian position was mounted by Breslow (1981) along two fronts. On the one hand, he tried to demonstrate that success on the Bryant and Trabasso five-stick problem can be achieved without even an implicit form of inferential thinking. While on the other hand, he argued that for a child to be credited with a true understanding of the concept of transitivity he must not only be able to solve appropriate problems but must also have a grasp of the underlying *necessity* of the operation. That is, part of Breslow's strategy has been to seek a further refinement of the notion of transitivity that will allow him to maintain the required distinction between pre-operational and more advanced children.

To date there have been no relevant reports of pre-operational children exhibiting a grasp of inferential necessity. Yet how could such tests be made? Presumably a grasp of necessity can only be unambiguously displayed through acceptance of the validity of a syllogism with abstract or factually false content. For necessity is equivalent to formal validity, and this is the only circumstance in which the validity and truth content of a syllogism are separated. However, if failure on such a test was taken as evidence that a subject lacked the relevant concept of necessity, then, as we have seen, this would have to be held true of non-literate adults also. For they do not accede to hypothetical arguments concerning abstract or unfamiliar content. We have the choice of attributing the power of inference making to both non-literates and young children or of taking the stricter line advocated by Breslow and denying it to both of them. What we decide depends on what we are

prepared to classify as an instance of inference making. For the rationalist, who asserts the existence of universally acceptable inference patterns, neither alternative offers any encouragement. If one follows Breslow's criterion, then the cross-cultural data flatly contradict the assertion. While, on the other hand, I have already argued with respect to the Scribner and Henle position that simply finding instances of what we might want to classify as inference making cannot be taken as support for the rationalist assertion.

The point is, and it seems that the debates surrounding the empirical work do not so much prove it as exemplify it, that the notion of an inference pattern is a culture-specific one arising from particular social practices and language games, or language 'genres'. We can attribute or not attribute inferences to children and nonliterates (even perhaps to dogs!). What we cannot do is force them to operate our conventions and join in our unfamiliar games. Which, in brief, is the relativist position on logic. That what constitutes a valid step in reasoning is to be determined by convention not on the basis of self-evidently rational criteria.

THE SOCIAL STANDING OF LOGIC

It is probably the case that no conclusive disproof of mental logic is possible. The best one can do is to call into question the plausibility of the idea and its utility. The present argument has been of this sort. It functions against a background of the proposal that steps in logic and their accompanying sense of compulsion are matters of social convention (Bloor 1983; Barnes and Bloor 1982). Examples have been given that demonstrate the problematic nature of attributions of logicality, or the identification of instances of inference making. Because the notions of deductive inference and deductive validity are themselves based in social convention, because in other words the terms refer to socially sustained catego-

ries, there is always a possibility of disagreement over any future application. Just as a newly discovered species of animal may be judged readily assimilable to or dangerously subversive of established taxonomic categories, so any new experimental outcome in cognitive psychology may or may not result in a perceived need to make adjustments in the way categories of logical ability are defined and interrelated. A graphic example of this process of negotiation and renegotiation occurs in the case of the work on transitive inference. It shows that inference patterns are no more to be found in nature than are phyla or species; all are examples of social categories of discrimination with which we interpret the world. For this reason, it should be noted, an experimental result that may be accepted as unproblematic at one time may become problematic if developments elsewhere force alterations to be made in the classificatory network.

The above amounts to a provocative claim both about the reality of mental logic and about the nature of logic itself. Inference patterns are not commonly thought of as cultural conventions. On the contrary, the symbolic formalism in which they receive their canonical representation tends to be regarded as somehow more fundamental than any natural language representation. It is seen as approximating more closely to some ideal, rather than as simply one more case which interested parties agree to call an instance of the pattern. Even in the theory of analytical validity, once the meanings of terms have been fixed by linguistic convention, inference schemata are supposed to carry their own necessity within them. To argue the opposite, to deny that the discipline of logic brings us into contact with something outside of ourselves, is to venture a heresy. The effort to understand logic as social convention may be interpreted as an attempt at debasement. For it is from precisely that promise of contact with something external and objective that the credibility and high esteem of abstraction and formalization derive. For example, Dasen (1974) tells us how willingly cross-cultural psychologists turned to Piaget's theory

with its talk of structural properties of the mind that could be described by models drawn from logic and mathematics. The use of such terminology served to underwrite the claim of the unformalized theory to be a culturally unbiassed account of a universal process of cognitive development. In fact it seems likely that the persisting appeal of all forms of mental logic owes something to the mystique of the very term itself. To 'explain' a piece of reasoning by reference to a set of internalized rules of logic has the ring of a move away from the more complex to the less complex. As in all cases of 'psychologism', mere redescription has the guise of simplification.

There is one last point that perhaps requires further emphasis. Anyone who is impressed with the doctrine of mental logic will want to ask how, if there is no such thing, deductive reasoning takes place at all. If inference schemata and, by implication, inference rules are only part of our way of talking about things, how can reasoning that accords with them ever come about? Several possibilities have already been suggested. Behaviour that might be judged to exhibit inference making could be a result of conditioning or of recollection, it might be based upon mental images or upon the making of analogies, or it might be generated in almost any way at all. A concrete example has been developed in detail by Johnson-Laird (1983). He reports a programme which when presented with pairs of premisses computes what, if any, valid and non-trivial conclusions can be drawn from them. The programme has no inference schemata or inference rules built into it. It operates by constructing models, or instantiations, of the premisses, then proposing conclusions which it tries to falsify by testing them against the models. Construction of models and generation of possible conclusions is guided by the principles alluded to earlier; but these principles are not rules of inference, rather, like Grice's maxim, they have the force of conventions. The important point is that the programme, given a pair of premisses, can compute what may be classified as a deduction. But it does so by means of

a whole series of varied operations, not through applications of an inbuilt logic of deduction. In this respect it resembles the young subjects of Bryant and Trabasso. Its performance also mimics that of educated adults working with premiss pairs, and Johnson-Laird proposes it as a model of actual human cognition.

Whether or not this claim can be sustained remains to be seen. It may be that in Johnson-Laird's brand of cognitive science esteem for the computer replaces esteem for logic as a principal source of cultural bias. Be that as it may, relativist ideas about the conventionalism of logic and research into cognitive psychology can be seen to complement each other in building a case against the existence of either mental logic or of a common core of rationally-justified patterns of inference.

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KATHY WILKES

'EXTERNAL' FACTORS IN THE DEVELOPMENT OF PSYCHOLOGY IN THE WEST

"Psychology has a long past but a short history"
(Ebbinghaus)

I. INTRODUCTION

The 'long past' is the past of common-sense psychology. Older than Homer, it is often as brilliant as Proust or Dostoyevsky. Its theoretical past, also thousands of years old, dates back to the philosophy of thought and action of the ancient Greeks.

Scientific psychology has the 'short history'. William James, 1867: "It seems to me that perhaps the time has come for psychology to begin to be a science."[1] More firmly, Wundt, 1874: "The work I here present to the public is an attempt to mark out a new domain of science" (1904). Wundt founded his Leipzig laboratory in 1879; in 1891 Cattell became the world's first Professor of Psychology, at Columbia. Since then:

> The oldest science in the world has suddenly expanded its factual content and the ranks of its scholars at a rate exceeding all other sciences. No other science, in no other century, has moved with such swiftness, confusion, and vigor. (Jaynes in Henle *et al.* [eds] 1973, p.x.)

The short history has been a merry one.

This history owes much to 'external' rather than 'internal' factors. Now: the internal/external distinction is a blurred one. Moreover, sciences can be influenced by internal and external considerations on at least three levels: (1) at the empirical (observational or experimental) level; (2) at the level where theories are constructed and assessed, and where methodological principles and

Imre Hronszky, Márta Fehér and Balázs Dajka (eds), Scientific Knowledge Socialized, 265–286.
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scientific norms are discussed and chosen; (3) at the metaphysical or metatheoretical level, where principles and assumptions of which the protagonists may be entirely unconscious condition theoretical endeavours. (See Lelas 1985 for elaboration of this tripartite structure.) Especially at the level of metatheory it will be difficult to judge the extent to which the implicit metaphysical presuppositions are due to internal/rational, or to external/social, factors. However, I have no space to do more than rely on an intuitive grasp of the broad outlines of the internal/external distinction; that should suffice for present purposes.

One more preliminary comment: I shall not in general discuss the numerous external factors which affect psychology *no more than* other sciences. I am concerned with influences that affect or have affected psychology disproportionately, and shall examine three of these: (a) the impact of other disciplines, (b) the role of national and school differences, and (c) the demand made of psychology that it should be *applicable*.

II. OTHER DISCIPLINES

1. *Physiology and physics: early days*

Wundt, and most other German founders of psychology (e.g., Brentano, Stumpf, G. E. Muller, Ebbinghaus, Kulpe, Kohler, Stern) held chairs of *philosophy* even when they had trained as physiologists. Ben-David and Collins[2] have analyzed the reasons for this; I need not rehearse their arguments. Briefly: in Germany almost all research was conducted in universities, and in universities run on traditional rigid departmental lines; physiology was popular but very oversubscribed, philosophy was less popular and had space; so ex-physiologists migrated to philosophy departments – but brought to their new home and their emerging science the methods, scientific values, and shapes of theory construction, of their former discipline. Their physiological background is a mani-

festly 'external' factor which helps explain much of the 'internal' content of the new science, particularly at the empirical and theoretical levels.

Emphasizing the numbers of physiologists among the founders of psychology tells but part of the story, though. It conceals a deeper influence, from the highest in the hierarchy of the sciences:

> The rejected child of drab philosophy and lowborn physiology, [psychology] has sometimes persuaded itself that actually it was the child of highborn physics. It identified with the aspirations of the physical sciences, and, consequently, acquired an idealized version of the parental image as a super-ego, especially concerning scientific morality, i.e., the 'right' way for a scientist to behave. (Watson 1973, p. 20.)

Now according to Freud, superegos can influence an individual both directly – by setting standards, prescriptions, models, norms – and indirectly, by colouring his reactions and presuppositions. So we find here. Physics not only influenced the empirical and theoretical levels of psychology, by setting the 'idealized' standard of morality, method, experimentation, and theory construction; it also provided an ontology and a metaphysics, which, without always being made explicit, supplied a metatheoretical framework that conditioned theory construction.

For such as Helmholtz, Thorndike and others, physics was unchallengeable. Its basic metaphor was the Newtonian universe of elemental particles attracting and repelling one another in strict accordance with mechanical laws – a model that had fuelled the atomistic associationism of generations of British philosopher-psychologists, and which seemed to be working excellently for chemistry. Wundt's structuralism adopted the model without question (Wundt had worked for a year [1858] with Helmholtz, whose influence over all German science was overwhelming). The psychologist's task, Wundt wrote, is threefold:

> The *first* is the analysis of composite processes; the *second* is the *demonstration of the combinations* into which the elements discovered by analysis enter; the *third* is the *in-*

vestigation of the laws that are operative in the formation of such combinations. (Wundt 1897, 1907, p. 28.)

Thus the prestige of physics fostered, on the metatheoretical level, an atomistic ontology for structuralism; and later for functionalism and even Watson's behaviourism. Nor was this all. As physics altered its ontological presuppositions, new models and metaphors became available to psychology. The new idea of the force field was one of the factors that inspired the Gestalt school – Kohler, of course, had studied with Max Planck in Berlin, and there picked up field physics.

There was another metatheoretical influence from the same source. Helmholtz had studied both physics and medicine, and had no doubt but that the latter could be grounded in the former. This reductionist attitude spread to all early psychological theories. The assumption was that physical reductionism was achievable; or, if not that, at least that physical and psychological worlds could be reduced to one, for which the laws of physics could be adopted (cf. Fechner's panpsychist monism); or, if not that, at least that there would be strong psycho-physical isomorphism, as Gestalt theory stresses. Never before or since can there have been such faith in the unity of science, a faith heavily conditioning the emerging discipline's choice of ontology, theoretical law, and methodology.

In sum, the impact of physics upon the origins of psychology is such, especially at metatheoretical and theoretical levels, that, although entertaining, "it is forbidding to speculate about what might have happened had [it] had to start its career against a background of modern quantum theory". (Oldfield 1960, p. 33.)

2. *Physics: the twentieth century*

The analogical application of ontologies, concepts, models, principles, norms and methods of popular physical theories has continued unabated in psychology. In the 20th century the most striking

single instance of the physical sciences acting as psychology's superego is seen in Watson's insistence on what he took to be the *method* – in drastically unmodified form – of natural science. (Here I exploit MacKenzie's 1977 analysis.)

However far other psychologists had gone towards adopting the methods of natural science, none had yet rejected *in toto* both the use of introspection (despite increasing dissatisfaction with it) and all 'mental' phenomena. Watson did both. It seemed – not only to Watson – that introspectionism and subjectivism had led to sterile and profitless inter-school rivalry. Moreover, both when Watson declared his revolution (in "the most important book ever written"),[3] and as behaviourism started to dominate American psychology, positivism and logical positivism were entering their heyday: claiming that the posit of unobservables was unintelligible, and/or that all scientific theories should do was provide the most economical classification of observables, and/or that whatever could not be confirmed or disconfirmed was meaningless. Operationism was all the rage, combined with stringent hypothetico-deductivism. Watson eagerly adopted these positivistic methodological trends:[4]

> The behaviorist asks: Why don't we make what we can *observe* the real field of psychology? Let us limit ourselves to things that can be observed, and formulate laws concerning only those things ... His sole object is to gather facts about behavior – verify his data – subject them both to logic and mathematics (the tools of every scientist) ... Experimental technique, the accumulation of facts by that technique, occasional tentative consolidation of these facts into a theory or an hypothesis, describe our procedure in science. Judged upon this basis, behaviorism is a true natural science. (Watson 1924, p. 5 ff.)

Behaviourism, in fact, was "a serious attempt to develop a science on methodological principles alone" (MacKenzie *Op. cit.*, p. 154). It cannot be called a *theory*: there were no shared views on such things as what was learned, nor how, nor what sorts of theoretical conceptions were appropriate. The methodological principles chosen were those of the physical sciences. Here we see the superego virtually vanquishing the identity of the new science.

3. *Applied science*

Watson expelled the mental; to assist his immensely counter-intuitive programme, he exploited the analogy of the switching circuits of telephone exchanges. Later, as 'information' replaced 'energy' as the main concern of technology, psychology seized upon a far more powerful metaphor: the computer. Again the technology came first. Servomechanisms were in use before psychology heeded them; it was rather when the military, during the war, asked psychologists to help design these machines to make them 'user friendly' that their potential significance was recognized. The significance was great. Notions like 'purpose', 'goal' and the like, blackballed by behaviourism, could be rehabilitated because of their manifestly innocent use in feedback mechanisms.

Now, as all know, the computer metaphor dominates; but it came from *outside* – an external factor having an immense impact on all three levels. Metatheoretical: the presupposition that mind and brain can indeed be understood by analogy to a computer. Theoretical: the computer model redefined the domain of psychology, supplied new ways of conceptualizing the subject matter, and offered a new methodology of research. Empirical: countless experiments are concerned to devise computer programmes to simulate human activity.[5]

4. *Opposition to the superego*

The impact of the physical sciences has not gone unchallenged, even at the beginning. Ebbinghaus, for example, criticized associationist psychology precisely for its reliance on physical and chemical analogies. James, even though he said that psychology should "begin to be a science", clearly had a non-Germanic view of 'science':

> ... what one may call a microscopic psychology has arisen in Germany, carried on by experimental methods, asking of course every moment for introspective data, but eliminating their uncertainty by operating on a large scale and taking statistical means. This

> method taxes patience to the utmost, and could hardly have arisen in a country whose natives can be *bored*. Such Germans as Weber, Fechner, Vierordt, and Wundt obviously cannot ... (James 1890, p. 192.)

James wanted science to be practical, to speak to personal concerns; we shall look at the pragmatism in American psychology shortly.

Today there are almost 40 subdivisions of the American Psychological Association; over half of all professional psychologists work in applied fields. Thus it is not clear that there is a single study called 'psychology', and the debate over which science (if any) to emulate has become far more restricted in scope. Koch, for instance, suggests that Wundt and James were wrong to claim scientific status for psychology at all:

> To persist in the use of this highly charged metaphor is to schackle these fields of study with exceedingly unrealistic expectations ... The inevitable heuristic effect is the enaction of imitation science rather than the generation of significant knowledge ... It is a deadly form of role-playing. (Koch 1974, p. 25.)

Or, equally forcefully, if it emulates physical science psychology "resembles the duckling that has imprinted on the football" (Lipsey 1974, p. 409).

Whether any of the serious branches of what Koch prefers to call 'psychological studies' will get far without the leading-strings of natural science is, however, unclear. Although this century has seen more man-hours put into psychology than were put into physics up to the time of Einstein, progress still eludes. It is important to note, too, that superegos are invariably distorted. This has certainly been the case here; the rationality and purity of physics, ascribed to it by psychologists seeking to emulate (or despairing of emulating) it, have been grossly exaggerated. Physics is now, and has been for some time, in intriguing theoretical and metatheoretical/metaphysical chaos. Philosophers of physics are turning to emphasize messy practice rather than abstract theory, phenomenolog-

ical rather than fundamental laws, the role of models and approximations, the variety of explanations and methods.[6] Arguably, the present might be precisely the *right* time for the physical sciences to supply useful methodological and theoretical leading-strings for psychology. This 'external' factor can still contribute powerfully to the 'internal' working of psychological theory.

5. *Philosophy: early days*

I have suggested that philosophy had a useful external role in Germany in the 19th century supplying academic (departmental) respectability for the new science. Otherwise, the positive impact of philosophy was relatively slight. Certainly the pragmatic philosophy of such as Dewey jibed well with the move in America towards functionalism, while Germany's phenomenological philosophy sat well with structuralism. No doubt, too, the philosophical training of philosopher-psychologists like Kohler, Wertheimer and Stern gave their work a theoretical sophistication that it might otherwise have lacked. There was also a (dubiously positive) influence at the metatheoretical level from centuries of philosophical metaphysics: "the study of the history of the behavioural sciences *is* the story of attempts to transcend Cartesian dualism, the doctrine of primary and secondary qualities, and the banishment of final causes from science" (Young 1966, p. 21). Philosophy seems to have supplied doctrines, dogmas, metaphysical presuppositions, in the framework of which the new science was semi-consciously trapped – whether the framework was Cartesian dualism or Hegelian idealism. (Another speculation, almost as unimaginable as Oldfield's fantasy of psychology starting up against a background of modern quantum theory, is of it developing against the background of *Aristotle's* picture of man and his place in nature.)

An influence from philosophy that hits the 'internal' development of psychology at the *theoretical* level can be found, but it is

a negative and obstructive one. It is illustrated in Thorndike's complaint:

> [Descartes'] physiological theories have all been sloughed off by science long ago. No one ever quotes him as an authority in morphology or physiology ... Yet his theory of the nature of the mind is still upheld by not a few, and the differences between his doctrines of imagination, memory, and of the emotions, and those of many present-day psychological books, are comparatively unimportant. (Thorndike 1898.)

Thorndike's comment could be repeated over and over again – not just about Descartes, but about other philosophers theorizing about the human mind. Predictably: for centuries philosophy had provided the only theoretical psychology that there was. Unsurprisingly, it took a long time to slough off.

6. *Philosophy: the present*

Philosophy now, too, serves rather to brake than foster scientific psychology. Whereas established sciences such as physics are treated with respect by lay philosophers, and few venture into the philosophy of physics, biology or chemistry without some scientific training, psychology has seemed a game that anyone can play. There is no 'philosophical physics', but there is allegedly a 'philosophical psychology'; no philosophical theories of velocity or momentum, but plenty of such 'theories' of action or of perception. Physics, chemistry and biology have provoked important studies of central issues in the philosophy of science; but practically none of these topics have been considered by philosophers studying the mind, even when they describe their work as 'philosophy of psychology' – yet they are vital to the science.

The problem is the 'long past' of psychology. It is brute fact that we have a sophisticated, rich and flexible folk psychology in terms of which all of us – layman and scientist alike – can explain and predict individual occasions of behaviour. There is no comparably-rich 'folk physics' or 'folk biology'. Folk psychology (the legitimate object of study for the philosophy of mind) thus

typically gets fused or confused with scientific psychology. Yet the differences in aims, methods, *explananda*, and *explanantia* are so great that the two are not even rivals. (For fuller defence of this polemical point see Wilkes 1984.) So long as philosophers fail to accept the independence of scientific from everyday psychology, philosophy will continue to misunderstand, and thus to obstruct, the progress of the science.

III. NATIONAL AND SCHOOL DIFFERENCES

All would agree that psychology is radically fragmented. "No psychologist of course *agrees* with any other psychologist, but they all have strong views about learning, reinforcement and John B. Watson" (Hebb 1974, p. 73). The discipline abounds in partial and underdetermined mini-theories, variously incompatible or incommensurable with one another. Of the many internal and external reasons for this, I consider next the fact of national and school differences. Again I shall abbreviate discussion by exploiting the article by Ben-David and Collins *(Op. cit.)*.

1. *National differences*

Wundt made Germany the capital of the empire of psychology in its early days. The list of those from the US and UK who studied under him includes almost all the great figures of the 19th century. Perhaps it is only James's work, the short-lived functionalist theory, and the behaviourism that quickly overtook functionalism, that can be called indigenous American schools; while France and Britain scarcely developed 'schools' at all.

There were great differences between German and other universities, which affected the style and nature of research. *Germany* had a long tradition of university- and department-based research, where the authority of the professor in his particular field of expertise was unchallenged, and the importance of theory em-

phasized. As we have seen, the physiological background of the early psychologists pervaded their work. In *France* much research was conducted in academic institutions, but the most prestigious of all was the College de France, with no students and hence with little opportunity to develop 'schools'. Experimental psychologists did not hail from physiology departments; in French universities physiology had room to expand. (The main psychological tradition in France was anyway towards pathological and clinical psychiatry.) Moreover, academic prestige there was not tied so closely to expertise in a specific field: France's greatest early experimental psychologist, Binet, was in the 'dilettante' tradition of Galton, dabbling effectively in anything that seized his interest. In *Britain* almost all physiological or psychological research was conducted outside the (few) universities, by those with independent means. Indeed, as late as 1949 psychology was still an unusual university course (after an unsatisfactory decade at Oxford from 1906, McDougall emigrated to the more congenial atmosphere of the US). *America* had at first no research tradition; the first graduate schools, which required research, were not founded until 1876. When research did get under way, the hierarchical German deference to professors never crossed the Atlantic, given the American pride in egalitarianism – James was said to be shockingly informal, and his distaste for the authoritarian organization at Leipzig was not his alone. Nor was theory regarded as so central. Titchener (British, but in this respect thoroughly American) once remarked: "Facts are all-important. Carry your theories lightly." Yet Titchener was one of those associated with 'a theory' – his adaptation of Wundtian structuralism kept a version of that school thriving in the US.

Indeed, the practicality that we associate with America at the time encouraged people to 'carry theories lightly'. The pragmatic philosophy of James and Dewey gave, one might say, a theoretical justification, at the metatheoretical level, to the relative neglect of pure theory. But even without this philosophical validation, prag-

matism would have dominated. We need not rehearse the social and political factors in 'the melting-pot of America' that fostered this. What seemed essential were theories that could be *used*, fitted to social, personal and political concerns: the 'is–for' of American psychology rather than the 'is' of the German. Thus functionalism was adapted – adapted *from* structuralism – to tackle problems of education, mental health, development, adjustment, individual differences. (Wundt, hearing that his former pupil Cattell was researching into individual differences, remarked *'ganz Amerikanisch'*.) And it was no accident that behaviourism took off so fast: it was a methodology that could obviously, and swiftly, be applied.

The general trend in the stabler society of Germany was indeed to study the static adult mind. (Wundt wrote: "the whole task of psychology can therefore be summed up in these two problems: (1) What are the elements of consciousness? (2) What combination do these elements undergo and what laws govern these combinations?") This was backed – as philosophical pragmatism in America backed the move to functionalism, behaviourism, and developmental and social psychology – by a phenomenological philosophical tradition which lent itself well to the methodological and contentual subjectivism that flourished steadily in Germany. Although there were exceptions on both sides (e.g., Buhler at Wurzburg, working on developmental problems, and Titchener at Cornell, keeping structuralism thriving), the simplifying 'is/is–for' distinction remains helpful.

The wars naturally exaggerated the splits. Weimer (1974) notes that Wundt was known as a staunch German patriot, which made American psychologists anxious to distance themselves from his tutelage – and which helps explain the embracing of behaviourism, that least-German of all methodologies. Émigrés were of course welcomed; so the Gestalt school, fleeing Naziism, became strongly entrenched in the US. However fewer members of the important Leipzig *'Ganzheit'*, 'holistic psychology' school (represented by Krueger, Sander, and Volkelt) emigrated; and their

work challenged much of Gestalt theory. Thus they were barely translated or known in the US – and receive only scant mention in American histories of psychology. Buhler was close to the Ganzheit school in his theoretical orientation, and, despite the interest his work should have held for developmental psychologists in the US, when he eventually fled there he was able to get a place only at a very obscure college. Perhaps he should not, in 1930 before he had to leave Germany, have refused Boring's offer of a job at Harvard: he is almost written out of Boring's history, arguably the most authoritative history we have.[7]

2. *Fragmentation*

Such schisms, between countries and between university departments in those countries, fomented the fragmentation of the discipline which we inherit today. Cross-fertilization came mainly from the physical movement of scholars (especially with the diaspora of German scholars in the war period); there was little fruitful 'internal' theoretical discussion between the various schools. The science, "unique in the extent to which its institutionalization preceded its content and its methods preceded its problems" (Koch *Op. cit.*, p. 15) has always lacked agreement about its subject matter, conceptual apparatus, and what count as 'critical experiments'. Consider the difference in theoretical constructs in its early days:

> ... we have the voluntaristic system of Wundt with its emphasis on will, the act psychology of Brentano stressing the basic importance of the intentionalism of psychical acts, the structuralistic psychology of Titchener glorifying sensation as the key to mental life, the psychoanalytic psychology of Freud elevating the unconscious wish to similar status, the hormic psychology of McDougall organized around the concept of instinct or innate propensity, the behaviouristic system of Watson with its focus on reflexes and conditioning, and still other systems based upon such varied central ideas as that of the principle of association or the concept of self or the notion of a Gestalt. (Klein 1970, p. 222 f.)

Indeed, even when the various schools *seemed* to be working within the same research tradition, with similar constructs, appearances

often deceived. The anomaly of the question of imageless thought split Leipzig/Cornell and Wurzburg; both schools employed introspection, but had different techniques – so whereas Wurzburg never found images for thought, Cornell did. Analogously, Wundt's school confirmed his tridimensional theory of feeling, other schools did not. Furthermore, theories determine the *domain* of research; so Wundt assigned to his *'Volkerpsychologie'* the higher cognitive processes, because they resisted the introspective method; more dramatically, Watsonian behaviourism excluded the mental altogether.

Thus in the astonishingly fertile period between 1900 and 1914, the main schools were unintegrated and mutually isolated. Yet those years were fruitful indeed. Gestalt, Wundtian structuralism, and the Wurzburg school were energetically thriving in Germany. Titchener kept structuralism a force to be reckoned with in the US, and functionalism in America had not yet been overtaken by behaviourism, which was starting on its meteoric path; there, too, the first textbook of social psychology was published, and research into individual differences was also getting under way. Elsewhere: Binet had invented psychological testing, Freud was publishing the first formulations of psychoanalysis, and Pavlov had discovered the conditioned reflex. Yet, as Warren (1971) notes, the *only* well-documented important cross-fertilization was the impact of Pavlov on behaviourism. And even then, as he and MacKenzie *(Op. cit.)* both argue, Watson adopted the conditioned reflex without study of Pavlovian theory. To the behaviourists – and Watson discovered the conditioned reflex belatedly (not until 1916) – it was *just* an objective method for studying behaviour: used piecemeal, and unrelated to specific problems or to any theoretical framework. Indeed, it was rash of Watson in 1916 to promote the method of conditioning so wholeheartedly, for by then it had received no empirical support except by Russian scientists, whose publications were inaccessible to Watson.

But Watson never minded criticism, because he completely ignored it. Titchener literally annihilated him in a devastating article written in 1914;[8] there is no evidence that any behaviourist bothered to read it. However, such neglect of argument and theoretical sophistication restricted the spread of behaviourism. None of the German émigrés of the late thirties had been converted to behaviourism, but were found still to be working in Gestalt theory, field theory, or psychodynamics. This is unsurprising – the Gestalt school had independently produced its own infinitely more compelling and penetrating attack on analytical introspectionism (Watson had just junked it), and the atheoretical nature of the school would have been anathema to the Germans. British psychologists, too, although less committed to theory than their German counterparts, were unimpressed by Watson when he came to lecture in 1920. Behaviourism remained an American peculiarity for a long time.

All in all, the fragmentation between countries and schools worked to preclude the sort of creative *internal* interaction for which we hope when examining the growth of a science.

Matters have not improved much since. There is the same tendency to focus on specific problems which one's theory decrees to be the important ones, and the varieties of theoretical orientation make inter-school debate virtually impossible. The new behaviourists continue to ignore criticism as light-heartedly as did Watson. Skinner, when asked about Chomsky's merciless criticism of *Verbal Behavior* (a review which he did not read for ten years, and then only because his students told him he should) and of *Beyond Freedom and Dignity* (which he has not read) remarked: "Well, if he's right, I'm wrong, and if I'm right, he's wrong, but I can view that with equanimity. It seems he can't" (Cohen 1977, p. 281). Such equanimity, though, is hard to distinguish from refusal to debate.

That is a dramatic schism. More generally, Hudson remarks of psychologists:

... they often aren't [involved in the same discipline]. The latent metaphorical systems are different, the language is different, all the assumptions are different, and what counts as a decent piece of research is different ... And, certainly, they don't have the same conceptual apparatus – they don't use the same system of ideas. (*Ibid.*, p. 168.)

IV. APPLICATION

We expect the sciences to get behind and beyond the deliverances of common sense, enabling us to do more than lay wisdom allows. Thus, meteorology provides a more accurate forecast than can 'Red sky at night ...' rules of thumb, even for the experienced sailor; and biology and botany have transformed farming. We need not rehearse applications of the physical sciences. Not unnaturally, the same has been demanded of psychology, only with greater intensity; for whereas only a few of us, most of the time, need accurate weather forecasts, all of us have to deal with other people daily. It is not *prima facie* unreasonable to ask a scientific psychology to assist us with such pressing questions as education, motivation, achievement, aggression, mental health, crime, penal reform, policing policies, marriage counselling, racism, or unemployment.

The pragmatic tenor of the American *Zeitgeist* fostered from the start in the US theories that seemed to have direct relevance to human and social problems. Part of the attraction of behaviourism was its manifest ease of application – indeed, Watson was himself one of the first to benefit, when he had to leave academic life and take to advertizing.[9] But early on, in America, psychologists of all schools were invited by business and industry to advise on advertizing, consumer reaction, personnel management, management consultancy, and the like.

This demand for applicability led to psychology finally getting institutional recognition and respectability in Britain, and (in 1941) an independent programme in German universities: when the war effort required psychologists. They were needed, as we

have seen, to help design user-friendly servomechanistic devices; but also to study problems of behaviour under stress, achievement motivation, leadership selection, small-group interaction, propaganda and morale, 'national character', etc. After the war came more problems, with the displaced, fugitives, camp survivors, traumatized or shocked or brain-injured soldiers, the bereaved and orphaned, all needing attention or treatment, and thousands of discharged soldiers wanting career guidance. Since applied psychology presupposes theory to apply, departments of psychology sprang up or expanded, and joined the mainstream of academic life.

The pressure has since intensified tenfold. Increasing numbers of students enroll in university psychology courses, often with expectations that are harshly disrupted when, if the department takes seriously the adjective 'Experimental' before the magnet term 'Psychology', they find themselves studying the Norway rat. Disenchantment with the Norway rat has encouraged the mushrooming of bastard offshoots like 'humanistic psychology', 'encounter groups', 'sensitivity groups', 'Be-Ins' *(sic)*, and the like. The sale of popularizing magazines has boomed. Doctors and nurses, lawyers, teachers, architects, policemen, town planners, business managers, probation officers, social workers, clergymen, speech therapists, personnel managers, communications engineers (the list could continue), all ask for a psychology component in their training. More than half of all professional psychologists, as noted earlier, work in applied fields. Further, the cold war has poured even more money into psychology than did the hot war; anyone who doubts this should read the incredible story documented by Schefflin and Opton (1978) of the CIA's waste of dollars in its hunt for techniques of behaviour control, 'brainwashing', conditioning, and 'truth drugs', or its research – if the dangerous and amateurish bumbling can be so called – into the effects of sensory deprivation and of drugs of all kinds.

Applied psychology *in general* needs theory to apply. (The qualification 'in general' is needed; the techniques of behaviour modification inspired by behaviourism are often more efficacious than the merits of classical behaviourism would suggest.) But there is little or no theory to apply – least of all in social psychology, where the pressure is most strongly felt. So we find the expensive and ludicrous swings of fashion from one to another school of thought in child rearing, education, penology, policing, housing, and the care of the maladjusted or depressed – again the list could continue. This is not just a philosopher's sneer about another discipline; cf. Liam Hudson:

> The discipline's health is suspect ... it has failed to produce a coherent body of scientific law; and its fruits, unmistakably, have about them an air of triviality. Attempts to justify psychological research in terms of its social utility at present lead inexorably to bathos. There is little we have produced in the last fifty years that is, in any sense of that complex word, 'relevant'. (Hudson 1972, p. 40 f.)

Apparent applicability, though, militates against the development and assessment of theory. This is – as usual! – easiest seen in behaviourism. I have noted already that Watson, and Skinner, seem untroubled by critical disagreement. Koch, sounding exasperated, writes: "In my humble opinion, behaviorism is finished. If there is residual motility, it is only that the corpse does not understand my arguments" (*Op. cit.*, p. 4). It is more probable, though, that the 'corpse' is just uninterested in the arguments:

> ... it's basically trivial and wouldn't be taken seriously by anyone if it weren't for the fact that it fills a certain role for those who are accepting the system ... you cannot find a substantive thesis that's even worth discussing, let alone refuting ... the appeal and the acceptance has to do with other matters: namely, that the system, though quite vacuous, does provide a kind of aura of acceptability for techniques of control and coercion that are very naturally sought in situations where people have to be controlled, coerced and guided. (Chomsky in Cohen *Op. cit.*, p. 97.)

It is 'natural' to seek scientific assistance for the interpersonal problems that surround us; natural again not to inquire too closely into the theoretical credentials of the assistance provided. But given the fragmentation of the field, the absence of theoretical consensus, the lack of anything resembling a Kuhnian paradigm or a Lakatosian research programme, only a dedication to extreme instrumentalism would allow one to endorse the present state of the game. And instrumentalism, whatever its viability in quantum mechanics, has yet to be defended with respect to a psychology aiming to be a science.

V. CONCLUSIONS

The applied tail is wagging the theoretical dog. While the field remains so disunited, the demand for 'relevant' research militates against the pursuit of theory that might foster greater theoretical consensus. Moreover, the demand might be mislocated: maybe for *relevance* we should be looking rather to common-sense psychology – which, with its thousands of years of development, is infinitely more sophisticated, penetrating, and profound in its own domain than is the century-old science in its domain. (Proust – or Euripides – provide better understanding of 'the whole man' than any scientist.)

Newton was happy to describe himself – however inadequate we may find the description – as one of those he called the 'dry calculators and drudges'; and he revolutionized the field. Before his time the physical world seemed no easier, more organized or homogeneous, than does the psychological domain today. The suggestion is not that 'psychology awaits its Newton'. It is rather that psychology needs to return to its original ambition of emulating the physical sciences, looking to them particularly for hints about winning greater consensus on conceptual apparatus, taxonomies of *explananda* and *explanantia*, the demands of realism and the possibilities of instrumentalism, the nature of explanation, the forma-

tion and assessment of theories. It is surely no accident that the real and solid achievements in psychology have largely come from physiological psychology, where it is evidently easier to emulate Newton's dry drudgery. Pure, rather than applied, psychology is what we need today, and that requires 'external' influences from the established natural sciences, not the fragmenting pressures of school rivalries and social demands.[10]

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NOTES

[1] See H. James (1920), p. 263.
[2] Ben-David and Collins (1966). I am grateful to David Bloor for this reference.
[3] See Woodworth (1931), p. 97. This reference is owed to MacKenzie, *Op. cit.*
[4] It should be noted that there was a mutual influence between behaviourism and the Vienna Circle. See Bergmann (1954).
[5] Later again the powerful mathematical theory of information was picked up and exploited; but this too preceded its adoption by psychologists, having been independently developed by Shannon in the Bell telephone laboratories in 1949.
[6] See, for example, the recent books by van Fraassen (1980), Cartwright (1983), and Hacking (1983).
[7] It seems to some that little has changed today: "in the United States there is an all too common dismissal of work in psychology in other countries as quaint, odd, or irrelevant. National differences, negligible in such paradigmatic sciences as physics and chemistry, assume great importance in psychology" (Watson *Op. cit.*, p. 15).
[8] Titchener (1914). I owe the reference to Weimer and Palermo (1973).
[9] There was also a highly laudable product of behaviourism's flagrant insistence upon its applicability. At just the same time as the rise of behaviourism the eugenics movement was gaining immense popularity in the US, and this was an extremely dangerous movement: it advocated immigration control and the compulsory repatriation of those from 'inferior' races, and asserted the moral and intellectual superiority of whites. Watson's famous boast that he could take any child, no matter what its racial or social background, and turn it into anything from a judge to a thief, must have come to many as a breath of fresh air and helps account for part of the popularity of behaviourism.
[10] I am grateful for the discussion of an earlier version of this paper at the Veszprém conference in August 1984, and particularly to the criticisms of David Bloor and Srdjan Lelas. I should comment that at the request of the editors the present paper has to be half its intended length; anyone who wishes to see the fuller arguments in the unexpurgated version should write asking for a copy.

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ANNE-MARIE MOULIN

NORMATIVE AND DESCRIPTIVE ISSUES IN THE ANALYSIS OF MEDICAL LANGUAGE

The other day, Robert Cohen recalled sarcastically Pearson's aphorism about natural and human laws. The first would be descriptive and the second prescriptive. I should say that scientific laws are altogether natural and human in medicine, descriptive and prescriptive; descriptive means are in fact normative: any description generally involves the reference to values external to the descriptive framework. My claim is here that these two aspects are never so exclusive of each other, and that two 'styles of thought', in various degrees, are most of the time associated. I should add that, still more generally, the styles of thought are simultaneously suggestive, on the basis of different conceptual contexts that can be successively put forth but are in some way present at the same time. The shift of viewpoint is therefore never such a revolutionary one, never an absolute breakthrough, it is only a different way of focussing some aspects of phenomena. I am not advocating ambiguity but complexity of meanings. If description must be 'thick' (in the sense Y. Elkana used the term), it is surely interwoven with non-descriptive elements, namely values.

I shall try to analyze here an example drawn from the field in which I am currently working, history of medicine and more especially, the history of medical immunology. It has been a big debate for philosophers of science to know whether medical concepts were descriptive or normative. Mere descriptivism was at a loss to explain the phenomenon of disease, and reference to values was, on the other hand, cumbersome and often confusing. That is why such themes as disease, illness and sickness have been recently nicely differentiated to offer different criteria and a wide spec-

Imre Hronszky, Márta Fehér and Balázs Dajka (eds), Scientific Knowledge Socialized, 287–296.
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trum of denominations to describe and investigate various classes of pathological phenomena.

One alternative was to distinguish the biological as the descriptive, the medical as the normative (and secondarily prescriptive). Blood circulation is a biological phenomenon when it refers to blood pressure, myocardial irrigation, nodal tissue activation and so on. It is a medical phenomenon if it is connected with boundary limits such as running, climbing stairs, coping with stress, and so on, when it is connected with having or not having pain in the chest, dysponea, etc. This is very close to opposing biological and clinical languages. But this distinction is easily blurred. Clinical language always refers to what we know or think about coronary circulation, or about hemodynamics understood through cardiac catheterization, and many other items. It appears to be more realistic to consider that several languages always coexist, where descriptive and evaluative elements are intertwined. (I prefer to avoid the term 'level' which provides too much laxity and tears apart what should remain united.) These languages are located along a continuous line, an imaginary line of course, between two poles, perception and action. At one end, an ideal language would describe the human body as a coherent system of actions and reactions, possibly in mechanistic terms, all elements of which would be described with the minimum cost of primary laws and definitions. At the opposite end, another language would focus on the inadequacies and defects, even the 'holes' of the system, to know how to improve or induce health. The second language would be exquisitely sensitive to the shifts of meaning and redundancies in the system, which elude the first approach.

That nexus of both languages explains why many scientists take – for various reasons – different meanings for the same words for granted, or reuse old concepts with fresh definitions. Words are trapped in numerous and various conceptual networks. But how can we define the alarm rates and the critical levels? Now my example.

FROM ONE SYSTEM TO ANOTHER

Bacteriology supplied medicine, at the end of the 19th century, with the triumphant model of the theory of germ disease. The model held very strongly for three generations and was the leading one for investigations in medicine until the Second World War. Immunology has now succeeded and sometimes superseded bacteriology as one of the main biological trends in medical research according to several criteria like money invested in grants, number of people working in the field, and so on. What does all this mean?

Bacteriology first emphasized the importance of 'bacteria' as the cause of disease. Immunology actually emphasizes the opposite view, that a failure of the organism is always at the root of disease. Nevertheless it is obvious that bacteriological and immunological knowledge coexisted in some obscure way and that phenomena acknowledged as bacteriological (bacterial agglutination, for instance, bacteria versus antibody) might have been labelled immunological as well (antibody against bacteria)! It does not mean that things have evolved, only the values available for their description have changed. (It is exactly the same if *angina pectoris* is viewed from the viewpoint of human comfort.)

When bacteriology began, it seemed as if any disease could be soon explained in terms of bacteria, inside the framework of the so-called Koch postulates. It was a time where clinical medicine and laboratory knowledge came to a crossroads and struggled together for supremacy over medical language. By the end of the 19th century clinical medicine had begun to exhaust its resources and it was the turn of the lab to suggest novelties to improve diagnosis and prognosis. Still it supposed that laboratory language was adequate to describe new pathological phenomena viewed in a new way. Clinical medicine had once overcome numerous drawbacks, and physicians had progressively accustomed themselves to descriptions of disease founded broadly on functional symptoms and anatomopathological data that allowed checking on the former.

Accordingly, laboratory work suggested a new language. First it suggested a shift of size. Important events took place on the slide, in a two-dimensional space and on a microscopic scale, and no longer in the three-dimensional body. The crucial question was: was anything liable to be translated into the new language? Some disorders seemed to be easily amenable. Intra-vascular thrombosis was due to blood cell agglutination, for instance. But it was hardly a specific statement. Many doubts remained as to the adequacy of the new language and delayed the widespread use of such neologisms. What exactly did bacterial serology mean? Which reality do these aggregates of bacteria indicate? Does it trivially mean that people are sick, which is a truism? Or does it mean something more specific, that it is a type of disease that might have escaped the most clever physician had he lacked laboratory tools? Does it mean more, that medicine is victoriously waging war on bacteria? Or is it of no consequence for the issue of the conflict? What about serology positive non-sick people ('healthy carriers')? What about long-lasting positive serologies long after recovery?

The big success of bacteriology seems to be due to the substitution of a dry description of a causal mechanism, bacterial damage, for an everflowing description of symptoms. But this new descriptive language, however fascinating it might be, was in fact full of obscurities and misunderstandings.

The choice of bacteria as the main agent of disease was widely inspired by the fact that bacterial cultures were currently available and therefore liable to be modified through the action of many reagents, most of all chemical agents. The advent of vaccination, from this viewpoint, was important in concentrating on bacteria and the modified strain, and not on the host's reactions (clearly non-descriptive reasons). The immunological point of view did not prevail because emphasizing the importance of the host's reactions was to emphasize the variability of reactions (i.e. idiosyncrasy), something it was utterly impossible to control. An immunological interpretation would have impaired the programmatic value of

bacteriological language and made unlikely the presupposed efficiency of therapeutic procedures like serotherapy. A descriptive system can only hold on if it has some 'positive' values, if it involves pleasant expectations. Immunology has become more important now that the host himself is considered as potentially transformable. Bacteriology once monopolized all the possibilities of systematization in the system of medical knowledge. Immunology is now competing. But in so doing, it takes advantage of concepts and denominations which early biologists would have rejected.

The switch was a switch of functions. As in a kaleidoscope, some aspects were stressed that had previously been forgotten, but had all the same been parts of the whole. Rationality switched to new trends and functions.

THE EXAMPLE OF RECEPTORS

From this point of view, reductionism is never so integral as it claims to be. A fair example is the notion of receptor that was used by the biologist Paul Ehrlich in a strong reductionist way. Ehrlich's theory was the following. All cells are endowed with receptors still called lateral chains (German *Seiteketten*) and these chains are able to fix specifically food, drugs, poisons and all kinds of metabolic products. On first analysis, it meant that any biological phenomena including disease (disfunctions or toxic damage) could be explained on the basis of chemical interactions, chemical molecules interacting together with the nice precision of 'lock and key', a metaphor by the way that is still flourishing in biology. It meant that receptors could act with the exquisite specificity of chemical molecules (base plus acid, salt plus acid ...). This type of approach was at the same time goal-oriented, since it could justify the mode of action of experimental drugs in the treatment of diseases.

Paul Ehrlich himself called attention to some other meanings far exceeding the strictly reductionist one. He admitted that he did not know exactly how many receptors there were on the cell wall, what they were and where they were. He nevertheless urged the necessity of establishing the repertoire of receptors, without giving any methodological clue as to the way of proceeding, but insisting that some of these receptors must have the function of connecting other receptors and conferring unity on the organism.

Far from attacking him as a reductionist, his adversaries attacked Ehrlich, the father of immunochemistry, on the grounds that he forged new receptors when he badly needed them, and that these receptors were nothing but pure fiction, as floating and immaterial as metaphysical entities. They suggested alternative approaches like concentrating on physical or biological phenomena, approaches which could avail themselves of physical laws concerned with electric charges, the ionic nature of molecules, and so on. These laws were supposed to be applicable not only to restricted local phenomena occurring in human or animal bodies, but to whole living organisms. It was the time of Loeb's experiments at the Rockefeller Institute, the study of the experimental conditions of cellular life: temperature, salinity, p_H and so on. That was another sort of reductionism, a physical one, one might say, and maybe reductionisms are hierarchically ordered, the physical one being above the chemical. But that is not my point. I should like to indicate that none of these supposed reductionist approaches was *jusqu'au boutiste*, and the physical approach leaned on such arguments as the importance of considering the whole system, and not particular or local reactions.

My claim now is that no so-called style of thought is exclusive of the others. The style of one type of approach is often emphasized by means of rhetoric, and for programmatic purposes. This would-be descriptive approach is always prescriptive and normative, due to the fact that any discourse (and especially medical discourse) is bound to enunciate clearly what should be done.

This explains why those categories assumed to be strongly reductionist, like the modern concept of *receptor*, for instance, are nevertheless liable to be used in various contexts. And it is not unusual that they are utilized by some authors in a non-reductionist way. A fair example is still the concept of receptor. After Ehrlich's death, this concept was sufficiently alive to cope with new discoveries about blood groups long before immunochemical developments, and helped to establish a futurist classification of humans according to serological criteria. Broadly, it provided the main outline of the theoretical framework of blood group immunology until the fifties.

In the next decades, receptors, which had just begun to be interpreted in the context of the new stereochemistry, were once more called to contribute when immunologists, at odds with the too numerous cell lines involved in immunity, tried to sketch a global picture of what they called the immune system. Receptors, whatever their correct chemical denotation might be, were once more utilized as an *operator of intelligibility*, and the more eagerly because they were supposed to be neutral descriptive categories. The 'immune system' was described as a web of receptors, each of them interacting with other receptors endowed with complementary structures and vice versa. The immune system was described as a *functional* network and this idea afforded the new system far-reaching powers of self-regulation and fair adaptation. The 'receptor' idea had been loose enough (or polytypic?) to authorize theoretical leaps, so that a new immunology could emerge, without having to dismiss all the old fads – thanks to re-employed material. This has something to do with the tricks of architecture – using the former builders' foundation stones – or with the technology of palimpsest.

In the same vein, it appears to me to be illegitimate to consider some sciences, like physiology, to be mostly descriptive, and some, like medicine, to be inescapably normative or prescriptive. There are no pure descriptive or normative concepts. There are descrip-

tions oriented towards different values, like well-being in medicine, selective consequences in theories of evolution, and so on. The choice of these values orientates the discourse and eventually permits some practical applications. One of these values may be the importance of understanding phenomena as an integrated whole, the 'holistic' claim that is never completely put aside, even in overtly restricted local research. Referring to these values, some fields of course may cross or overlap. This is the case with the recent predominant importance granted to the 'individual' in biology (leucocyte groups, graft rejection, genetic polymorphism, etc.). Biology often meets medicine in this respect, but they do not coincide; some of their values coincide, but never all of them. There is no full-fledged parallelism either.

The supposedly merely descriptive category drawn from hard sciences (e.g. receptor) derives part of its magical power of persuasion from the fact that it integrates the explanation in a general and more loose-meaning network of concepts. In that meaning, such categories can be called historical; they function thanks to their historical scientific background. Concepts not only designate things which they stand for, but involve the whole scientific field of which they are a part.

This 'effect of complementarity' explains that the same scientist can be applauded as a rigorous scholar and deprecated as a quack at the same time. There are numerous scientists with regard to whom contemporaries were hard put to decide whether they were geniuses or fools. And possibly they were both. I do not intend to give a genuine detailed case study here, only to suggest that any number of case studies, if carefully and thoroughly investigated, would reveal a jumble of meanings.

LATENCY AND POTENTIALITIES

To put to rest the claim that the reductionist style is a thoroughly coherent and progressive one, I should add, as a conclusion, that science has daily something to do with reductionist terms that were first programmatic terms and have become secondarily materialized. A well-known example is the gene, whose material nature was denied long before the discovery of chromosomes and nuclear acids. Antibodies are another example: their nature was debated for two generations, until it was finally proved that they *were* gamma-globulins. Materialization deprives these terms of a part of their potential meaning. Some prominent functions have, thus, to be attached to other terms whose boundaries are not so strict, ones that offer new opportunities for understanding regulations and overall anomalies. In close proximity to the honourable terms dignified by a long development, we find some badly determined entities characterized by suffixes: '-ioid', '-like', or prefixes such as 'pseudo-': pseudo-genes, immunoglobulin-like... Functions are as important as substances. To get back to my former example, the idea (by no means experimental) of the immune system was put forth only when immunochemistry was far advanced, when many immunological substances had been chemically identified, so that one could fear that the whole field was crowded with compact material things hindering any new insight into phenomena. In fact, there followed an outburst of theoretical views and general models.

Does all this suggest a kaleidoscopic image of scientific development? To do so would be to confer too much dignity to the Gestalt paradigm. But the kaleidoscopic comparison suggests more than the Kuhnian shift to a new vision. It suggests the importance of the latent meanings and potentialities of a system, these latent meanings interacting with the manifest. How can one scientific worker shift from one picture to another? This matter is crucial to understanding and improving the shortcomings of experimental re-

search. The main drawback in contemporary biology is repetition and dull reproduction. The social control, the budget lines that are so often mentioned lead to the adoption of a strategy of experiments that are carefully planned and should obtain expected and clear-cut results, in conformity with the programme of grant applications. One of the major difficulties lies in planning exciting experiments for which the findings are altogether unexpected and promising. There is a risk, on the other hand that, if an experiment is interesting, it is often not immediately reproducible.

This is why styles of thought, if they were absolute, would be redundant. What once made them successful would make them faulty. But history is there to show that there never was such absolutism, and epistemology confirms it.

C.N.R.S., Paris

PART II

SCIENTIFIC CHANGE, RATIONALITY, INCOMMENSURABILITY

CRAIG DILWORTH

THE GESTALT MODEL OF SCIENTIFIC PROGRESS

The Gestalt Model takes its beginning from the idea that scientific theories are not entities which are either true or false – entities intended to provide us with knowledge about reality – but that they are conceptual schemes or frameworks which are more or less applicable, and which are intended rather to provide us with an understanding of reality. On this view, theories are to constitute a background against which one can make sense of the world, and are not simply to provide factual claims describing a world which we take in all essential respects to be unproblematic.

The basic structure of this conception of science – the Perspectivist conception – was in fact formed independently of the Gestalt Model. On the Perspectivist view theories are conceived to be *conceptual perspectives*, which are analyzable in terms of five fundamental notions. These are: (i) intended domain, (ii) logical simultaneity, (iii) perspectival incompatibility, (iv) sameness of category, and (v) relative acceptability. Thus, in terms of these notions, conceptual perspectives are to be such that, when they involve differing predicates from the same category, they cannot be applied logically simultaneously to the same intended domain without conflict ensuing. When this occurs, it is also to be in certain cases possible to obtain evidence for judging which of the perspectives is in fact better applicable to their common domain. Thus this conception is intended not only to provide an account of theory conflict, but, among other things, also to afford an interpretation of Kuhn's and Feyerabend's notions of incommensurability (here: difference of perspective) which explicitly avoids the relativism to which many believed their views to lead.

Imre Hronszky, Márta Fehér and Balázs Dajka (eds), Scientific Knowledge Socialized, 299–311.
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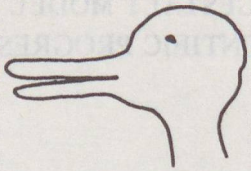

Figure

The role of the Gestalt Model has been to introduce the Perspectivist conception in such a way as to provide it with an intuitive basis. Gestalt switch phenomena are particularly suitable for this task, for they exhibit features corresponding to all of the basic notions in the Perspectivist view of theories. At the same time however, they are disanalogous in certain respects, most particularly in their being perceptual psychological phenomena, where scientific theories seen as conceptual perspectives are not ways of perceiving, and involve psychological states only incidentally. Also, in being relatively simple, Gestalt phenomena do not afford the possibilities for elaboration that are available once one moves over to the more abstract core of the Perspectivist view.

In the above duck–rabbit figure, which has earlier functioned as the Gestalt Model (cf. Dilworth 1978 and 1981), the basic notions of the Perspectivist conception are reflected as follows. The notion of distinct conceptual perspectives having the same *intended domain* is manifest in the idea of there being a duck concept and a rabbit concept each of which is to apply to one and the same duck–rabbit figure. *Logical simultaneity* is captured in the fact that both concepts cannot be applied at the same time by the same person; and this incompatibility is in turn the counterpart of the *perspectival incompatibility* of conflicting scientific theories seen as conceptual perspectives. It may be seen that such incompatibility would not arise here were one to consider applying, say, the concepts 'duck' and 'red' to the figure, but requires that the concepts applied be similar in an important respect. This may be

expressed by saying that, in order for conflict to ensue, the concepts involved must be of the *same category*. And finally, it is meaningful to speak of either the duck or rabbit concept as being the better applicable to the figure, in which case e.g. the notch on the figure's right-hand side might constitute a decisive factor in favour of the better applicability of the rabbit concept (in which the notch represents the rabbit's mouth). We might then say that the *relative acceptability* of the rabbit concept is greater than that of the duck concept, and that its adoption would constitute an instance of epistemological progress.

It should perhaps be noted that the above presentation of the Gestalt Model is not intended to afford an explanation of Gestalt switch phenomena themselves, but to introduce certain notions which are subsequently to be employed in an explanation of theory change in science. Thus, for example, when two Gestalt concepts are suggested to be of the same category, the term 'category' is only being used for lack of a better word, and no claim is being made as to whether our inability to see the figure under both aspects simultaneously is a matter of conceptual necessity or of mere psychological fact.

An important aspect of the model, which has not been emphasized before, is the way in which it allows us to conceive of two fundamentally disparate concepts' having one and the same domain of application. On extensionalist views, such as those which take their basis either in first-order logic or intuitive set theory, there is no natural way of conceiving how different predicates or concepts from the same category might have the same referent or intended domain. In the case of approaches based on first-order logic, this contributes to what has been called the problem of meaning variance; and in the case of intuitive set theory, it leads directly to the problem of delineating the intended domain. (These specific issues are discussed in some detail in Dilworth 1981 and 1982, respectively.) These problems, which are really different manifestations of the same problem, ultimately lead to

relativism, for if conceptually disparate theories have different domains of application, it makes little sense to try to compare them to see which is the superior.

In the present approach, on the other hand, the idea of there being a shared intended domain plays an integral role. With the aid of the Gestalt Model, a natural means is provided for conceiving how two conceptually distinct theories, which might well involve the same terms with different meanings, can nevertheless be applied to one and the same state of affairs. In this way it becomes meaningful to speak of the comparison of such theories as regards how well they respectively take account of their common domain. Relativism can then be avoided in principle through the positing of a rational process of adjudication; and the subsequent transition from one theory to its successor as a consequence of such a process may be viewed as constituting an instance of scientific progress.

While the Gestalt Model is thus capable of providing an intuitive understanding of such fundamental notions of the Perspectivist conception as 'intended domain', the further development of the Perspectivist view has gone beyond what can be directly represented by the model. Where the different 'perspectives' in the Gestalt switch case involve relatively simple concepts like 'duck' and 'rabbit', in the case of scientific theories what are involved are rather *idealized models*. The relation between such models, in application, should thus be characterizable in terms of the five basic notions introduced via the Gestalt Model. But in being more complex, conceptual perspectives conceived as applied idealized models, allow of an elaboration of the Perspectivist view which goes beyond the simpler case.

A first step in such an elaboration consists in conceiving of the 'categories' manifest in the Gestalt Model as being quantifiable – that is, as being *parameters*. By seeing them in this way, one can retain the structure of the simpler case, and at the same time both speak of *measurements* as playing a role in determining the appli-

cability of the perspective(s) in question, and describe perspectives themselves partly in terms of *mathematical equations.* This latter move allows one then to go on to depict the relation between certain perspectives as being that of *reduction* in the case where the equations of one perspective give results identical to those of another when certain parameters occurring in the former are given limiting values.

Due to the fact that this elaboration does seem to go beyond what can be characterized in terms of the Gestalt Model, and in order to show how the whole of the Perspectivist view developed thus far was to apply to actual science, it was presented in *Scientific Progress* in the context of a case study. The case chosen was from the kinetic theory of gases, and involved the application of the ideal gas model and van der Waals's model to the gas laws.

As should be clear from the above, what was intended here was not that features of Gestalt switch phenomena should manifest themselves in the example but that all of the hitherto developed notions of the Perspectivist view be applicable to it. As it turns out, however, some Gestalt features – mainly of a psychological nature – which are not connected to the basis of the Perspectivist view are in fact in keeping with what transpires in certain scientific cases. Such features are often linked to psychological and sociological aspects of science, such as the ability of an individual scientist mentally to switch back and forth between competing theories, and the role of argumentation in theory change, as will be treated below. But the investigation of the extent to which such features of science can be captured on the Gestalt Model lies outside the 'intended domain' of the Perspectivist view, and does not directly affect it.

What does affect the Perspectivist view here, however, is whether its basic notions are in fact applicable to the example. In this regard, it has been suggested by others that there is no conflict in the simultaneous application of the ideal gas model and that of

van der Waals, and that as a consequence the notion of perspectival incompatibility is not applicable to this case.

It seems that the motivation for this suggestion lies in the fact that van der Waals's model is developed from the ideal gas model, and both models characterize gases in essentially the same way, namely, as consisting of multitudes of tiny particles in rapid motion. But a closer look at the characterizations of the two models reveals that they in fact exclude one another. On the ideal gas model the particles or molecules are taken to have zero volume, while on van der Waals's model they have some positive volume (to account for the resultant volume of the liquid when the gas undergoes a change of state). Also, where in the case of the ideal gas model there are no attractive forces operating between the molecules, in the van der Waals case there is such a force, which binds the molecules together when they pass sufficiently close to one another at a sufficiently low speed. The effect of either of these two differences of characterization is that one cannot apply both models simultaneously to the same gas – one cannot depict one and the same gas as consisting of molecules which both do and do not have negligible volume, or which both do and do not experience an attractive force with respect to one another. In other words, the two models, in application, are perspectivally incompatible.

It is important in considering the Perspectivist conception to distinguish this sort of incompatibility – perspectival incompatibility – from the incompatibility of the *results* which may be suggested by competing perspectives. In the context of the Perspectivist view, it is in principle possible for conceptually distinct theories to be perspectivally incompatible while suggesting precisely the same results, though this sate of affairs seldom obtains in actual science.

In the case of the ideal gas model and that of van der Waals, both sorts of incompatibility are manifest. Not only are these models perspectivally incompatible in application, due to the dif-

ferent ways in which they characterize gases themselves, but they are also incompatible as regards the experimental results they respectively suggest. According to the ideal gas model, gases should obey Boyle's law; according to van der Waals's model, they should obey van der Waals's law. And both laws, though approximately similar, cannot be correct of the same gases.

These two sorts of incompatibility are both reflected in the Gestalt Model, the latter coming under the notion of relative acceptability. On the model as it has been presented in earlier work, the relative acceptability of competing perspectives may be judged in terms of their relative scope, accuracy and simplicity.

The *scope* of a perspective consists of those states of affairs to which it more or less applies. This may be understood in the context of the duck–rabbit by imagining the drawing to be one of a whole body which, let us say, clearly resembles a duck more than a rabbit. In this case we should consider the scope of the duck concept to be greater than that of the rabbit concept, for it can account for the whole of the larger figure, whereas the rabbit concept only has application to the figure's head. More complicated examples can also be imagined where, e.g., what are first seen as two distinct figures merge into one. (This example, and its converse, were suggested in correspondence by Dr. H. Brown.) In such a case we could say that the scope of the concept applied to the larger figure is broader than the scopes of the concepts applied to its parts.

In the context of the kinetic theory, the scope of the ideal gas model includes the behaviour of gases at non-critical pressures and temperatures. The scope of van der Waals's model, on the other hand, is broader. Not only does it apply to those states of affairs to which the ideal gas model is more or less applicable, but it can also be used to account for gases' undergoing changes of state, which lies quite beyond what is possible on the ideal gas model.

Accuracy is similar to scope, and in certain cases the difference between them may be a matter of degree. Since a perspective need

not apply exactly to a state of affairs in order for that state of affairs to fall within its scope, two perspectives may have the same scope in spite of their differing in their characterizations of the outcomes of identical operations. This would provide a clear instance of the second sort of incompatibility treated above, that is, incompatibility of results. This is reflected in the Gestalt Model by the notch in the duck–rabbit being a 'result' of the rabbit concept but not of the duck concept. Though both concepts are roughly applicable to the whole of the duck–rabbit, for the sake of illustration we might say that the rabbit concept is nevertheless the more accurate.

This notion too is applicable to the kinetic theory case. We may say that van der Waals's model, in application, is more accurate than the ideal gas model in those situations falling under the scopes of both of them. Not only does van der Waals's model have the broader scope, but it suggests more accurate results than does the ideal gas model when applied to gases at non-critical pressures and temperatures.

The notion of *simplicity* can be captured on the Gestalt Model by considering the supporter of the duck concept as claiming, in order to save appearances on his view, that the duck-rabbit figure is nevertheless that of a duck – one which has been struck in the back of the head! In these circumstances we should say that the rabbit concept is simpler than the duck concept, in that it does not require this sort of sophistication in order to portray the figure accurately. We note, too, that the duck concept can be made just as accurate as the rabbit concept, but only with a concomitant loss of simplicity, and that the notion of simplicity presented here is to be conceived as varying inversely to ad hocness.

As in the presentation of the main concepts of the Perspectivist conception via the Gestalt Model, the intention here has only been to give an intuitive idea of the role these notions ultimately are to play. In the context of the Perspectivist conception itself, these notions may become more refined – 'accuracy', for instance, is

there treated in terms of the measurement of parameters related in mathematical equations.

It might also be noted that the notions of scope, accuracy and simplicity, even in their more developed form, are by no means intended to provide an algorithm for theory choice. They are intended rather to indicate important criteria in terms of which theory choice may be made. Different researchers may give unequal weight to these criteria, and it may even be the case that they do not agree on how certain of the notions are to be applied in certain cases. For example, one researcher may consider a particular emendation to be clearly ad hoc, while another thinks otherwise; or one may believe the scope of a theory to be strictly limited, while the other believes that further research could result in its being broadened. In this way, argumentation might arise. But the main point being suggested here is that the reasoning behind the ultimate choice of a theory, whether or not it involves actual argument, may to a large extent be understood in terms of some combination of the present notions of relative scope, accuracy and simplicity.

The question of argumentation in the case of theory choice is closely connected to the distinction drawn here between, on the one hand, being able to shift from seeing the Gestalt Model under one aspect to seeing it under another, and, on the other hand, judging one of the two aspects to afford a better representation of the figure itself. Being able to see the two aspects may be likened in the case of scientific theories to being able to understand two different theories. Though this point is not central to the Perspectivist view, it may nevertheless be mentioned that once this understanding is obtained, a scientist can conceptually move from thinking in terms of one of the theories to thinking in terms of the other, in a way which is quite analogous to one's shifting from one Gestalt aspect to another. This kind of conceptual shift is not dependent on argumentation. What often may be dependent on argumentation, however, is the judgement as to which of the theories

or perspectives is the superior. And this sort of argumentation can be represented in the context of the Gestalt Model as involving the relative applicability of the different aspects of the Gestalt switch figure. Thus the Gestalt Model seen as an analogy in its own right actually seems to be better able to handle the issue of argumentation and theory change than has been thought by some commentators.

The fact that different researchers may, within the context of the Perspectivist view, value the criteria of choice of perspective differently is of interest in connection with the distinction between instrumentalist and realist views of scientific theory. Though the intention has been that the Perspectivist conception should be capable of embracing both the instrumentalist and realist positions, some readers have thought the view actually to be implicitly instrumentalist, while others have claimed it to be fundamentally realist!

If we look at the instrumentalist–realist distinction in relation to a choice between two perspectivally incompatible perspectives, then the criteria of choice employed by the realist will require his considering the extent to which, to the best of his knowledge, the respective perspectives depict a physically possible reality. His interest is in having a theory which comes as close as possible to depicting what may be a real causal mechanism responsible for the laws the theory is thought to explain.

The instrumentalist, on the other hand, feels free to choose whichever of the incompatible perspectives that best serves his purposes of the moment, which may be, e.g., the prediction of the outcome of a particular experiment. Thus, on some one occasion, he may choose a perspective which is highly accurate in just that area in which he is interested, and ignore another perspective with a much wider scope. And where the realist may highly value simplicity as an indicator of the plausibility of a perspective as a candidate for depicting the true nature of a certain aspect of reality, the instrumentalist might well adopt a theory with ad hoc ele-

ments, so long as its lack of simplicity does not hinder its functioning as a tool for providing him with the particular information he is seeking.

The particle and wave theories of light may be used to illustrate certain differences between instrumentalism and realism as seen on the Perspectivist view. Each theory is itself a conceptual perspective sharing with the other an intended domain consisting of certain light phenomena. Due to their respectively characterizing light in contrary ways, the two theories cannot be applied logically simultaneously to their common domain – that is, they are perspectivally incompatible. They are also incompatible as regards the results they suggest of certain operations, such as those involved in the slit-screen experiment and observations of the photo-electric effect; however, the relative acceptability of neither theory is clearly greater than that of the other.

For the instrumentalist, the fact that the particle and wave theories are perspectivally incompatible is of no great concern. Though he cannot employ them simultaneously (cf. the complementarity principle), he may nevertheless use either of them at will, and at any one time may be expected to use the one which is e.g. the most accurate with regard to the particular phenomena he is investigating. Thus, for example, if his interest is in diffraction phenomena, he could be expected to use the wave theory; if it is in phenomena involving energy transformation, the particle theory.

The realist, on the other hand, is primarily concerned to learn about the true nature of light, and thereby be able to explain all of the various phenomena associated with it. While he recognizes that light manifests itself as though it were both continuous (wave theory) and discontinuous (particle theory), he admits that, given the fundamental principles in terms of which we conceive of physical reality, it cannot actually be both. He thus sees the perspectival incompatibility of the two theories to be highly problematic, for it represents what is presently seen as an ontological impossibi-

lity. However, the fact that neither of the theories has been clearly shown to be the more acceptable means that the realist has insufficient grounds for abandoning either, for the theory abandoned might equally well turn out to have an important bearing on what the nature of light really is.

The direct application of the Gestalt Model in its present form to the wave–particle case might suggest that light itself *can* be both continuous and discontinuous, if these states are taken in analogy to those of being a duck aspect and a rabbit aspect of the duck–rabbit. The figure may be considered to be, at one and the same time, a picture of both a duck and a rabbit, though it does not manifest itself as both simultaneously to the person viewing it. It would thus be more in keeping with the Perspectivist view to take as the model a situation, say, where one sees something which looks as though it might be a real duck or rabbit. For the instrumentalist then, it may be treated as either, so long as it is in some sense useful to do so; while for the realist, more importance is put on the unresolved question of whether the entity actually is a duck or a rabbit, or perhaps neither.

In the above ways then the Perspectivist view may be seen to be in keeping with both the realist and the instrumentalist conceptions of scientific theory. And though the primary role of the Gestalt Model has been to afford an intuitive introduction to the Perspectivist view, in certain situations it may be applied by itself, and thereby provide its own insight into a particular aspect of science.

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TRUTH AS SUCCESS OF ACTION. THE CONSTRUCTIVE APPROACH IN THE PHILOSOPHY OF SCIENCE

Philosophy of science now reveals such a variety of approaches that a general judgement on it seems to be impossible. Paul Feyerabend's maxim 'anything goes' – intended to forbid imposing philosophical postulations upon science or scientists – has apparently been applied to philosophical work itself. Despite this, I see a deficit in present-day philosophy of science in general, a pragmatic deficit for that matter, in a very literal sense. Little attention is paid to the fact that doing scientific research is acting purposefully. More specifically, research in physics essentially offers the clearcut prototype of purposeful actions, namely the *poietic* actions of manufacturing or craftsmanship.

My paper argues against a double 'descriptivism' in present-day philosophy of science in the following sense. Roughly speaking, it is held that philosophy of science describes science, and science describes nature; the philosophical description analyzes in what sense the scientific description describes nature. I do not, however, intend to go into a detailed critique of this double descriptivism, rather I am trying to present the main outlines of an alternative philosophical approach which, to translate a common German expression into English, is called 'constructive philosophy of science', and is sometimes termed 'constructivism' by its critics. I hope that the positive results of this way of philosophizing will reveal deficiencies or even mistakes in the concurring approaches and may help to avoid them.

Imre Hronszky, Márta Fehér and Balázs Dajka (eds), Scientific Knowledge Socialized, 313–326.
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1. A THEORY OF TRUTH

In the first part of this paper, I intend to sketch the outline of a theory of truth which could properly be called a pragmatic or an instrumentalist theory of truth. However, these labels are misleading if they recall the philosophy of American Pragmatism (or the Pragmaticism of C. S. Peirce). In order to exclude such connotations, the proposal ought to be regarded in its own right. Beginning with everyday language, we distinguish two forms of knowledge. We know, on the one hand, how things are and, on the other hand, how things work (or ought to be done). Knowledge of the first form is formulated in statements which describe facts like 'this rose is red' or 'the woods in Germany are dying', whereas the second form of knowledge consists of abilities, skills, methods or strategies.

As I am interested in scientific knowledge, I restrict myself to such knowledge as can be represented linguistically. Therefore, we have to characterize these two forms of knowledge with respect to the sentences which represent them. What is it that distinguishes sentences which contain knowledge about the world as it is – so to say, in itself and independent of human actions – from sentences which concern 'know-how'? (For the sake of brevity, let me keep to the terms 'world knowledge' and 'know-how' in the following.)

A glance at the linguistic representation of knowledge seems to make the difference between 'world knowledge' and 'know-how' disappear because each of these can be regarded as being part of the other. On the other hand, know-how can be subsumed under world knowledge which then contains human actions: one describes how one has to act in order to be successful. Or, on the other hand, world knowledge can be subsumed under know-how: one knows how to describe the world adequately. (A preference for one or the other subsumption has a lot to do with personal temperament: the rather phlegmatic, passive or fatalist character

prefers the view that we have first to know how the world is in order to be able to act in it: this is the temperament fitting all forms of empiricism and analytic philosophy. The more sanguine, active or optimistic character prefers the view that we have first to act in order to know – by success or lack of success – how the world is: this is the temperament adhering to different forms of pragmatism, instrumentalism and relativism. Both claim to be rational and undogmatic.)

But, fortunately, neither is there a problem of 'the chicken or the egg', nor is it necessary to make a decision on grounds of personal taste or temperament. For there are at least two clear criteria for distinguishing world knowledge and know-how.

(1) Without loss of information, one can linguistically represent know-how by *de*scriptive and *pre*scriptive sentences (think of recipes or of instructions for use). World knowledge, on the other hand, can never be packed into the form of instructions.

(2) Any discussion about the truth of a sentence representing know-how can logically be traced back in a finite number of steps to a sentence the truth of which can definitely be decided by mere acting, that is, by a non-linguistic action. In other words, know-how sentences can be founded in a finite number of steps or arguments without circularities or gaps.

In contrast to this, K. Popper's so-called Fries trilemma and its elaboration towards the Münchhausen trilemma by Hans Albert hold for world-knowledge sentences: any argumentative foundation of a scientific theory ends up either with dogmatism or a logical circle or infinite regress.

This second difference between world knowledge and know-how – a conclusive decidability of the truth of sentences – is a main motive for constructive philosophy of science and its attitude towards natural science. Roughly speaking, this approach consists in the attempt to trace back logically and methodologically ascientific knowledge to elementary actions, that is to say, to those manufacturing procedures which bring about the instru-

ments and laboratory equipment of physics and chemistry and which are the prototypes of rational know-how. This rationality is the rationality of a successful choice of means for (explicitly known) ends.

2. PROTOPHYSICS

In the second part of my paper I try to draw the consequences of the above on a foundation of physics.

The tradition of modern philosophy of science urges one to think of scientific know-how only with respect to experience. Accordingly, experience tells us what actions are successful under what conditions. But this is one of the points which reveal the pragmatic defect of a descriptivistic philosophy. If scientific knowledge as opposed to everyday knowledge is to be based on *repeatable* procedures under *reproducible* conditions there have to be instruments or, in general, some apparatus like measuring tools and other laboratory equipment. I first discuss the question of which kind of know-how concerns the *reproducibility of data* gained by measurement. As data are supposed to contain 'empirical' information about objects to be measured, reproducibility is not a property of the object which is controlled by a measuring instrument but a property of the instrument itself. The so-called objectivity gained by measurement is nothing but the reproducibility of a few relevant properties of measuring tools. Our problem now reads: what really does make one measuring tool equal to another of the same parameter in such a way that the same object leads to the same data when obtained by the two measuring tools? I leave aside empiristic oddities such as, for instance, the proposal that clocks should be controlled by means of other clocks and in the hope that those logical circles do not cause any trouble if hidden in the cloud of an entire complex theory. The answer to my question (What know-how brings about measuring tools as to make empirical data possible?) is given by constructive protophysics.

Protophysics is a non-descriptivistic, or to be more precise, a normative theory. It consists of systems of norms which prescribe properties of measuring instruments as the technical goal of their production.

The question as to which goals have to be pursued in order to make the relevant properties of measuring tools reproducible is again blocked by an empiristic theory. From H. v. Helmholtz till R. Carnap and C. G. Hempel, measurements are said to be reducible to the counting of the units of magnitude measured. Despite the practical use of acknowledged definitions of – say – the meter, the second and the kilogram, the reproducibility of units is a matter of empirical knowledge on the basis only of already successful measurement. The empiristic answer to the question 'What makes measurement possible?' is therefore circular and epistemologically worthless.

As opposed to this tradition, the normative operational definitions of protophysics are founded in the discovery of the German philosopher Hugo Dingler that homogeneous forms are reproducible without control by measurement. Unfortunately Dingler did not succeed in elaborating this programme to an extent which could provide success against his empiristic competitors. This has been done but by philosophers of the constructivist school.

According to my own work on protophysics a brief sketch of this approach runs as follows: I distinguish instructions like 'Close the door!' or 'Bring these two bodies into touch!' from norms like 'Produce a homogeneous surface, motion or consistency of a body!' by the criterion that an instruction says what to do, whereas the norms say what goal to pursue, what state of affairs to realize, nota bene without saying by which means or actions.

Geometry, kinematics and dynamics are regarded as normative theories of the measurement of length, duration and mass. In contrast to the formalist or axiomaticist view they are not treated as formal and axiomatic theories which later are applied by physicists but are built up on the basis of norms prescribing the pro-

duction of homogeneous forms of space, time and matter, namely the forms of flat surfaces, straight edges, rectangular coins, uniform motion and uniform density.

These homogeneous forms are reproducible in a methodically strict sense, and the reproducibility can even be logically proved. As it would require a lot of technical discourse I prefer to illustrate this reproducibility in the strict sense by an example: think of sets of weights as they are used in pharmacies together with symmetric scales. And let us presuppose that the measurement of volume is protophysically established – a presupposition which is fulfilled in the articles and books by P. Lorenzen, R. Inhetveen and myself. Then you can calibrate sets of weights by means of measurement of volume (mass-ratio equals ratio of volume of two bodies) as long as you know that your weights are made of a homogeneous material. This knowledge, however, can be provided as basic know-how by the norm to produce a body, any two arbitrarily chosen parts of which are undistinguishable on a symmetric scale or any other symmetric machine acting on those parts as long as they have the same volume. The consequence of reproducibility of homogeneous forms of space, time and matter is known to everybody: in practice it does not matter whether we use sets of weights made of iron, silver, or even different quantities of water. The mass-ratio of two bodies of the same homogeneous material depends only on the ratio of their volume. By analogy ratios of lengths or durations do not depend on the particular yardstick or clock in use. In geometry, which is regarded as the theory of length measurement, the analogous reproducibility means that any homogeneous, that is, even surface, operationally defined as surface, two fitting counterparts or plaster casts of which fit each other when brought into touch, and any four rectangular coins fit if brought in touch at their edges. And protophysical kinematics is based on the norm of uniform motion saying that two machines are clocks if parts of them (the pointers or hands) move at a con-

stant ratio of velocities if repeated independent of the starting positions of their hands.

The constructive approach of protophysics has been sketched to a greater extent in this paper than the rest of scientific know-how as it is less well known to the English-speaking scientific community. It turns out that the know-how which makes measurement, and therefore physics, possible is the art of instrument builders theoretically mastered or reconstructed as a system of protophysical norms. Their validity or 'truth' lies in their normative character and therefore is not empirical or emprically refutable. Of course, these norms are not categorical imperatives but relative or conditional norms: they are rational relative to the goal of making properties of measuring tools reproducible, measurements repeatable, and physics empirically controllable. Anyone who does not share these goals does not have reason to accept protophysics.

3. EMPIRICAL KNOW-HOW BY EXPERIMENTS

In this third part I intend to show that empirical theories and not only pre-empirical knowledge of scientific rank consist of know-how. In short, any experiment in natural science starts with the construction and production of a machine. Every experimenter needs to have at least a rough idea or a hope of how that machine is supposed to work, otherwise he would be unable to construct it. The only aspect of an experiment which provides emprical information is whether this machine works or does not work as it is supposed to do according to the plans or purposes of its builder. In other words, experiments are empirically informative inasmuch as they show whether the actions of the experimenter constructing the experimental device are successful or not. In both cases, although not to the same extent, the experimenter acquires know-how.

In many cases so-called natural laws are immediately represented by a corresponding experimental machine, e.g. the Atwood machine for the Galilean law of free fall or the Cavendish balance for the Newtonian gravitational law. In other cases this correspondence is less immediate. But there is no experiment of any importance which does not have a logical connection with at least one so-called law, and there is no so-called natural law which has no logical connection with at least one experiment. Therefore, any so-called natural law is know-how. The truth or validity of this law lies in the success of human actions in producing a machine representing more or less immediately this law.

4. THE NATURAL PHENOMENA

But one might object: What about the *natural* phenomena not artificially reproduced in laboratories; for instance, what about astrophysics? Here, indeed, things are more complex. But even in case of objects not made by man any scientific knowledge turns out to be know-how, that is to say does not have any other truth criteria than the success of technical measures oriented towards explicitly known goals.

First of all, there is no controversy on the point that all of our knowledge of natural phenomena is based on observation. Observations of any importance for science are made by means of instruments. Their technical complexity is varied and ranges from a simple diopter (as used by Tycho Brahe) to a fully automatized artificial satellite. Despite these different properties of observational tools they do have a methodological property in common: any user of an observational instrument must be able to distinguish between information about an observed object and a mere misfunction of his instrument. In other words, he has to know how his instrument is expected to work. Otherwise he could not, for instance, distinguish between the colour of an object seen through a

microscope and a colour caused by a bad system of lenses. Alternatively an observer would think he had discovered a new star by means of a telescope because he was unable to distinguish a light point on the heaven from a light point caused by a reflex of a faulty mirror.

It therefore makes sense to say that observations are *actions* which may or may not succeed. A necessary condition of success is the mastering of the instruments in use. An observational instrument is mastered if its effect or performance, e.g. the enlargement of a visual field, is known beforehand, regarded as a technical goal and reached by technical means – for instance by lense grinding and composing lense systems. In short, making successful observations is know-how, otherwise observations do not provide any empirical information.

Of course, granted the availability of well-functioning instruments for observation on the basis of normative definitions of their purpose, there is more empirical knowledge of natural phenomena. Up to now only the repeatability of observations has been reached inasmuch as it depends on the instruments in use. Furthermore, certain expectations if not conceptual framework concerning the observed objects are needed in order to obtain observational data. To observe something scientifically is always to look for something and never a mere passive roaming about without paying any attention to a particular point. It is often said that discoveries may be or have been made 'by chance' and completely surprisingly. This, however, is no objection but a confirmation of the thesis that any scientific observation is looking for something specific. For first of all, a surprise presupposes attention or a psychical setting of the observer towards some observation other than that really made by chance, and secondly, this unexpected observation is of scientific use only if it can later be repeated or at least witnessed or documented (in the case of single singular happenings), that is, if it is repeated under a certain question or with a certain attention towards something particular.

In common terms the observer needs a theory or a model in order to gain observational data at all. My point is that such a theory or a model is again knowledge of the know-how type. Both the etymology of the word 'model' and the cultural history of the concept of model starting with Renaissance architecture hint at the use of technical or machine models.

It is of no significance for my argument that modern relativistic or quantum physics prefers arithmetical models to mechanical ones. As long as there is no mastering of a model in the sense that any input of observational data into the model does not lead to other derived data which can then be looked for by observation there is no knowledge of the observed objects.

From this follows that scientific knowledge of natural phenomena consists of a twofold know-how: the know-how to realize the function of an instrument for observation, and the know-how to construct a model simulating the observed.

4. THE PRAGMATIC CHARACTER OF CONSTRUCTIVISM

Regarding the foregoing topics of protophysics, experiments, natural laws, and observation of natural phenomena, the claim of constructive philosophy of science that it fills a pragmatic deficit in other philosophical approaches should have become clear.

There is no quantitative, empirical science possible without the use of instruments. Instruments are artificial or, translated from the Latin *'artificium'* to the Greek τεχνη *(techné)*, technical objects. This means two sorts of things: first, they are produced by man, more precisely they are manufactured or even more strictly speaking, they are, when it comes down to it, manually shaped by changing naturally found bodies, their properties are results of actions and their availability is granted by the know-how to construct and build them; secondly, no *empirical* description of tools or instruments can be exhaustive. The functions of instruments are

defined by norms which, as technical ends or goals, guide the construction and the use of these instruments. A full appraisal of science and its results therefore is only possible if attention is paid to the pragmatic character of tool making towards technical ends. The objects of natural science are constructed, not natural, and experience takes place only in the form of success of technical actions (although not every success of actions which is relevant for science must cause *experience* as was explained in the section on protophysics).

6. HOW TO LOOK AT THE HISTORY OF SCIENCE AND WHY THOMAS S. KUHN IS WRONG

So far four arguments have been put forward:

(1) Know-how as opposed to world knowledge can be represented not only in *de*scriptive but also in *pre*scriptive sentences. Know-how sentences can be judged with respect to their truth or validity in a finite number of steps without gaps and circles by reducing them to instructions which can be followed in non-linguistic actions.

(2) Measurements require well-functioning measuring tools. These functions can operationally be defined by systems of norms for the manufacturing of homogeneous forms of bodies with respect to space, time and matter. Those norms prove to be realizable or fulfillable by a know-how linguistically given as basic instructions to shape, move or mix bodies.

(3) Any laboratory experiment in natural science requires a machine to be built according to a plan. Whether the machine runs as planned or not determines the empirical result of the experiment. The repeatable experiment is therefore a kind of successful technical action, is a know-how.

(4) Non-artificial or so-called natural phenomena are scientifically known by way of observation with respect to a theory or a model. The so-called description of natural phenomena thus turns out to

consist of two different components of technical know-how: first, the know-how to produce or use the observational instrument, and secondly, the know-how to fit a technically mastered model into the observed data.

Taking into account all four arguments, scientific theories – if claimed to be empirically controllable in a repeatable and finite way – cannot any longer be seen as descriptions of nature in any sense. To be sure they have an empirical character with the exception of their pre-empirical protophysical norms, but this experience happens as a success of actions which have to be directed towards ends known in advance in order to dispose of a criterion for success at all. If the empiristic attitude in science stresses the character of experience as something which merely happens or occurs, then this is only halved rationality: experience always occurs or happens to somebody, and scientific experience in particular occurs or happens only to somebody who makes tremendous efforts, as everybody knows. It is therefore not only appropriate but even indispensable to ask about the purposes, about the technical goals and about the ends of producing scientific experience in order to philosophically understand science. Admittedly, in history some goals have been irrational in the sense that they were morally or politically unjustified, and allowing that some means to reach the purposes of science have been irrational in the sense that they were unsuccessful or erroneous ways of research, I would like to stress the fact that technically provoking new experience by means of machines is the most rational part of our history – despite the bright antirationalistic clownery of P. Feyerabend.

The fashionable objections to scientific rationality merely start out from the wrong starting-point: as long as one assumes that science describes nature or maps nature as a realistic painter does, there is no place for cultural or social influence on scientific knowledge. On the contrary, as long as one assumes that scientific theories have only a social or even institutional basis as musical taste has, there is no place for rationality continuing through our

history of culture. But there is neither a natural law nor any other need forcing us to decide between the alternative either of a cumulative growth of empirical knowledge or of revolutionary changes of incommensurable scientific paradigms, as proposed by Kuhn.

Taking into account the pragmatic aspects of acquiring scientific experience by means of purposefully produced and used tools, one can see that the history of science is a development of ends or purposes and of the means of reaching them. And both, means and ends influence each other, that is, reliably available technical means often give rise to new ends. However, these interrelations of theoretical ends and practical means, of goals ranging from the technical use of scientific knowledge to the construction of world pictures *(Weltbilder)* and their technical fulfillment, never lead to incommensurability of paradigms, because there is a continuous history of technics. From the early craftmanship in antiquity onwards to our most modern information technology there is a cumulative growth of knowledge – or rather, of know-how. The number of parameters or magnitudes observable or measurable has continuously increased as has the preciseness of measuring results. Of course, proposals have been made to interpret acknowledged data in a different way by assuming a new and different theory. But the technical ends, e.g. all the criteria of the functions of scientific instruments, have remained the same from the pre-scientific art of measurement and instrumentally supported observation until today.

The pragmatic deficiencies in the approaches of Kuhn and Feyerabend and their fixation on the descriptive character of scientific theories alone are responsible for their particular blindness to the irrelevance of theory making for the development of technology. To put it more precisely: technology is not, as is often stated, applied science or the spin-off of science, but science is applied technics. Technical means have been applied within scientific research and have made it possible. Therefore the history of science has its basis in the history of technics, the revolutions of which are

at most social ones but not revolutionary changes between incommensurable theories.

7. CONCLUSION

To conclude with the question must be asked as to what the advantage of a shift from the double descriptivism to the constructive philosophy of science would be. I shall mention two advantages:

First, both science and philosophy of science are cultural phenomena. They are practice or results of practice which do not simply happen to passive individuals or to a passive mankind, but they are brought about as a more or less successful pursuance of ends or goals. Constructive philosophy of science looks for systematic reconstruction of science without neglecting its practical foundations, namely craftsmanship and technics; and it regards this foundation as practice under the guidance of ends or goals. Constructivism therefore allows of a cultural understanding of science and in particular of an explanation as to how social factors can influence science.

Secondly, constructive philosophy of science formulates and follows a principle of a methodical order of steps in gaining and testing theories. No philosophy of science is able to elucidate science if it leads to reconstructions or interpretations of scientific theories which factually, that is, by actions in laboratories, could never be realized or followed. There is, for instance, no operational realizability of circular definitions, and there is no jump from a technical basis of science to another incommensurable one.

Focussing on the fact that doing research in natural science is to an indispensable extent manually acting towards ends, the constructive approach covers both the historical and the systematical aspect of science, as well as the social and the rational one.

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A PLEA FOR AN INTERACTIONIST EPISTEMOLOGY

1. INTRODUCTION

More than twenty years after the 'historicist turn', hardly anybody will deny that the common devotion of history and philosophy of science to science requires them to help each other to overcome their essential deficiencies. These deficiencies concern the temporal dimension of science on the part of philosophy of science, and the question of truth on the part of history of science. Although the need for help is mutual, the burden is not equally distributed. It is philosophy of science that has a twofold task: it has to accommodate the fact that in the course of history our representation of nature – in whatever way we understand the term 'representation' – has been reorganized several times, and to reconsider the notion of truth in the light of the newly discovered historical nature of science.

Generally speaking, contemporary philosophy of science offers two strategies as optional for approaching the task: epistemological objectivism in the form of scientific realism, and epistemological subjectivism as instrumentalism or relativism.[1] Epistemological objectivism can be epitomized by the claim that scientific theories are true or false in virtue of how reality is, independently of ourselves. Epistemological subjectivism holds that scientific theories are neither true nor false but are our instruments for reasoning about reality, or that the truth of the theory depends on ourselves in the specified way. Relying heavily on tradition, and being exclusive of each other, both strategies, so far, have failed.

Namely, for a traditional realist who in one way or another is committed to some variant of the correspondence notion of truth,

Imre Hronszky, Márta Fehér and Balázs Dajka (eds), Scientific Knowledge Socialized, 327–345.
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a knowledge system is unselectively open to any input coming from reality. The system grows by the accumulation of ever more correspondences. On the other hand, from a subjectivist perspective behind which there is always some kind of coherence notion of truth, a knowledge system is essentially closed and indifferent in respect of reality; it occasionally reacts to external perturbances in order to reestablish internal equilibrium. The system grows by unfolding its internal logic in a more complete and more efficient form. Now, if it is true that the only systems capable of not just growing but also evolving through adaptive self-reorganization are open systems which maintain their integrity in a continuous inflow and outflow in the building up and breaking down of components, then it is no wonder that both objectivism and subjectivism face insurmountable difficulties when confronted with the historical dimension of science.

Richard Rorty (1980) is very convincing, claiming that the mainstream of traditional epistemology has been caught up in what he terms the 'ocular metaphor'. As a way out, he offers a replacement of epistemology with hermeneutics as a theory of what he calls 'normal discourse'. Kurt Bayertz (1981), pleading for something quite different, that is, for an epistemology that would be a theory of the 'constitution of a scientific research-object', sees traditional epistemology in the same vein as framed according to the 'perception-oriented model'. Even the declared realist Roy Bhaskar (1975) speaks critically about empirical realism or epistemological individualism "in which men are regarded as passive recipients of given facts and recorders of their given conjunctions" (p. 16). Lining up with Dewey in his critique of the spectator theory of knowledge, Ian Hacking (1983), another realist of sorts, claims that "most of today's debate about scientific realism is couched in terms of theory, representation, and truth" and that in this context "there can be no final argument for or against realism" (p. 31). Finally, following Lukács and the Frankfurt School, an influential faction of Marxist philosophers abandon the ortho-

dox 'copy theory of truth' as essentially positivistic in favour of the 'mediation of nature through society and society through nature', as Alfred Schmidt (1971) phrases it. Thus it seems that the critique of the ocular metaphor, of the spectator theory of knowledge and of perception-oriented epistemology is becoming the common meeting place for different traditions in epistemology and philosophy of science. They join here shortly before proceeding further in their different directions.

It is then worthwhile to look more closely into the 'ocular metaphor' and to bring out its main drawbacks, particularly those related to a possible account of the historical nature of human knowledge. This is done in Section 2. The result is that the perception-oriented epistemology separates representation of nature from interaction with nature, the unity of which is the essential feature of evolutive systems, and that, as Hacking (1983) put it, "the harm comes from a single-minded obsession with representation and thinking and theory, at the expense of intervention and action and experiment" (p. 131).

In order to begin the restoration of the lost unity of representation and intervention in Section 3, an attempt is undertaken which aims to show that in real open systems which obviously evolve and are engaged in a kind of 'cognitive' relationship to their environments, namely in living systems, representation and interaction are inseparably linked to each other, not for any contingent reason, but because of the very nature of life.

The human mode of interaction with nature is a peculiar one and therefore biological analogies are of limited value. What characterizes it is its mediation by human artefacts. It may look as if we now have grounds to separate representation from interaction. However, the closer analysis of the nature of human artefacts and the dynamics of their production, carried on in Section 4, shows that this is not the case. Moreover, it comes out that the dynamics of artefact making are of the same structure as the dynamics of human history, hence an epistemology that concentrates on inter-

action and closely relates it to representation has a much better chance of accommodating the historical dimension of human knowledge. A future historical epistemology, first of all, must be an interactionist epistemology.

2. OCULAR METAPHOR AND HISTORY OF SCIENCE

The essential feature of the metaphor is that it adopts an aristocratic subject-and-object framework in which the cognitively active subject is perceptually passive, that is, almost literally 'placed underneath' to suffer imprints from the object, and in which the cognitively passive object is perceptually active; its activity consisting solely of generating visual and auditory stimuli. The subject and object are always aristocratically separated by the distance the stimuli have to travel. There is no other bodily contact, no other activity going on in between, except signals emitted by the object and arriving at the subject's surface in the form of assorted frequencies and intensities of light and acoustic waves. So, the possible actions and the roles played by subject and object are extremely reduced. All an object can do is emit some signals into the surrounding space, irrespective of whether they are observed by a subject or not; all a subject has to do is to receive signals, to extract from them as much information as it can, and eventually gently stimulate the object to emit more signals.

When observation, as this kind of contact is usually called, is all that is going on between subject and object, that is, when the much-exploited concept of experience is reduced to input reception, then it is no wonder that the independent existence of the object and the verisimilitude of the reflection become dubious and that epistemology is engaged in a superfluous and useless endeavour to provide reasons for believing in the independent existence of external objects and in the credibility of careful and controlled observation. The same doubt, of course, is cast on reflections or

representations, both in the Mind and the Language, so that they have to be justified, their accuracy tested. Epistemology has been given the impossible task of supplying the subject with a firm procedure of justification on the sole and feeble ground of one-way action at a distance between the passive subject and active object. If action is imagined as originating in the object, we have objectivism and the correspondence theory of truth; if it is the other way round, then we have subjectivism and the coherence theory of truth.

Moreover, there is one more reduction imposed on epistemology. If it is to provide us with the firm foundation of knowledge, then whatever way the contact between subject and object is conceived, the epistemological account of it should involve no contact between subject and object. Since the independent inspection of the contact, a necessary condition for a successful outcome of the epistemological project, is impossible, the same condition which defines the project renders it impossible. Hence no one should be surprised that traditional epistemology has not been able to resolve the controversies between subjectivism and objectivism, between two theories of truth, between scepticism and dogmatism, between the internalist and externalist approach to the growth of knowledge and the like.

When framed according to the 'ocular metaphor', the process of justification, regardless of the subjectivist or objectivist option, is confined within the subject. It is reduced to a comparison between diverse kinds of representations, some of them more reliable than others, or between different kinds of propositions, some of them more firmly founded than others. Therefore, the most important part of the cognitive process does not happen where one would expect it to, that is, in the space between subject and object, but is hopelessly imprisoned in the subject, that is, in theory, representation, language and the like.

Some modern variants of the perception-oriented epistemology are still imbued with what Rorty named 'mirror imagery', accord-

ing to which a subject or a medium of cognition, whether it is an individual mind, a collective consciousness, or a language, is put before or 'under-placed', so to speak, as 'a great Mirror of Nature' in order that various reflections – some accurate, some not – of objects thrown against it might shine therein. The cognitive process consists of an effort on the part of the subject to get a more and more accurate reflection or at least a faithful representation of an external object. Since the subject, conceived as Mind or, after the linguistic turn, as Language, is supposedly capable of being studied by pure, non-empirical, that is, exclusively philosophical methods, epistemology is able to help the cognitive process 'by inspecting, repairing, and polishing the mirror'.

If now scientific realism, as a supposedly new answer to an old dilemma, takes on the 'mirror imagery', the only way to escape subjectivism is to declare dogmatically that whatever science establishes as truth about nature exists in objective reality, in other words, belongs exclusively to the object. This is precisely what happens to many of those who try to interpret the mathematical formalism of quantum mechanics from a realist standpoint still impregnated with the 'mirror imagery'. After an early period when realists were puzzled about how to imagine the atom without Bohr's orbits, spin without spinning, an entity which is both a particle and a wave, etc., they followed Bohr's advice not to strive to visualize or make a picture of the atomic processes. As a consequence, everything that enables theory to accurately reproduce experimental results, be it palpable or not, receives the accolade of belonging to microscopic reality. Not only have 'pilot-waves', the 'waviecle', or non-local hidden variables become part of the new ontology, but something called 'propensity', too. The new ontology has only one further limitation, besides providing us with the correct experimental outcome. It must not contain the concept of 'measurement', since it belongs – according to the orthodox realist interpretation – exclusively to the subjectivist repertoire. The realists usually take for granted that the subjectivist interpretation

of the concept of measurement in quantum mechanics, like that provided by von Neumann and Wigner, is the only possible one.

Other modern variants of the same species, of spectator's epistemology, are – we say by analogy – permeated with 'cybernetic imagery'. As this imagery depicts, a system of knowledge is characterized by a certain referent state or a goal it aims at. The reference state might be defined by the shared values of a certain scientific community, by a disciplinary matrix, positive and negative heuristics, present state of the theory, etc. Among the possible goals one may cite prediction of future events, the realization of certain social interests, the solution of problems and puzzles appearing in the system, etc. Observations, that is, the input of the system, are regarded as stimuli or disturbances for an otherwise closed system, and they are assessed in respect to the main parameters of the system. The system responds only to those stimuli which bear some reference to the goal or reference state; the input is loaded by, or interpreted according to, the present state of the system and these parameters. Reaction is determined by the inner logic or regulatory mechanism of the system which, of course, is not constructed in order to get a credible picture of the environment but to achieve an internal reference state.

The narrowness of the choice offered by spectator's epistemology is particularly visible when confronted with quantum mechanics. Caught in the false dilemma of subjectivism or objectivism, unavoidable if somebody sticks to the 'perception-oriented model' of epistemic process, the philosophy of quantum mechanics, orthodox or not, has not yet assimilated the essential message of the so-called Copenhagen interpretation, that is, of the interpretation that is still the basis for the working physicists' understanding of the theory. The message purports to say that quantum mechanics describes neither the independently existing micro-world nor our experience of it, if experience is taken in a classical empiricist sense. Quantum mechanics describes our objective interaction with

the micro-world, an interaction that is inevitable and can be neither neglected nor subtracted from the theory.

It is true that this message was not elaborated systematically or pressed to its final conclusions. Crucial questions like: Why is it not possible to give a physical account of the interaction between the apparatus and the micro-object, even though measurement is seen as an objective physical process?; What is the nature of measurement, if it is neither a completely subjective nor a completely objective process?; How can this subjective process be consistently incorporated into the mathematical formalism of the theory? etc., have not been fully answered, in spite of Bohr's repeated efforts. Nevertheless, careful analysis shows that the Copenhagen interpretation does not necessarily drive us into the subjectivist camp but forces us, first, to dispense with 'mirror imagery' and, secondly, to take the interaction between subject and object seriously. Since it is evidently trying to say something new about 'the central paradox of science', that is, about science as both a human product dependent on man and at the same time a truthful account of things existing independently of man, it certainly still deserves a thorough philosophical study.

Besides the failure of perception-oriented epistemology to perceive the novelty of the situation in modern physics, its greater shortcoming is its inability to give a proper account of the historical nature of human knowledge in general, and science in particular. The model of development which is consistent with mirror imagery is either simple growth by the accumulation of image points, or a move towards a supposedly more perfect picture when the mirror is polished better. Cybernetic imagery allows for a more versatile set of models which accounts for the way a system pursues its goal and ends up in the reference state. However, a cybernetic scheme cannot explain the change of the goal or reference state itself; a system only appears as evolutive or historical.

Still playing with system-theory vocabulary, one may say that a knowledge system, as described by the spectator theory of knowl-

edge, is incapable of proper historical development because it has no 'metabolism'. It is 'open' with respect to incoming information in the form of sense stimuli, but it is 'closed' with respect to 'matter and energy', the substance of knowledge about the real physical interaction between subject and object. The aristocratic framework of the ocular metaphor, wherein the only admissible contact between epistemic subject and object is observation, insulates the human representation of nature from human interaction with nature. It separates 'representation and thinking and theory' from 'intervention and action and experiment'. Still in the same vocabulary, we may say that until we apply an open-system schema to the knowledge system, there is no chance we shall ever understand its historical dimension. But can we apply an open-system schema to knowledge? Can we unite representation and interaction without falling into the trap of the subjectivism/objectivism dichotomy? Can we speak about the 'metabolism' of a knowledge system? If so, what would it be?

3. REPRESENTATION AND INTERACTION IN LIVING SYSTEMS

To indicate the answers to these rather metaphoric questions, let us first take a look at the context from which the idea of open system originates. Let us take a look at the basic features of living systems as open systems. The first and foremost difference between the living and the inanimate worlds is the need for interaction which is present in the former and absent in the latter. The living system, as an island of orderly structure and accumulated energy, cannot oppose the laws of thermodynamics, and therefore cannot maintain, much less reproduce, itself without being limited in space and time, and without a repeated interchange of matter with its surroundings. Space–time closure and interaction with the environment are natural necessities for a living being. That is why the living being and its environment, as spatially and temporally

well-defined systems, and the interaction between them, provide the most general framework for an interactionist epistemologist's study of the cognitive process in general, and the subject–object relationship in particular.

For an organism to be a closed and open system at the same time, distinct from its environment and in constant conjunction with it, it is necessary that the interchange of matter, energy, and momentum with the environment, in one word, the interaction, be both selective and doubly structured. Obviously the interaction should be: first, such as to provide an organism with material rich in free energy and in the elements needed for functioning, growth, and reproduction; secondly, such as to protect it from noxious substances and harmful external influences; and thirdly, such as to free it from degenerated and dispensable material. Hence, not everything that comes from the environment to the surface of an organism should be allowed to enter, nor everything that tends to escape from an organism should be permitted to leave.

The principal ways by which an organism establishes selective interchange are combinations of various semipermeable membranes at the morphological level, and differential behaviour at the ethological level. By these means the interaction is structured according to a double standard, one established by the environment, since the organism can only use those materials, forms of energy, and patterns of momentum that are available in the surroundings; and the other by the organism striving to satisfy its specific needs. Usually these standards overlap due to adaptation and one may speak of a unique doubly-structured interaction wherein the compromise, or better said the synthesis, between two standards is reached. Sometimes they are at odds and the interaction ceases; thus the organism dies.

This general, structured, selective, differential interaction is *ipso facto* the basic cognitive interaction. It presupposes at least one elementary cognitive operation, namely recognition, as a basis for selectivity and differentiation. Sorting out input and output

requires a kind of identification of whatever should be sorted according to some criterion, model, or template. In most biological membranes this is done by the so-called 'transport' molecules of different proteins, asymmetrical in shape and embedded in the fatty bilayer that prevents most atoms and molecules from passing. The different asymmetric structures of these protein molecules form a system of 'templates', a 'code' according to which non-differentiated but already structured potential input or output is 'read' and transformed by selection into what actually passes through. Accordingly, the 'code' is also doubly structured; it follows the organism's needs and the environmental dispositions, or 'affordedness'.

Since all living systems are in need of an environment, precisely, in need of interaction with an environment, the 'template' according to which this interaction is structured must express, in the language of the organism's needs and the capabilities embodied in the forms of certain parts of its own body, the relevant portion of the required environment. This expression could be seen as though the portion of the environment is present once more, or re-present, in the specific organic forms of the organism's body. This re-presentation, this 'making something present again in something else' is, of course, also a compromise or a synthesis of two sorts of demands: organic and environmental. It could be said that the portion of the environment being transported into the organism has a new form imposed on it by its recipient and represents itself in this new form.[2]

The re-presentation is both the consequence of and the prerequisite for the interaction, and therefore for life. For the essence of life is neither the peculiar structure of the organic body nor the specific behaviour of the living system but the interaction to which both the organism with its behaviour and the niche which it exploits are subordinated. Consequently the re-presentation is not a separate, self-standing, independent process but an integral and essential part of metabolic interaction.

This makes clear, I hope, that by the very fact of being alive, that is, maintaining a metabolic interaction with the environment, an organism is a cognitive system, a system wherein the re-presentations of other systems happen and that it is a cognitive system only insofar as it maintains a metabolic interaction. In this context, it should be pointed out that the re-presentations do not occur as a consequence of some process of mirroring the external world, or reacting to external disturbances, but as a part of the creative synthesis of the organism's needs and potentialities and the potentialities and dispositions of the environment realized in the interaction, to which it is so tightly bound.

4. RE-PRESENTATION AND INTERACTION IN PRODUCTION OF ARTEFACTS

In the course of organic evolution multifarious types of interactions have been established and maintained between organisms and their environments. Each new type of interaction has brought about new elements in the cognitive process. These elements have often been just variants along the same line of development, but sometimes they open up a new line, or even a new survival, and hence also a new cognitive strategy. Generally speaking there are three strategic lines along which different solutions of the problem of existence have been created and different ways of re-presenting formed. The first one is a morphological strategy wherein new problems of maintaining a stable interaction are solved by developing new organs or new forms of existing organs. The second is an ethological strategy where the same task has been achieved by the adoption of a new form of behaviour. And finally, the third one is a technological strategy where the challenges for survival are met by the construction of appropriate artefacts. Of course, these strategies have often been combined, especially in the evolution of

higher animals, in a way that the former establishes a basis for the latter.

With the technological strategy, which is primarily, although not exclusively, a human survival strategy, organic evolution has made, so to speak, a step beyond itself, a step beyond the organic world, into a new realm. An artefact, as I shall understand this word here, is an ordered material structure with accumulated energy emerging from thermodynamically unfavourable processes. In this sense it bears some resemblance to living systems, and differs from ordinary, natural, inorganic objects. Yet it is an ordered structure which cannot maintain itself or reproduce itself through selective interaction with an environment. It can never originate spontaneously from the random play of natural forces, nor can it be the result of the reproduction of one or two beings of the same kind. So it is also different from any living system whatsoever and bears some resemblance to ordinary physical objects.

On the one hand, an artefact extends its animate producer into the environment through externalization and the embodiment of some of the living beings' needs, tasks, and features. It completes the living being by solving some inner organic problems externally. On the other hand, although the artefact as regards energy and information content has been, and for maintenance always remains, dependent on the living being, materially it is self-sufficient and independent. It comes from the environment, is made in the environment, and remains in the environment. Therefore, like those parts of an organism's body that are exposed to interaction with the environment, the artefact is a synthesis of the needs and potentialities of the organism and the potentialities and actual features of the environment. It is a successful projection of the internal demands of the living world and its capabilities onto the pre-existing external structure found in the non-living surroundings. So most of the points made in respect of a living system could be repeated here.

This is a chance for the morphologically and ethologically unfinished, incomplete species *homo sapiens* to complete itself technologically through the new artificial world of human artefacts. Consequently, the basic metabolic interaction of humans with their natural surroundings is no longer a direct, immediate one, but it is mediated by the production of artefacts. This gives rise then to a peculiar cognitive dynamics.

The process starts with a certain human need, first of all with a certain biological need, but also with any other one that cannot be satisfied internally. The need is firstly transformed into a conception, an 'idea', a design of an artefact which is a tentative internal synthesis of the need and the dispositions of the environment within reach of the interaction. Then the idea of an artefact becomes the matrix for the mental control of the ordered sequence of bodily actions through which the idea takes off into the external world.

Trying to implement itself in the new media of the external world through guided movements of the body, the idea meets there the pre-existing actual structure of what is called reality. By means of the physical interaction which aims to transform actual reality into an artefact, the idea forces reality to open itself up to the producer and to disclose to him both its actual and potential capabilities; the actual ones as the starting-point for the transformation, and the potential ones as the final form of the finished artefact.

The idea might change in the process, but it would not be the idea of an artefact unless something new entered the external world with it. Looking from the perspective of that world, the final form of the artefact is brought into it from the outside as a strange, novel, yet-not-experienced form. So the idea is always imposed upon, projected onto actual reality, causing it to change along its yet-unrealized possibilities. Finally, modified on the way, enriched or endowed with reality, or if you prefer, justified by reality, the idea enters the external world to find itself implemented,

embodied, externalized, and justified as real or true in the form of the finished artefact. The compromise, the harmony, and the synthesis is reached due to the fact that in the process the idea accommodates reality by stripping off those elements that reality cannot receive, and that at the same time part of reality changes from its actual to its previously potential form, that of the preconceived artefact. (Here we have a new 'context of jusitification' and at the same time a part of the 'metabolism' of a knowledge system.)[3]

Incorporated now in external reality, settled down into the form of the finished, self-standing, detached artefact as its alterbeing, the idea is in the position to become an object for its producer. He can now critically examine the externally and objectively existing ideal; he might compare it with its original internal subjective form and learn something about reality through that comparison. Moreover, taken for this critical examination, the subject externalized in the materialized idea of an artefact becomes its own object, recognizes itself in it and acquires some knowledge not only about the reality transformed during the process, but about itself, too.

However, the object also becomes the subject, since the objectified form of the idea endowed with reality, that is, the artefact, influences the subject by embodying and emitting back everything that has been objectified and externalized in the artefact. Thus man, who partly shapes the external world, is also shaped by it, especially by that portion of it which is shaped by him. (Here we have a new 'context of discovery' and the complementary part of the 'metabolism' of the knowledge system.)

Regarding the nature of modern science, one can easily draw the parallel with the cognitive dynamics of artefact production. Research starts with certain problems and, of course, with the need to solve them. A problem is transformed theoretically, one may also say internally, into a hypothesis. Regardless of how one perceives hypotheses, as a realist or as an instrumentalist, as a verificationist or as a fallibilist, if it is an hypothesis of modern natural

science it should also contain, in whatever form, the idea or the design of an experiment, an artificial experimental arrangement. In other words, a scientific hypothesis is simultaneously a hypothesis about a possible experimental setup – a possible artefact – and about the object of investigation.

During the process of externalization of the hypothesis, when building up a conceived experimental arrangement, the hypothesis does not usually change except for some minor details. This does not mean that the hypothesis is not embodied in the arrangement, that it is something external to a manufactured setup, or that it has not been partially tested already before the finished arrangement has been switched on. However, the crucial test is usually performed through the functioning and outcome of the apparatus, notably during that part of the cognitive dynamics where the finished artefact is taken as an object. The hypothesis is then either rejected or refined or taken as justified, which first of all means 'received by nature as a possible form and functioning of the artefact'. In other words, it is attested that its form and function are not at odds with the actual features of the portion of nature included in the experimental arrangement.

Having this in mind it is now easier to understand the message of the Copenhagen School, which puts forward two claims. First, that the object of microphysics is real but always caught in an artificial experimental setting; it is always part of a human artefact. Microphysics represents not only actual microscopic reality but also a potential one, one brought about in the artefact. Secondly, that the arrangement is created by man who has a certain cognitive history and is manufactured by man with definite physical characteristics. These claims are not necessarily bound to subjectivism and, therefore, should be seriously taken into account by the new epistemology.

5. CONCLUSION

The cognitive dynamics of artefact production, as described here, resembles Hegel's phenomenology of mind, along with Marx's reinterpretation and completion of Hegelian dialectic in the Paris manuscripts and *The German Ideology*. If for 'the artefact we substitute an experimental arrangement', the above also delineates the method of modern experimental science. Because such a conception of human interaction with nature can serve as the common ground for grasping both the historical development of human cognition and the internal dynamics of modern science, I believe it can also serve as the basis for a sophisticated form of scientific realism freed from a commitment to 'mirror imagery' and the ocular metaphor. For, instead of simple reflection we have here an account of a complex subject–object dynamics grounded in full-fledged physical interaction between man and nature, which makes a realist theory of the historical constitution of the epistemic object, and therefore of the scientific research object too, possible.

This possibility rests upon the fact that history essentially records the development and differentiation of various types of interaction with the environment and, accordingly, the development and differentiation of different ways of life for one and the same species. The diversity of the interaction between humans and nature is the result of the mediation of artefacts. Each type of interaction developed through history is mediated by its characteristic set of artefacts that both establishes and is organized around the basic, biological, and therefore trans-historical, metabolic exchange of matter and energy.

Now the cognitive dynamics of artefact production, which in a sense constitutes the complete feed-back loop, nevertheless gives rise not only to 'blind variation', but also to improvement through the critical examination of and reflection on the finished artefact. If one adds to this the fact that artefacts, as embodiments of ideas,

also fix them externally and so make them usable for other people and new generations, the continual and, in a certain sense, cumulative model of human history becomes more plausible than the one based on revolutionary unbridgeable leaps.

However, continuity and cumulation do not exclude the existence of distinct ways of human life, and distinct modes of human interaction with nature made possible by the introduction of new kinds of artefacts. Viewed epistemologically, the existence of different modes of interaction created in the course of human history yields to different types of experience of the same reality and, therefore, to different types of constitution of the epistemic object.

After all is said we can conclude, I think, that there are grounds for believing in the possibility of a historical epistemology which will do justice to the historical character of human knowledge if we first direct our efforts towards understanding the real interaction between man and nature, from which knowledge springs.

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NOTES

[1] For a detailed description and discussion of these positions see Newton-Smith (1981).

[2] The concept of 're-presentation' is an important and delicate one. Here is all I need at the present state of analysis; representation as doubly structured re-presence. The concrete analysis should show the concrete forms of re-presentation.

[3] This analysis of artefact production reveals two sides of the unique process, namely, that on the one hand during the artefact production nature discloses to the producer its actual and potential features, and that on the other hand this disclosure happens as a projection or enforcement of the design of an artefact onto nature. This could provide grounds for the reconciliation of realism and instrumentalism, objectivism and subjectivism and for overcoming the dichotomy.

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JÁNOS KELEMEN

HISTORICISM AND RATIONALISM

(A historicist defence of rationalism, a rationalist defence of historicism)

1. The notions of rationality and reason have different meanings.

1.1. To begin with, it is conspicuous that modern rationalism was born as an enemy of history: Descartes excluded history from the domain of those things which can be known with certainty and can be grasped by reason. According to this view, there is no rational connection in history; reason is not only metahistorical but lacks its own historicity, too. It is universal also in the sense of not being subject to change.

The Enlightenment inherited this conception of rationality. Insofar as the philosophers of the Enlightenment raised the problem of the relation between reason and history, they conceived it in the form of the question whether and how it is possible to introduce reason into history.

1.2. Passions are another example of things to which reason is usually opposed. The heredity of the modern form of this opposition is to be attributed to Rousseau. For him, it is but one of the manifestations of a series of further oppositions like origin and decline, South and North, natural and artificial (conventional), gesture and voice, accent and articulation, vowel and consonant, metaphor and exactitude, song and prose, living talk and writing, life and death. Reason is identified with the negative pole of these oppositions. It is the offspring of man's departure and deviation from nature, a sign and cause of moral decline, self-love *(amour-propre)*, artificial needs and interest. ("Reason gives birth to self-love.")[1] So, it cannot be the bearer of humanity, unlike language which, in opposition to the Cartesian view, does not rest on artificial convention

Imre Hronszky, Márta Fehér and Balázs Dajka (eds), Scientific Knowledge Socialized, 347–364.
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and, originating in nature and passions, did not come to life for the sake of expressing our ideas. For Descartes and the rationalists of the Enlightenment the reign of reason and public welfare coincide; Rousseau dissociates them. Evidently, this antirationalist attitude was justified when it attributed social and moral evil to rationality. Rousseau discovered that the world of reason he envisaged was the world of the rising bourgeoisie, and rationalism was the apologetics of this world. His criticism was directed against the instrumentalism of the bourgeois reason and its indifference to values. (We can assume that this motive is present in critics of rationality and method like Feyerabend. Feyerabend is a modern Rousseau who, similarly, blames reason for its indifference to the values of life, the suppression of the free play of imagination, and the standardization of people's conduct and their way of thinking. It is to be expected in his case, too, that his arguments prove to be valid only against an instrumentalized reason.)

1.3. In Rousseau, the notion of reason was transformed in a double way. Instead of being praised it was blamed and, being associated with man's emergence out of nature and his moral decline, it assumed a quasi-historical dimension. It was German idealism and Romanticism that continued and accomplished the line of historicizing reason. In the meantime it was not only reason that had been made historical, it was history, too, that had been made rational. The two processes of historicizing reason and rationalizing history became identical only, as a final result, in the Hegelian system.

This double act of reconciling reason and history initiated the epoch of historicism.

The philosophically radical step of rationalizing history was accomplished, as a matter of fact, by Kant. The statement according to which reason is not alien to history but, on the contrary, history is in itself rational, has two senses, a narrower and a wider one. Its narrower sense is to be derived from identifying the 'rational' and the 'logical'. In this sense it can be said that history is

not only rational but logical as well, or the 'historical' and the 'rational' have some kind of unity. The thesis of Fichte and Hegel (or of Marx) concerning this unity is a direct continuation of the transcendental philosophy. Since for Kant "the whole world of events in time ... is a schematized representation of the world of logical or conceptual relations", and "Fichte's attempt to detect a conceptual scheme underlying the temporal succession of historical periods is ... a perfectly legitimate application to history of the Kantian doctrine of the schematism of the categories."[2]

The wider sense of the rationalization of history was expounded by Kant in his minor writings about philosophy of history which do not belong to the transcendental philosophical system. In the 'Idee einer allgemeinen Geschichte in weltbürgerlicher Absicht' he clearly formulates the idea that envisaging a series of events in their mere temporal succession has nothing to do with history. Besides this we need an interpretative scheme in the framework of which the chaotic world of events can be thought of as a connected whole subjected to some intelligible lawlike pattern. Kant found the point of departure for such an interpretative scheme in the notion of a purpose or plan of nature, that is to say, in the assumption that the events become history for us if they are related to the unity of an immanent purpose. Without touching upon the difficult problem of the Kantian teleology it is to be remarked that in the light of Kant's later system the notion of the purpose of nature turns out to be regulative and not constitutive: "For the conception of a physical end is altogether unprovable by reason in respect of its objective reality, which means that it is not constitutive for the determinant judgement, but merely regulative for the reflective judgement."[3]

It amounts to saying that the purpose of nature has no objective reality; however, history displays an order as if it obeyed a plan. So, for the sake of arranging the historical material, reason is entitled to use the concept of the purpose of nature as a regulative principle.

Kant's writings about the philosophy of history, like his review of Herder's *Ideen zur Philosophie der Geschichte der Menschheit*, were conceived in terms of the polemic against Herder. One of the main topics of the debate was in fact the question what the purpose of history consists in (whether it consists in the development of the species or in the happiness of the individual). In the light of our earlier remark this teleological language can easily be translated into a naturalist one and the debate makes sense also in terms of historical progress. It can be said that the debate is focussed on discovering the true essence and the criteria of historical progress, or on establishing the conceptual framework required for its correct assessment. According to this, rationalizing history, in the wider sense, amounts to adopting the view that history as a temporal succession of events has an objective order and a definite direction.

The proposition that reason, in the form of a purpose of nature, is inherent in history, does not mean for Kant that history is *directly* rational or that history and rationality *directly* coincide. His rationalizing history amounts only to his stating that "he who observes the play of the human will on a large scale is able to discover its regular course".[4] Rationality does not appear on the level of single phenomena of history, it manifests itself only in relation to the species. The achievement of the purpose of nature is an unconscious by-product of the activity of the individuals. In the doctrine of the 'cunning of reason' Hegel takes over this idea of the purpose of nature which is realized independently of the consciousness of the individuals and manifests itself in the sum of their chaotic actions. (It is worth noticing that Vico's concept of providence has exactly the same meaning.) So, even the classical philosopher of the unity of the 'historical' and the 'rational', or the 'historical' and the 'logical' does not teach that history is directly rational and logical. It is just the function of the idea of the 'cunning of reason' that it prevents us from directly identifying these concepts and determines the logical place for their mediation. It

follows from this that the distinction between 'conscious' and 'unconscious' is a necessary element of the Kantian and Hegelian systems: historical rationality asserts itself on a deeper, unconscious level.

This transformation of the notion of reason which resulted in the rationalization of history had the further consequence of rendering it possible to conceptualize the possibility of historical knowledge. With regard to the Cartesian view of scientific rationality this created a completely new tradition. In this tradition history is not outside the domain of intelligibility; quite on the contrary, it is the paradigmatic case of rational cognition, partly because history is made by us (Vico), partly because it is immanently rational and reason recognizes itself in it (Hegel).

1.4. When surveying the variants of the notion of rationality we come across a conception according to which the 'historical' and the 'rational', the 'historical' and the 'logical' *directly* coincide. Its most remarkable formulation is Croce's absolute historicism, stating the identity of philosophy and history. The thesis is founded upon the analysis of the nature of individual and defining propositions according to which history uses individual propositions, while philosophy frames defining ones. Since, as a consequence of the unity of the individual and the universal, the two types of propositions are in fact identical, "philosophy and history are not two forms but just one, and they do not presuppose each other, they are identical".[5]

Identifying history and philosophy is tantamount to identifying history and logic because logic is the science of the pure concept and, so, it is just philosophy. Such identifications express Croce's view that life and reality are but history, and different manifestations of reason and spirit exist as historical forms of this unique reality. The absolute rationalization of history and the absolute historicization of reason here coincide. The idea of the positivity of history, one of the many consequences of this conception, must be emphasized. In consequence of the direct identification of the

'historical' and the 'rational', of the 'historical' and the 'logical' there is no place for the Hegelian distinction between the idea and the spirit realized in history, between the idea and the individuals driven by it. In such a way history can be viewed only as an unbroken progression which has no regressive phases. It "passes over from the good to the better where the evil is just the good viewed in the light of the better."[6] Now, it is no more possible to conceptualize the problem of historical evil. The charge which is usually brought against Hegel's thesis of the identity of the 'rational' and the 'real'[7] is certainly valid against Croce's absolute historicist notion of rationality. The latter can be charged with justifying the total course and every single moment of history. In this respect Croce's dictum is quite explicit: "History does not judge but it always justifies."[8]

2. No wonder that the notion of rationalism, like that of rationality, also has many variants.

2.1. In the first place, rationalism is a name for philosophical movements which can be well identified historically and are characterized by a system of definite theories. On the epistemological level it can be opposed to empricism in this sense. (Cf. the opposition of Cartesian rationalism and British empiricism.)

2.2 Rationalism is taken for the opposite of mysticism, Romanticism, aestheticism, intuitionism, and mythologism as well. In this sense, e.g. Aquinas is a rationalist against Gioacchino da Fiore, Kant is a rationalist as opposed to Hamann, and so on.

2.3. Rationalism is sometimes opposed to traditionalism, to the organicistic idea of collectivity, or simply to historicism interpreted as traditionalism and organicism. In the background of this opposition we discover the tension between universalism and relativism. This tension motivates Kant's and Herder's debate in the field of the philosophy of history and gives the grounds for Hamann's and Herder's 'metacritiques', in which these two founders of German *'Sprachphilosophie'* refute the *Critique of Pure Reason* from the point of view of a philosophy of language.

It is on the basis of a similar opposition that the philosophy of Lévi-Strauss's cultural anthropology can be taken for a kind of rationalism, in fact, a superrationalism. (According to it magic, totemism, mythology and science are permeated by the same rationality, each of them being an equivalent manifestation of the basic structure of a timeless, uniform spirit.)

2.4. Lucien Goldmann takes rationalism and empiricism for two variants of epistemological and ethical individualism as opposed to the attitude focussed on totality. Among the different notions of rationalism, Goldmann's belongs to those in which a negative appreciation is involved inasmuch as they stress the limits of rationalism (e.g. moral values cannot be deduced from it). This is a line which strongly parallels Adorno's and Horkheimer's critique of the Enlightenment and rationalism. According to Goldmann's opposition, Kant and Hegel are to be found on the side opposite to rationalism like Marx, Lukács and Heidegger who, equally, are qualified as representatives of a tragic vision deduced from the principle of totality.

2.5. It seems natural to define rationalism in opposition to irrationalism. This approach has its difficulties, too. Let us recall the fact that two great critics of irrationalism, Lukács and Popper, use this term in quite different senses.

For Lukács irrationalism is a given philosophical school, a product of the dissolution of German idealism, a characteristic current of the imperialistic epoch. For this reason he has to make it clear that irrationalism cannot have a comprehensive history comparable to the history of materialism or dialectics.[9] From this point of view irrationalism cannot be used to define rationalism that has incomparably broader historical grounds and was the creed of different schools in different epochs. For the interpretation of Lukács's notion of irrationalism, we need the classical distinction between understanding and reason since, according to his analysis, irrationalism comes to life when the limits of understanding become evident and the transition from the level of under-

standing to the level of reason meets with difficulties. Irrationalism turns up at the crossroads where the obstacles to understanding can be surmounted only by dialectical reason. "Putting understanding on a level with cognition [equating understanding with cognition], conceiving the limits of understanding as the limits of cognition in general, referring to what is 'beyond reason' (intuition, and so on) where the transition to the cognition of the level of reason is possible and necessary: these are the most general features of philosophical irrationalism."[10]

When characterizing irrationalist philosophers Lukács refers to features (mysticism, aestheticism, intuitionism, etc.) which in the earlier sections have been mentioned as opposed to different notions of rationalism. So does Popper. Popper's notion of irrationalism, however, is radically different. According to him in the history of philosophy there is a comprehensive irrationalist line. It is, above all, a theory of human nature which holds that "emotions and passions are the mainsprings of human actions"[11] and, as a consequence, the disputes and conflicts among people cannot be resolved by arguments. Any acceptance of the idea that thoughts are determined is a feature of irrationalism, too (like traditionalism, 'tribalism', etc.). So, whoever holds that thoughts are determined by social relations is to be taken as an irrationalist just like those who reduce the different ways of thinking to the characteristics of a race. At last, do not forget that in Popper's opinion historicism is another feature of irrationalism.

Popper defines rationalism simply as a practical attitude of readiness "to listen to critical arguments and to learn from experience".[12] Rationalism or the attitude of reasonableness is the belief that by means of argument and careful observation people may reach agreement. It rests on the scientific attitude which in the search for truth and objectivity requires cooperation. It is essential that Popper, in sharp terms, raised the question of the possibility of establishing our choice between rationalism and irrationalism. His answer, briefly, consists in admitting that, since no ra-

tional argument has an effect on those who do not accept a rational attitude, the choice of rationalism cannot be established rationally. Irrationalism, however, can be based on rational argument. The adoption of rationalism can only be the consequence of an irrational decision or a moral choice. (The adoption of rationalism "may be called 'irrational'," it may be described "as an irrational *faith in reason*.")[13] Critical rationalism is but the admission of this impossibility to ground rationalism.

I intend to demonstrate in this paper that rationalism can be grounded without any concession to irrationalism. ('Critical rationalism' is called by Popper himself a 'minimum concession to irrationalism'.) In order to do this I should like to point out that Popper's concession follows directly from Popper's notion of rationalism and from the way he formulates the question relating to the possibility of establishing rationalism.

3. Before I adress myself to this task some remarks have to be made also on the different notions of historicism and antihistoricism.

3.1. Historicism rather than modern science is a product of modernity. In general it means nothing other than the conquest of the universe of history in the sense of maturing the conditions of scientific historiography. We must emphasize the importance of the birth of a historical outlook in the framework of which the past, earlier opposed to the enlightened present, no more appears as an epoch of darkness, a conglomerate of confused individual actions, and can be regarded as the antecedent of the present, something which discloses an intelligible pattern. Historicism did not represent any kind of irrationalism, it expanded the domain of rationality.

3.2. Historicism means, of course, the legitimation of the philosophy of history as well, or a system of certain methodological and epistemological assumptions. On the level of epistemology, e.g., we may call historicist the assumption that, by knowing the

genesis, the genealogy and the history of the development of a thing, we know the thing itself.

3.3. The thesis that history has some objective pattern is usually considered a historicist assumption, too. Such an assumption can be expressed by the statement that there are specific historical laws which explain not only static states of affairs, but also transitions from one state of affairs to another. As we have seen above, the assumption of the existence of historical patterns may involve the belief that logical relations are inherent in history, and that logical categories are at the same time historical.

3.4. But, ironically enough, historicism is also a name for the conception according to which in history there is no pattern and, as a consequence, the existence of any historical law must be denied. This sense of historicism is mainly involved in the *Geisteswissenschaften* of the early 20th century. This is epistemologically reflected in the opposition of scientific *explanation* and historical *understanding* (Droysen, Dilthey) and, in general, in the sharp distinction between the natural sciences and history.

3.5. The latter distinction has two forms: (a) historical knowledge is an original, independent and autonomous form of human knowledge; (b) historical knowledge is the *unique* form of human knowledge. (This is Croce's absolute historicism.)

3.6. The different senses of historicism we have listed indicate that it is impossible to identify a unique, homogeneous historicist current. So, it is illicit to form a unique, comprehensive judgement about historicism. Popper, however, attempted to elaborate a comprehensive critique of it. In order to do this, he had to construct, on the basis of different historicisms, an aritificial, general notion of historicism within which he distinguished, as subtypes, the antinaturalist and the pronaturalist trends.

Since historicism has so many meanings, no wonder that we have different kinds of antihistoricism which have different ways of constructing the structure of the theories they reject. The most striking examples are Popper's and Althusser's. Popper rejects

Marx as a historicist who professes an oracular philosophy, whereas Althusser qualifies Marxism as a radical antihistoricism.

4. I do not pretend that it is easy to tidy up this conceptual confusion. On the basis of the foregoing summary, however, it can be said with certainty that there is a kind of historicism, or a sense of it, which cannot be opposed to rationalism. In other words, whoever is committed to rationalism might adopt certain historicist claims without being inconsistent. And what is more, we need the teaching of historicism for the grounding of rationalism.

What historicist claims is it reasonable to accept?

4.1. It is just the rationalist who can hardly deny that history has some pattern. For a scientific historiography this is the minimum assumption by means of which it can be distinguished from mere chronicle. (In what this pattern consists, to what extent it depends on the historical interpretation, and by means of what conceptual scheme it can be grasped is, naturally, an open question.)

4.2. The claim that history has an objective pattern does not amount to stating that logical necessity reigns in it. Of course, history in itself is not rational, nor is it logical, since it cannot be pictured as a thinking subject (e.g. in Croce's sense, as the very spirit "forever individualizing itself").[14] For this very reason there is no 'historical inevitability'.

However, it is reasonable to accept a moderate interpretation of the unity of 'historical' and 'logical'.

Even if there is no historical inevitability, there are historical states of affairs for which it is true that the states of affairs preceding them are their necessary conditions. If we envisage them from the point of view of their essential structure then the temporal relation between them can be described as a kind of logical relation. Accordingly, there are succeeding states of affairs, a, b, c, ..., such that a precedes b and a is a necessary (though not necessary and sufficient) condition of b, but b is not a necessary condition of a, nor does it precede a. Assuming this to be a chronological sequence, it can be regarded as a developmental order. (Let us no-

tice by the way, that we have referred to necessary but not necessary and sufficient conditions. It has not been stated that if *b* presupposes *a* then, in case *a* obtains, *b* inevitably obtains, too. Let us notice, equally, that we have not referred to causal relations which hold between singular events and do not hold between comprehensive historical formulations or totalities such as feudalism and capitalism. Feudalism is a necessary condition of capitalism, but it is not one of its causes.) The picture outlined here can be instantiated by the cumulative development of technology (the technology of the Stone-Age logically precedes the use of metals). And we need not be technical determinists in order to recognize the essential role of technology in the life process of societies.

4.3. On the other hand, historicizing reason, claiming that reason itself is historical, apparently runs counter to rationalism. Certainly, a dogmatic rationalist is not ready to accept the claim of the historicity of reason. There is no place for arguing in its favour but, from the point of view of a critical rationalism, its importance must be evident. It protects us from universalizing a particular form of rationality, a single paradigm of science, the rationality of a given culture or form of life, and from the intellectual and cultural intolerance implied by such a procedure.

4.4. Finally, it is reasonable to adopt the historicist claim as to the autonomy of historical knowledge. In the debate about the 'covering-law' model of scientific explanation it has been confirmed that the unlimited extension of the deductive-nomological explanatory scheme is a form of universalizing a particular type of rationality. Instead, it is suitable to embrace the idea of the plurality of scientific explanations.

5. Let us now turn to the question of the possibility of grounding rationalism. As a preliminary to the argument it will be pointed out that Popper's notion of rationality is deficient.

5.1. Its deficiency lies in its being too narrow. The readiness to accept critical arguments is, of course, an essential element but just one element of the rational attitude, since only its communicative

aspect is involved in critical argumentation. However, the question of rational attitude can be raised on the level of instrumental action, too, a level which involves the examination of the adequate relationship between means and goals (Weber). It is to this level that Sidney Hook refers in saying: "there are working truths on the level of practical living which are everywhere recognized and which everywhere determine the pattern of reasonable conduct in secular affairs, viz., the effective use of means to achieve ends."[15] But the effort to harmonize the needs and the means of satisfying them, or ranking the conflicting interests (e.g. the immediate and prospective ones) also belong to rational attitude (Marx).

Popper's definition is too narrow also because he describes the rational attitude rather as an intellectual one. Rationality, both in view of its communicative and its instrumental aspects, is interwoven with the material activity, the working and productive processes. (It manifests itself in the organization of the division of labour, in a given economic structure, etc.) Popper's notion offers no basis for an analysis of such, sometimes conflicting, forms of rationality as 'economic rationality', 'technological rationality', etc.

5.2. The situation of the choice between rationalism and irrationalism is left completely undetermined in Popper's description. It is described as if it were free from any constraint and took place no matter where and when, with equal chances. What is described in this way holds true, at best, for the scientist who, sitting at his desk, must choose from abstract theoretical doctrines. Although argumentation is a form of cooperation (recommended by Popper), this kind of notion of rationalism is wholly individualistic. (It must be recalled also with respect to Goldmann's position that epistemological and ethical individualism does not characterize all forms of rationalism.)

5.3. Popper is right in saying that rational arguments have an effect only on those who previously adopt the rules of rational argumentation. However, it does not follow from this that rational-

ism rests on irrationalism unless we adopt a limited notion of rationalism and ignore that rationalism has different forms and different levels in depth, too. Besides, the dichotomy of rationalism–irrationalism is too narrow.

5.4. It is hard to see why the theory about the social determination of thought (and accordingly, also of the rational and irrational attitudes) must be taken to be irrationalistic. The thought which, becoming aware of its own limits, reveals its dependence on social structures, too, is more critical and self-critical than the thought which projects itself into the vacuum of a total independence.

5.5 Finally, Popper's critique is self-destructive because of his concession to irrationalism. If, like Popper, we accept that, in adopting a theory its consequences must be taken into consideration, then we have to search for other alternatives just because of this self-destructive consequence.

Another consequence of Popper's critique is its unsuitability to legitimate the use of the practical syllogism in explaining actions. In accepting it, in fact, we ought to abstain from rational explanations in which it is assumed that the agent does what he thinks to be rational. So, in a number of cases it would be illicit to search for motives and reasons in explaining actions. (Such a case is the very act of choosing the rational attitude.) However, this type of explanation based on reasons and motives or the "rationale of actions" (Dray) is indispensable in history.

6. In the course of the subsequent argumentation the following two statements will be distinguished:

(1) Rationalism can be grounded.

(2) Rationality has a foundation.

6.1. Popper's problem is related to (1), which is to be transformed into (1').

(1') There are motives such that if human beings rely on them in choosing rationalism then their behaviour is not to be taken as arbitrary (irrational).

(To this it could be added that insofar as human beings become aware of such motives they are likely to choose rationalism. In order to avoid complications, this further extension of the argument will be put aside.)

In this place I argue in favour of (2) and assume that ascertaining (2) is sufficient for accepting (1') and with it (1). Hence, I state that rationalism can be grounded because rationality has a foundation.

The historicist assumptions adopted above can be used as premisses. Taken together they have this meaning: history in itself is not rational, but it is the foundation of rationality.

(Incidentally it may be added that saying that history is not rational does not amount to saying that it is irrational. On the level of the problem of rationality there is no dichotomy corresponding to the dichotomy of rationalism–irrationalism. What is not rational cannot be taken without further ado as irrational. The problem of the foundation of rationality is comparable to the problem of the foundation of knowledge. The foundation of knowledge is not something we know, nor is it something we do not know: it does not belong to the domain of the conceptual distinction between knowing and not-knowing. Analogical remarks apply to the foundation of rationality.)

In consequence of the peculiarities of their historical existence human beings are rational. Before the question of choosing rationality *consciously* would in fact arise their activity, primarily their working and productive activity, has already rational features even if they are unaware thereof. The degree of this unconscious rationality has a tendency to continuous growth in history as it is shown by the growth of technological rationality. (It is, by the way, one of the meanings of the claim that reason is historical.)

Both the communicative and the instrumental aspects of rationality are built in the structure of human activity which, by its very nature, depends on harmonizing means and ends, is subjected to a continuous self-correction, and is cooperative. Whoever uses lan-

guage is a critical rationalist since, by the very act of communication, he/she presupposes the intersubjective validity of the rules of communicative cooperation. (This remark also reveals that Popper's notion of 'critical rationalism' is simply vacant.) Therefore, whoever in the situation described by Popper chooses between rationalism and irrationalism is already rational (being able, e.g., to argue rationally for his choosing irrationalism).

Referring to history, human activity, language and, above all, to the historicity of reason is sufficient for accepting (2). It follows that rationality is not a question of choice whereas rationalism and irrationalism can indeed be a matter of choice (mainly as regards highly abstract philosophical doctrines). Whoever chooses rationalism relying on the knowledge of (2) and the arguments supporting it, chooses what human beings have become through history. To put it pathetically, he chooses himself. The choice of rationalism is an interest of human beings (which, of course, can be recognized or not). Whoever chooses irrationalism denies history, refuses what human beings have become in the course of history.

Consequently, there is a motive which, if it is relied upon, can give the grounds for choosing rationalism against irrationalism. *QUOD ERAT DEMONSTRANDUM.*

6.2. Let us add some comments on 6.1.

From what the choice of irrationalism means, it follows that Lukács is right: irrationalism cannot have a comprehensive history. It is indeed a reaction (a reaction to a previous historical and intellectual development).

The situation of the choice between rationalism and irrationalism we analyzed following Popper is, of course, an idealization. I tried to show that rationalism is to be preferred even on this highly idealized level. As to the real situations Lukács is right: "The choice between *ratio* and *irratio* is not an 'immanent' philosophical question. The choice of a thinker, primarily, is not determined by speculative, philosophical reflection, it is determined by class relations and class constraints."[16] This answers at the same

time the question how it is possible at all that some people choose irrationalism.

In Section 6.1. I did not demonstrate that it is impossible to choose irrationalism. What has been shown is only that it is not rational to choose it. Certainly, rationalism can be chosen on the basis of an 'irrational faith'. My point has been that, if certain conditions obtain, the choice of rationalism is grounded and may be called rational. What has been said means, in the last analysis, that rationalism is humanism and historicism – in contrast to irrationalism, which is not.

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NOTES

[1] J.-J. Rousseau, 'Discours sur l'origine et les fondaments de l'inegalité parmi les hommes', in *Oeuvres complètes de Rousseau*, Firmin-Didot, Paris MDCCCLXXVI, I, p. 547.
[2] R. G. Collingwood, *The Idea of History*, Oxford Univ. Press, London–Oxford–New York 1970, p. 109.
[3] I. Kant, *The Critique of Judgement*, trans. J. C. Meredith (Great Books of the Western World 42), § 74.
[4] "Die Geschichte ... läßt von sich hoffen: daß, wenn sie das Spiel der Freiheit des menschlichen Willens im Großen betrachtet, sie einen regelmaßigen Gang derselben entdecken Könne." Kant, 'Idee zu einer allgemeinen Geschichte in weltbürgerlichen Absicht', in *Kants Gesammelte Schriften*, Bd. 8, Verl. von G. Reimer, Berlin 1912, p. 17.
[5] B. Croce, 'Logica come scienza del concetto puro', in Croce, *Il concetto della storia*, Laterza, Roma–Bari 1975, p. 47.
[6] B. Croce, *Teoria e storia della storiografia*, Laterza, Bari 1927[3], p. 73. ("La soluzione guista è quella del progresso non come passaggio dal male al bene ... ma come passagio dal bene al meglio, in cui il male è il bene stesso, visto alla luce del meglio.")
[7] Cf. G. W. F. Hegel, *Enzyklopädie der philosophischen Wissenschaften*, Akademie-Verl., Berlin 1966, § 6. ("Was vernünftig ist, das ist wirklich, und was wirklich ist, das ist vernünftig.")
[8] B. Croce, *Teoria e storia della storiografia, op. cit.*, p. 77. ("La storia non è mai giustiziera, ma sempre giustificatrice.")
[9] G. Lukács, *Die Zerstörung der Vernunft*, Aufbau-Verl., Berlin 1954, pp. 82–83. ("Es folgt ..., daß der Irrationalismus ... unmöglich eine einheitliche, zusammenhängende Geschichte besitzt, wie man von einer Geschichte des Materialismus oder der Dialektik sprechen kann.")

[10] G. Lukács, *Op. cit.*, p. 77. ("Das Gleichsetzen von Verstand und Erkenntnis, von Schranken des Verstandes mit Schranken der Erkenntnis überhaupt, das Einsetzen der 'Übervernünftigkeit' [der Intuition usw.] dort, wo es möglich und notwendig ist, zu einer vernünftigen Erkenntnis weiterzuschreiten – das sind die allgemeinsten Kennzeichen des philosophischen Irrationalismus.")

[11] K. R. Popper, *The Open Society and its Enemies*, Vol. II, Harper and Row, New York and Evanston 1963, p. 233.

[12] *Op. cit.*, Vol. II, p. 225.

[13] *Op. cit.*, Vol. II, p. 231.

[14] B. Croce, *Teoria e storia della storiografia, op. cit.*, p. 87. ("La storia ... è l'opera di quell'individuo veramenta reale, che è lo spirito eternamente individuantesi.")

[15] S. Hook, 'Naturalism and First Principles', in Hook, *The Quest for Being*, St. Martin's Press, New York 1961, p. 173.

[16] G. Lukács, *Op. cit.*, p. 79. ("Schon daraus ist ersichtlich, daß die Wahl zwischen Ratio und Irratio nie eine 'immanente' philosophische Frage ist. Über die Wahl eines Denkers zwischen dem Neuen und Alten entscheiden primar nicht gedankliche, nicht philosophische Erwängungen, sondern Klassenlage und Klassenverbundenheit.")

BALÁZS DAJKA

TWO USES OF FUNCTIONAL EXPLANATION

The aim of the present paper is to take up an old issue, all too familiar in the history of social science, and contribute a point of view which is not strikingly new but, nevertheless, seems to be in need of explicit formulation. The problem, that is, that of functional explanation, has been thoroughly discussed in sociological and philosophical literature, though much less in related disciplines such as psychology and even ethnology. Indeed, in spite of prolonged debates over functionalist social anthropology, anthropologists themselves were scarcely interested in a detailed analysis of the type of explanation they were using, however much they were discussing other theoretical questions. There was a period when philosophers seemed to lose interest in functions, too, but that trend did not last long and now, for the past five or ten years, we have witnessed a revival of pertinent investigations.[1] To some who are more sceptical, however, the credit of any kind of functionalism seems to be exhausted, and the very notion of a functional type of explanation extinct. For example, Anthony Giddens fears that "to raise the question of functionalism is almost enough to put everyone immediately to sleep". He suggests that "the term 'function' ... is of no use to the social sciences or history; indeed it would do no harm at all to ban it altogether as any sort of technical term". He also concludes that "there is nothing which can count as 'functionalist explanation'" (Giddens 1981, pp. 15–17).[2]

All the same, Giddens does not ignore the merits of the approach he criticizes, and his criticism will not be ignored in what follows. While attempting to contribute to a defence of functionalism with some restrictions, I will raise the further critical point that many instances of functional explanation, as they occur in so-

Imre Hronszky, Márta Fehér and Balázs Dajka (eds), Scientific Knowledge Socialized, 365–374.
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ciological, anthropological, psychological, etc. texts, display a confusion as to what exactly they are meant to explain, or whether they explain anything at all. At the time when B. Malinowski and A. R. Radcliffe-Brown developed the first functionalist theories in social anthropology, the sociologist Karl Mannheim established a difference between causal explanation and functional interpretation, from which it follows that he was not offering an explanation when he referred to functions. In this respect, his standpoint was close to that of the anthropologists, who also wished to provide reconstructions different from causal explanation. Mannheim's concept of cause is linked to that of an event, to which he thinks a function can also be attributed in a different context. "In the causal view we seek to *construe* an event through as many of its determinants as we are able to isolate. The final construct is largely an approximation of the actual event, and when the approximation is close enough for a given purpose we say the event is *explained.* On the other hand, we *interpret* the same occurrence if we detect its function in the equilibrium of the whole system in which it takes place." (Mannheim 1956, p. 76.)

Several years after the emergence of functionalism, philosophers of science submitted its main core to rigorous methodological analysis and dismissed it as unscientific unless it was incorporated in some model of causal explanation. That is to say, the above types of attempts at referring to functions in interpreting, not explaining, social events or facts failed the test of one sort of scientific methodology. It was only after the decline of the logical positivist tradition that the clarification of the functional frame of reference could take a new turn. A radical change ensued when functions were shown to be of use not only for interpreting, but also explaining, social occurrences. This of course presupposed a special model of explanation, which was nevertheless drawn up with reference to causes and, occasionally, to the possibility of prediction. Larry Wright (1973, 1976) and Gerald A. Cohen (1978) worked out different conceptions of functional explanation

through the introduction of an important distinction between ascribing a function to some item and explaining the emergence or persistence of an item in terms of the function(s) ascribed to it. That distinction is essential in dealing with one important criticism of the functional approach, which runs, in Giddens's wording, like this: "functionalism ... (like structuralism) ... rests upon a false division between statics and dynamics, or between the synchronic and the diachronic" (Giddens 1981, p. 17). Now, this objection is justified when it is directed against the 'great theories' of some thirty or fifty years ago, and there it is destructive. But when it comes to more recent reconstructions like the ones mentioned, there is at least a hope that the objection becomes constructive, that is, integrated into further theoretical developments. My intention is to show that, provided we can speak of functional explanations, these are used in two special ways among possible others which can be identified as the synchronous and the diachronous use, respectively. While arguing for the discernability of those two aspects in explanations by functions I will also attempt to reveal the close link which unites the two, thus offering a kind of counterargument to satisfy critics of the admittedly one-sided synchronicity of outdated structural functionalism.

Related to the previous question of what is being explained are the further questions of what is regarded as a function in a given case, and what is supposed to have that function. These, I think, are among the most difficult problems with this kind of explanation, but no final decision about them is required for the present argument. In order to stay on secure ground, at the outset I will limit my enquiry to cases in which a function can be considered a beneficial effect, and such effects can only be attributed to particular events if we are to deal with causal chains. That is to say, in the flux of actual social processes, an event is pointed out as having some (perhaps potential) beneficial effect according to some criteria, and this effect (called a function) is invoked in giving a causal account of the emergence of the event in question. These

restrictions may seem both counterintuitive and defying received traditions of social research, but they greatly facilitate the treatment of functional explanation as a type of explanation by cause. In this way, any lengthy discussion of theories of causation can be avoided through conceding that only events are taken to have causes as well as effects, which themselves are taken to be events.[3]

As will be seen presently, to distinguish between the ascription of a function to an event and the explanation of the occurrence of that event (which may parallel the synchronic and diachronic approaches) is essential for a new type of causal explanation. The new model, introduced by Cohen, avoids many of the problems involved in various teleological provisions so common to accounts of functionalisms of different sorts. This does not mean reducing teleology to causality. In fact, it must be mentioned in advance that 'causality' does not appear here in a simple and direct way, therefore Cohen's model ought to be considered a very special type of causal explanation, if it is convenient to label it so at all. However, this question is not vital for the present argument, for the term 'diachronic' remains applicable to a greatly modified interpretation of causality, too.

At this point, before summarizing Cohen's thesis, let me return to my original problem of the synchronic and diachronic aspects of functional explanation. The element of causality is apparently related to the diachronic aspect, and if the latter was neglected by some schools of social science, this might be traced back to their neglect for causal explanations in the widest sense. A field anthropologist before the Second World War was concerned with observable items of behaviour, interpreting them as parts of a structured whole, and ascribing meaning to them with the help of a symbolic code. Social anthropologists were characteristically opposed to a quest for the origins or causes of the occurrence or presence of any 'cultural isolate'. If they asked why an item was there, they sought an answer stating its role, purpose or function in some cul-

tural pattern, to which the circumstances of the item's emergence (often fallen into oblivion) seemed utterly irrelevant.

Now, the diachronic and the synchronic approaches pose different questions and, consequently, advance different types of explanations. To refer to the above example, 'synchronic functionalists' like many field anthropologists did not wish to know *why* an event occurred. When they had observed it, they took it for granted, and their question was *what* the purpose, significance or function of the occurrence might have been. Cohen addresses himself to the problem of why-questions but he does not entirely follow Carl G. Hempel in his analysis. While Hempel implies that all genuine explanations must provide an answer to why-questions, Cohen considers other kinds of explanation as genuine, among them answers to what-questions. According to him, an answer to a what-question could be like "a good explanatioin of the structure of DNA" without explaining why anything is so (Cohen 1978, p. 252; cf. Hempel 1965, p. 334). Nevertheless, he restricts his model to why-questions, as a result of which his account seems to cover just the causal, or diachronous, aspect of functional explanation. Later it will become clear that he assigns a crucial role to synchronous function statements, too. He does this in controversy with Larry Wright, who claims that functional ascriptions also serve to answer why-questions (Wright 1973, p. 154).

In Cohen's construal, a genuine functional explanation (answering a why-question) is structurally identical with a consequence explanation, which involves a statement assigning consequences to items (such as events) in a certain way. That is to say, "a consequence-statement explains when it relates to a *consequence law* in whatever way an explanatory precedence-statement relates to a pertinent law" (Cohen 1978, p. 263). This amounts to embracing a version of the deductive-nomological model of scientific explanation, with special reference to a consequence law. The latter is "a universal conditional statement whose antecedent is a hypothe-

tical causal statement. A consequence law relevant to the explanation of an event ... takes this form:

IF it is the case that if an event of type E were to occur at t_1, then it would bring about an event of type F at t_2,

THEN an event of type E occurs at t_3" (Cohen 1978, pp. 259–260). This is applied to an example from anthropology like this: "Whenever performance of rain dance R would bring about, shortly thereafter, a rise in social cohesion, rain dance R is performed." It must be noted that this model of explanation is not causal in the sense that it would be the effect (event of type F) which were to bring about its antecedent cause (event of type E). It is indeed clear that an event of type E brings about an event of type F, as in the example the rain dance brings about a rise in social cohesion. It is also obvious that social causation is more complex than that: a dance is rarely the unique cause of a change in social cohesion, both factors being analyzable into chains of mediating events. What explains here is a dispositional fact about the society: in the example, the fact that the condition of society is such that, at the given moment, the performance of rain dance R would bring about, shortly thereafter, a rise in social cohesion. It is not for us here to decide whether a dispositional fact can be considered the cause of a social event. In any case, whether causal in the genuine or only in some restricted sense, the above model of functional explanation has a diachronic character in that it accounts for the occurrence of certain types of events in the course of time. It makes explicit the claim underlying the much-criticized, traditional type of function statements in different branches of social science to the effect that an item having a certain (alleged) function is there, or persists, because of its having that (latent or manifest) function. At the same time, it must be remembered that it does not declare a function to be the cause of the event to which it is attributed. This would imply 'backward causation' or other mechanisms which would not easily be incorporated in such a model.

Within this necessarily curtailed sketch of a diachronic type of functional explanation it is relatively easy to find a place for the other, synchronic, type. One possibility is to discover it embedded in the minor conditional in Cohen's consequence explanatory model, where the event of type F brought about by an event of type E corresponds to E's function. But the ascription of a consequence to an event is not always the same as the ascription of a function, and the difference is not borne out by the structure of the model itself. Most probably, this would rather be determined by the disposition of the larger context (e.g. the society) which is invoked as the basis of explanation. This account of function statements which, to be sure, treats them as mere parts of a more complex model, lends itself to interpretation in terms of both synchronicity and diachronicity, as I hoped to show throughout. The apparent contradiction between the two approaches is resolved in one unified framework, in which both function statements (structural or static aspect) and dispositional consequence statements (causal or dynamic aspect) contribute to an account of the occurrence of function bearers. Without a function statement, such an account could not refer to an event precisely as a function bearer.

Arguing now for the recognition of the explanatory status of function statements seems to me necessary because of the preceding argument in the other direction, that is, in favour of interpreting dispositional consequence explanations as answers to why-questions. Here there are two alternatives: one can, with Cohen, contend that what-questions can also be explanatory, or say with Wright that function statements answer why-questions, too (Wright 1973, p. 154). In either case, it will become possible to regard the full structure of consequence explanations as composed of two statements (a function ascription and a consequence explanation encapsulating it), each of which may possess explanatory power, if only of different kinds.

If answers to what-questions are regarded as explanatory, this need not mean that the pure synchronic approach, until now the

target of severe criticism, will be allowed to regain force. Rather, this would imply a concept of explanation which is not as close to the deductive-nomological model as the one Cohen adopted. Once we are allowed to depart from that model we discover that, apart from a rather small segment of the rich social scientific literature, it cannot be readily applied to all texts in which functions or consequences are mentioned. However, if the whole model of consequence explanation is not always applicable (it will serve better future investigations), that part of it which corresponds to a function statement proves of more general use and may in fact be incorporated in some model of understanding. Even if the above concept of explanation is replaced by another one or, to change the term, by that of understanding, the scope of the function statements involved need not only be synchronical. It is well known that 'understanding' is often meant to retain the historical perspective. To quote the hermeneutic position, "understanding means a fundamental apprehension of truth which takes place in intersubjective processes of communication and in the mediation through history" (Bubner 1976, p. 69). We may also consider here Lucien Goldmann's insight that "if all behaviour is significant and functional in so far as it is directed towards an equilibrium, that does not, of course, mean that it will be able actually to achieve this equilibrium, since, quite apart from the external obstacles which it may encounter, the surrounding world changes as a result of the actions of the various collective subjects and therefore the situation changes, so that what was functional and significant at a given moment ceases to be functional and gives way to a different functionality. This transformation – which is, of course, a continued process, characterized in the move from one set of structures and functions to another by sudden changes – implies a synthesis of continuity and discontinuity which Hegel called the move from quantity to quality." (Goldmann 1971, p. 76.)

One final observation could be made about Cohen's concept of a 'consequence law'. Certainly, it satisfies the requirements of his

model (though not necessarily those of a deductive-nomological one). It could be objected, however, that "social 'laws' are not generalizations from experience but conceptual schemata for the interpretation of concrete historical situations" (Von Wright 1976, p. 434).

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NOTES

[1] To assess the importance of the shift to new approaches, compare Sztompka (1974), and Van Parijs (1981).

[2] It has been the special fate of functional approaches to have been often denied the status of a distinct method while being advocated as a general point of view. Weber declares that the "functional frame of reference is convenient for purposes of practical illustration and for provisional orientation", hardly more (Weber 1968, p. 15). Another famous statement reduces functional analysis to a myth "because in its essence sociological method (save some reductionist approaches) is functional." (As Sztompka 1974, p. 41 summarizes Davis 1959.)

[3] The risks of such a restriction naturally depend on the domain of investigation. In his treatment of human action, for example, Raimo Tuomela (following Donald Davidson) also focusses on singular events, though extending the concept over short-term states (Tuomela 1977, pp. 12–13). But when one's subject matter is long-term social processes events are more difficult to handle. Philippe Van Parijs revives the notion of social fact, and thus he needs to state that "functions are typically attributed to *persistent features*, whether organs, ongoing processes, artefacts, behaviour patterns, etc., rather than to events" (Van Parijs 1981, p. 41). Here he means to contradict Cohen, who brings events, among many other items such as properties, manners of behaviour, etc. under the scope of functional explanation. Cohen, in turn, rejects Merton's limitation of functional items to what is "standardized, i.e. patterned and repetitive", unlike singular events (Cohen 1978, p. 296, Merton 1968, p. 104).

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N. L. PORUS

INCOMMENSURABILITY, SCIENTIFIC REALISM AND RATIONALISM

The mainstream of discussions within contemporary philosophy of science from the sixties may be provisionally divided into two branches: on the one hand, there are debates concerning the normative methodology of science in its relation to the 'actual' history of science; on the other hand, a complex of problems is raised concerning the interrelation of theory and reality ('the external world'). Within the first branch, principal attention has been attracted by the polemic between the 'critical rationalists' and the proponents of the 'historical approach'; within the second branch, discussions between the 'scientific realists' and the intstrumentalists has moved to the forefront. Towards the beginning of the eighties, the opponents seem to have exhausted all the arguments and even innumerable 'corrections' to their positions have failed to bring closer the opposing sides. This is hardly surprising, since the debates concern the very first principles which govern philosophical investigation into the process of acquiring knowledge in general, and scientific knowledge in particular.

In the final analysis, the argument resumes old philosophical disagreements: what is truth, what is rationality and the progress of knowledge? J. Kekes names them 'enduring problems'[1] and he may be right. At the same time, we should not be tempted by the position of the sceptic, to whom it makes no difference who will take the upper hand in yet another battle of words since there is no hope for a definitive answer to the problem anyway. The philosopher's task is to seek answers to these questions and stand by his own convictions.

Although, as I have said, no compromise has yet been found, and debates do little more than discover new layers of philosoph-

Imre Hronszky, Márta Fehér and Balázs Dajka (eds), Scientific Knowledge Socialized, 375–383.
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ical problems, I do not regard the situation as hopeless. I would rather expect the eighties to become a period of getting the broken pieces together rather than further scattering them about.

Let us take up, for example, a crux of the sixties and the seventies, the problem of the 'incommensurability' of scientific theories formulated within different paradigms (in the sense of T. Kuhn and P. Feyerabend). Quite a few polemical spears have been broken around this problem. Some claimed off hand, to the detriment of the logical and semantical canons of rigour, that the outgoing theory and the theory which comes in its place are incommensurable, which led to a hasty conclusion that a change of theories in the course of scientific evolution cannot be described in logical terms. Others protested that this was tantamount to irrationalism, and rushed with equal haste to save rationalism by means of picking holes in the arguments of the partisans of 'incommensurability'. A flood of publications overwhelmed philosophical journals, calling forth ill-humoured responses by scientists, who were right in pointing out that if philosophers are earnest in putting to trial the idea of scientific progress, then theirs is a flippant or even harmful philosophy, worthy of being discarded right away. However, when the pathos petered out, it became gradually clear that behind the ill-formulated problem of 'incommensurability' hides an important and non-imaginary problem of scientific rationality, best to be grasped in the form of a question: Can the process of development of scientific knowledge undergo 'rational reconstruction', and if it can, what sense can we make of the concept of the 'rational' within the given context? Analysis of this problem should, to my mind, convince the conflicting sides that the notion of 'incommensurability' is in itself an artificial construct, performing a rather psychological function of prompting philosophers and methodologists to look for a more 'realistic' conception of scientific rationality, irreducible to pure logic. And this is the kind of search which could unite even the hardiest opponents, through the coincidence of their ultimate interests and insights.

Let us now consider the thesis of 'incommensurability'. Putting particulars aside, one can see that it stands upon two principal premisses: (i) there is no such thing as a 'theoretically neutral' language of observation, into which statements of fundamentally different theories could be translated; (ii) such statements relate to entirely different objects, even if these objects are designated by terms having the same lexical 'envelope'. Although partisans of the 'incommensurability' thesis are not too eager to talk about 'meanings' (Feyerabend calls such talk 'semantic chatter'),[2] I would still venture to say that premiss (ii) rests upon the 'holistic' conception of meaning, according to which the meaning of a term is defined by its function in a logically and semantically closed system. Within such a system, any change, however insignificant, leads to a change of meaning of all its terms (K. Ajdukiewicz in the thirties called such systems 'mutually untranslatable', and it is not without ground that some authors point out some conceptual affinity between the so-called 'radical conventionalism' of K. Ajdukiewicz and the contemporary ideas of T. Kuhn and P. Feyerabend);[3] therefore, if 'comprehensive' theories are regarded as such systems, then any pair of theories T_1 and T_2 consists of either 'identical' theories or 'incommensurable' ones. In any case, the two theories are 'compatible', since no sentence formulated within any of them can negate any other.

It is well known that 'incommensurability' has been functioning as the pivotal point in arguments against the possibility of logical transition from T_1 to T_2; hence the attention to the social psychology of scientific communities (T. Kuhn) and P. Feyerabend's slogan 'anything goes!' (reminiscent either of a Biblical quotation, or of Karamazov and Smerdiakov's 'all is allowed', or of a complacently utilitarian attitude in the sense of the Russian proverb 'что в рот полезло, то и полезно', an approximate equivalent of the English 'all is well that ends well'); hence also the tendency to interpret the 'interplay of factors' within the 'external' and 'internal' history of science as something analogous to the struggle

for survival, enacted not by biological species, but by 'populations of concepts' (S. Toulmin). One may resume the positive thought behind all this as follows: if your 'rational reconstruction' of the history of science aims at subjecting it to a certain scheme of understanding and presenting it as a process displaying certain immanent trends, continuity, and regularity – so far so good! It would be wrong, however, to reduce these immanent trends to elementary formulae of logical deduction, and to eliminate the concrete individuals realizing this process under historical conditions, which are also concrete: a theory of scientific rationality in which rationality is equated with the observance of logical rules is irrational and ahistoric.

Does a single ambiguously formulated thesis justify all this uproar? As a matter of fact, it is for two decades that critics have been refuting the concept of 'incommensurability'. Naturally, the task is best fulfilled through the demonstration of the falsity of the premisses on which the concept rests. Some authors try to show that the 'theory-ladenness' of the language of observation is not an insurmountable obstacle, since it is possible at any time to single out 'theoretically independent' measurements and observations enabling us (for any given pair of juxtaposed theories) to determine the preference of one theory over another.[4] Some say (and rightly so, one should concede) that the 'holist' conception of meaning cannot be applied to the actual scientific theories of the present and the past.[5] Other authors assert that appealing to social and psychological factors means 'disseminating vulgar sociology' and comes from elementary logical incompetence (which is blatantly untrue).[6] Serious criticism and frivolous attacks, refutations and redefinitions follow one another, and yet the hazy and repeatedly refuted thesis of 'incommensurability' continues to attract the attention of philosophers and methodologists with very different views. Ineradicalibility of a myth? A revival of Scholasticism? Or is here a strong feeling at work, one that doubts concerning the universal acceptability of the traditional views on the

problems of scientific rationality could not be eliminated even if arguments in favour of 'incommensurability' were shown to be built of sand?

Does not the feeling we have mentioned provoke the numerous attempts to demonstrate that the scientist's choice remains in a sense rational even if the 'incommensurability' thesis is true, since the reasons for his choice have an objective character, even if they cannot be reduced to elementary inferences which are valid only if the choice is being made between 'commensurable' theories? Such is W. Sellars's and J. Rosenberg's 'explanativism': the criterion of choice is the explanatory superiority of one theory over another (although there is no consensus as to exactly how this superiority should be interpreted).[7] Such is N. Rescher's 'methodological pragmatism': the rational choice is based on the success of methods typical of the theories being compared.[8] There are proposals to 'measure' theories on the basis of such criteria as 'better predictability',[9] 'better problem-solving capacity',[10] and 'syntactic and structural advantages of mathematical apparatus'.[11] Recently J. Sneed and W. Stegmüller's approach has become widely known: a way towards a rational representation of the process of scientific evolution is paved with the help of a 'non-standard' ('non-statement') scientific theory model, in the construction of which methods drawn from set theory and model theory are used.[12]

Each of these approaches has its values and its handicaps. The values are publicized by the authors, the handicaps are quickly discovered by the critics, thus making the polemic thrive. We do not intend here to enter the details of the discussion, and we shall limit ourselves to the following observation: participants of the discussion follow ideas of scientific rationality determined by their various philosophical positions. As a matter of fact, what we observe here is not a contest of different methodological models within the same philosophy but a conflict of different philosophies.

Let us consider for example the attitude of the 'scientific realists' and instrumentalists towards the problem of 'incommensurability'. The instrumentalist will naturally acknowledge a 'cardinal shift of meanings' in the transition from one paradigmatic theory to another. For the scientific realist to assert this is to plunge into serious difficulties: since theories describe reality, and are not just a means of organizing our experience, or of predicting and explaining phenomena, the 'incommensurability' thesis implies a plurality of 'realities' hardly acceptable to 'scientific realism'. Looking for a compromise, the 'scientific realists' sometimes say that 'incommensurable' theories should be treated as complementary pictures of one and the same Reality. But, first, if we favour the opinion that each theory 'projects' its own ontology, then the claim that different theories relate to the same world of objects becomes unprovable (such 'metaphysical' realism is rejected by many 'scientific realists').[13] Secondly, the question of the relative validity of different ontologies tends to turn into the problem of theoretical truth. It is here, to my mind, that 'scientific realism' has its Achilles' heel, being condemned to try again and again to solve the problem in terms of the correct usage of language within a given theoretical system, discarding as a 'metaphysical prejudice' any suggestion of a direct adequacy between a theory and the World (one should point out here that for some obscure reason, this adequacy is always treated in terms of the naive 'correspondence' theory of truth). This inevitably leads to an assertion of the plurality of truth,[14] which is very close to the same conclusions as are to be derived from the 'incommensurability' thesis, the first of these conclusions being the impossibility of the rational reconstruction of a transition from theory to theory.

Such conclusions would evidently destroy the foundations of 'scientific realism' and would make it indiscernible from both moderate instrumentalism and outspoken relativism. In order to mend matters, H. Putnam has proposed a reformed theory of

meaning: a relatively stable 'nucleus' is singled out within the complexity of meaning components, representing what is common in all the 'correct usages' of the term, against the relatively labile 'belt' of pragmatic components; the 'nucleus' is the 'centre of convergence' to which changing meanings are attracted in the course of history. Such convergence, Putnam says, is synonymous to scientific progress ('convergent realism').

The 'nucleus' of the meaning as proposed by H. Putnam is of course no more than a 'shy analogue' to the 'objective reality', conceived of in C. Peirce's sense as the 'regulative ideal' of knowledge, the latter being an infinite process satisfying the rules of scientific methodology. Such a conception is obviously liable to criticism from the position of dialectical materialism. But I would like to point out another side of the matter. The conception of meaning proposed by H. Putnam must inevitably appeal to the history of the formation and development of notions, since only through the historical approach is it possible to detect those components of meaning which are not discovered through 'traditional' analysis and which are responsible for the continuous development of knowledge.

We are faced here with an interesting and instructive situation. Partisans of the 'incommensurability' thesis, too, appeal to the history of science to find justification for their 'discrete' image of science there. They draw attention to the 'radical shifts of meanings' which destroy the bridges of logical transition. Now, the 'scientific realists' seem inclined to say that these shifts only look radical and that this illusion comes from the *lack of historicity on the part of the proponents of 'historical epistemology'*, who compare 'paradigms' divorced from their historical context, force them to figure as universal semantically closed systems and discover 'gaps' between them, which can allegedly be bridged only by way of psychological 'Gestalt switches'. This does away with those components of meaning which are actually at work in any given historical

situation, organically incorporating subjective features and characteristics.

From this point of view, the inclusion of historically relative components into the structure of meaning implies not only a widening of analytical methods but, which is most important, leads to *a revision of the traditional understanding of scientific rationality*: any explanation of scientific choice must include pragmatic and similar components. Now this is exactly what adherents of 'incommensurability' insist upon!

Thus the search for a 'non-traditional' theory of scientific rationality widens the common ground and unites the efforts of even the most conflicting trends within contemporary philosophy of science. A deficiency they have in common, to my mind, is their failure to consciously take into account the achievements of dialectical thought, helpful in solving these problems which are dialectical in nature (although some efforts, with interesting results, are being made in this direction);[15] hence the constant bias in favour of relativism. Let us hope that this bias will be overcome. The philosophy of science of the eighties, one would like to believe, is on the eve of important changes: it is ready to pass from the piecemeal polemical discussion of problems inherited from distant times to the synthetic generalizations which are being prepared not only in the heads of methodologists, but by the very essence of the overall scientific and technological revolution of our days.

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DAVID PEARCE

THE PROBLEM OF INCOMMENSURABILITY: A CRITIQUE OF TWO INSTRUMENTALIST APPROACHES[1]

Contrary to the popular wisdom of the leading epistemologies and methodologies of science, the instrumentalist and conventionalist tradition has shown itself more sensitive than most to the problems of intertheory relations, especially to the intricacies of translation and commensurability. One might well argue that this has given it an edge in dealing with the awesome business of the growth of knowledge and in trying to give scientific progress a rational complexion. Epistemological and scientific realists, on the other hand, are apt to look askance at questions of logical comparability. They tend to ignore or at best underestimate the issue of intertheory translation, by postulating semantic or ontological stability and conceptual continuity in the development of science. Sometimes they dismiss the very issue of the logical comparison of frameworks, theories and conceptual schemes as being founded on a pseudo-problem.

Not all realists, of course, follow this line of approach. Paul Feyerabend made realism a precondition for facing up to the consequences of the incommensurability thesis and for accepting his 'methodology' of theory proliferation. Ilkka Niiniluoto has combined realism with a sophisticated and subtle account of conceptual change and intertheory relations. But two of the most celebrated champions of realism in epistemology and the philosophy of science – Karl Popper and Donald Davidson – have penned harshly-worded manifestos that warn against the "myth of the conceptual framework".[2] They each produce brusque, no-nonsense arguments for the classic, realist view that the truth of our theories is determined by the way the world is, not by our choice

Imre Hronszky, Márta Fehér and Balázs Dajka (eds), Scientific Knowledge Socialized, 385–398.
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of language or conceptual scheme. And the thesis is accompanied by a solid antirelativist reminder that the world is one, not many.

Instrumentalists, at least since the last century, have adopted a different stance. Frequently under the influence of Kant's philosophy, they have looked much more favourably on the view that truth is co-determined by language and a product of our mode of conceptualization as much as it is a feature of reality. The 'moderate' conventionalism of Duhem and Poincaré, in particular, focussed on questions of conceptual comparison, and the intertranslatability and equivalence of different laws and theories. Where they saw problems of comparability and communicability as essentially solvable, conventionalists of a starker breed, like Le Roy and Ajdukiewicz, remained sceptical. They upheld radical claims of non-translatability that can be reckoned as ancestors of the modern incommensurability thesis. We can even sketch out the shadow of a line connecting the conventionalist tradition with Kuhn's epistemology; a line that passes through the work of Ludwig Fleck which exerted such a striking influence on Kuhn.[3]

Kuhn is, I believe, correct in maintaining that the problem of incommensurability turns crucially on the issue of intertheory translation. He is also right in claiming that the problem does not vanish under the scrutiny of philosophical criticism, of the realist sort just alluded to, or any other kind. It will not dematerialize, in fact, until questions of translatability are squarely faced. Even then, the problem will not be resolved at a stroke; though it can, I think, be gently remedied, one case at a time.

Despite the current dominance of the realist view of science, some pervasive forms of instrumentalism have lately established a following amongst methodologists. The conception of science as a problem-solving rather than truth-seeking activity is one such brand of instrumentalism to have gained a certain foothold. Its leading light is Larry Laudan. A second approach with instrumentalist affinities is the structuralist view of theories, developed by Joseph Sneed and taken up with a vengeance by Wolfgang Steg-

müller. At first sight the two philosophies have little in common. One is predominantly historical, the other chiefly formal in emphasis. But each has proposed a novel 'solution' to the problem of incommensurability, and their responses draw to a large extent on their own particular type of instrumentalism. I argue in this essay that their solutions are erroneous because, unlike their instrumentalist forefathers, they neglect the central issue of intertheory translation. My arguments do not appeal to realism, nor do they imply that the realist view is better equipped to tackle the problem of incommensurability. On the contrary, I hope that some healthy criticism may help to strengthen the instrumentalist approach.[4]

It is important at the outset to realize that neither Laudan nor Stegmüller embraces instrumentalism in the strict sense of the word, such as Popper characterized and condemned.[5] Neither of them explicitly renounces the concept of truth in connection with scientific theories. And, indeed, one may surmise that they conceive of truth in roughly the classical, semantical sense. Laudan's instrumentalism has a methodological tinge. His claim is not that scientific laws and theories cannot be true-or-false, but that truth is not intrinsic to the *aim* of science, and ultimate or absolute truth is not the rational end-point of inquiry. The progress of science is measured rather by the ability of its theories to solve problems; that they may or may not achieve greater verisimilitude is quite beside the point. Stegmüller's instrumentalism, on the other hand, is embodied in his conception of what a scientific theory *is*. A theory is not a set of true-or-false propositions, because it does not consist of propositions at all. A theory, for Stegmüller, is a mathematical structure of a certain sort, together with a class of 'empirical' domains where the structure can be applied to make predictions and solve problems. Thus, it is not that he assigns no descriptive function to the abstract elements of a theory, rather this function is allocated not to individual sentences (or even sets

of sentences) but to the structure as a whole and its relation to its field of application.

Not being instrumentalists of the hardened kind, Laudan and Stegmüller make no attempt to sidestep the problem of incommensurability by antirealist measures of the sort that Feyerabend has scorned: distinguishing observational from theoretical terms and construing all theories with observational overlap as commensurable. All terms, for Laudan and Stegmüller, are impregnated with theory. The former's 'empirical problems' and the latter's 'non-theoretical terms' are specified in a context-or-theory-dependent way. It is true, of course, that Laudan believes that as a rule rival theories always share some empirical problems that can be characterized independently of their respective research traditions (though they need not be free of all theoretical commitment). But this is a part of his response that is better passed over in silence. At the same time, Laudan's and Stegmüller's 'solutions' to the problem of incommensurability have evident instrumentalist origins. Here is a brief summary of their claims.

Laudan: If scientific progress is a matter of increasing the number and quality of solved problems, two rival research traditions do not have to be conceptually related in order to determine which of them is the more progressive. The problem-solving effectiveness (PSE) of each can be ascertained 'internally' and autonomously, and the numerical results compared. This is quite sufficient for a cognitive index of rationality, and no piecemeal cross-theoretical comparison of concepts, laws and problems (such as would be difficult or impossible in the case of incommensurability) is required.

Stegmüller: If we hang on to the idea that theories are sets of sentences, we shall have to concede that, in the case of a revolutionary change of theory, incommensurability may result because the sentences of one theory need not be translatable into the language of the other. The ensuing lack of rational comparability ceases to be a problem, however, if we view theories as structures

of a certain sort. Their cognitive comparison and appraisal is then feasible because structures can be canonically related where sentences cannot. In fact, we should find that in a successful revolutionary change the older theory will be (structurally) reducible to the theory that replaces it. And this sets the growth of knowledge in a rational light.

Both these claims are radical. Laudan's because, by divorcing progress from truth, it claims that a rational choice of theory need not be based on truthlikeness or content. In particular, in the appraisal of competing theories a comparison of their conceptual and empirical content is irrelevant. Stegmüller's strategy is, if anything, still more radical, since it reinterprets the basic categories of 'theory', 'paradigm' and 'reduction', and so redefines the metascientific paradigm in which incommensurability and non-translatability posed a cognitive threat. Whilst Laudan demands only comparability of progressiveness, Stegmüller asks for comparability of concepts and content, too; though the meaning of these notions has meanwhile altered.

The solutions proposed are striking and ingenious. Why do they not succeed? What if anything do they overlook that makes them unacceptable even against their own, instrumentalist background? In the case of Laudan, we have a response to incommensurability reminiscent of Feyerabend's own position. Feyerabend also holds that whereas incommensurability rules out a comparison of the theories' *content*, other forms of comparison are admissible; including, notably, the number of their solved problems. But the difference in other respects between Laudan and Feyerabend makes all the difference. Feyerabend is content to settle for weak standards of comparability in order to bolster his own pluralistic methodology and the attitude of 'anything goes'. He requires, therefore, no unified theory of progress applicable impartially to different epochs and varying disciplines. Laudan does. His model of progress employs a monistic methodology, designed to provide a unified account of rational scientific growth. The account is, of

course, liberal in allowing differences in individual conceptions of rationality among the separate research traditions. To this extent Laudan's view is also relativistic; but his relativism is contained at the 'lower' level of theories and research traditions and does not intrude into the 'higher' level of methodologies. Consequently, his handling of incommensurability does not mesh with the general aims of his theory of progress.

The reason for this failure is easy to detect if we consider for a moment Laudan's analysis of PSE. This progress-count is supposed to be determined for any research tradition by taking a weighted aggregate of the solved empirical problems, subtracting a weighted aggregate of the anomalies, and negatively adjusting the total by a suitable factor representing unsolved conceptual problems. The weights assigned to problems and anomalies are decisive here; for, the solution of relatively few significant problems may have a far higher value than the ability to answer a host of problems of minor importance. But, not only are problems unequally weighted, one and the same problem may carry different values in the context of different research traditions. This looks like an intolerable degree of relativism, since it seems to imply that even giant successes achieved by one tradition could be utterly devalued by a rival. Balance can only be restored, I think, if we assume that rival research traditions generally compete to solve a large range of common problems which they value in roughly the same proportions. They should also agree on some shared standards for evaluating the adequacy of problem solutions, otherwise each can lay claim to successes that the other refuses to recognize. With this proviso it may make sense to compare for their progressiveness two research traditions that compete in a given field. Numerical measures of PSE may mean little by themselves, but the comparison may be informative.

But suppose that two incommensurable theories or research traditions are alone in vying for allegiance in a given field. They can share no problem solutions nor anomalies. It is unclear, too, how

the successes of one could be the anomalies of the other, if their problem domains are distinct. Moreover, if they are incommensurable in Kuhn's sense, their way of posing problems and evaluating solutions will be different. In this situation a comparison of PSEs would be like a comparison of the birth rate in Japan with the inflation rate in the UK. We could look at the figures, draw graphs, apply statistical regressions, and maybe even correlate the results; but the exercise is never likely to be more than a meaningless manipulation of numbers. Under these circumstances, therefore, a calculation of relative progressiveness cannot be a ground for rational choice; and at this point Laudan's theory of progress and rationality is severely undermined. There seems no other conclusion but that the theory folds under the assumption of incommensurability.[6]

Stegmüller's theory stumbles over ground of a different kind. Its chief flaw is to suppose that we might slip smoothly into a new metascientific paradigm and cast aside with no regrets the recalcitrant problems of our old ways of thinking. The reason we cannot is not due simply to stubbornness and an entrenched philosophical conservatism, though I admit that methodologists no less than scientists tend towards over-attachment to their favoured research traditions. The problem in Stegmüller's case is that we lack convincing grounds for making the switch at all.

The structuralist view of theories over-reaches itself on one delicate point that is crucial to the whole argument on commensurability. It is the point at which it invites us not merely to take structures seriously, but to renounce all interest in language. The first piece of advice is well-meaning and unobjectionable. We have known at least since von Neumann's analysis of quantum theory and Suppes's work on particle mechanics that a serious study of structures may enlighten and inform. The structural perspective, set alongside the linguistic one, is a valuable addition. But the study of structures was never intended to *replace* the study of scientific laws as statements; why should it? In model theory sen-

tences and structures live together in perfect harmony; which is hardly surprising since the central task of model theory is to describe the interplay of the two. In Stegmüller's structuralism, however, we are compelled to divide the happy marriage of syntax and semantics. It is a sad divorce all round, and so much the worse for us, because the couple are truly inseparable and we are left with a poorer not a richer view of science.

What does the new paradigm offer us in return? It promises, in the first place, a fresh perspective on intertheory relations and, as a consequence, a clearer grasp of scientific change as a rational, well-founded process. But by whose criterion of rationality? The structuralist, of course. How can we compare the two to find out which we should prefer? Perhaps the answer is that we cannot. Perhaps the two paradigms are incommensurable and to that extent not rationally comparable. But this state of affairs would be disappointing indeed. We would be asked to switch allegiance to a new metascientific research tradition at least partly on the grounds that it resolved certain problems of scientific appraisal, rationality and commensurability, only to be faced with similar difficulties of comparability and choice among metatheoretical frameworks. Hardly a convincing premiss on which to build an argument, and probably not the kind of footing on which structuralists themselves would like to stand.

Let us then pursue the more plausible response that the two paradigms are commensurate and their conceptions of rationality are themselves rationally comparable. We can assess what the new approach offers us and judge the fruits. The first thing we would have to accept, if we buy the structuralist package, is a revised explication of intertheory relations like reduction; the new theory of scientific evolution hinges decisively on this. We may find some resistance here, because the new explication departs radically from tradition. In particular, it requires that we rescind our standard intuition that no member of a pair of incommensurable theories is reducible to the other. So we suspend, temporarily, our intuitions

and keep an open mind. We also agree to entertain the possibility that intertheory explanation and reduction might be characterized independently of any relations of translatability and inference that might hold between scientific laws. We might even go the whole hog with structuralists and agree that their explication of reduction is, in its own terms, an adequate one. Would this be sufficient for believing the problem of incommensurability resolved?

Obviously not, because a vital part of the argument is still missing. We still need to be sure that if two theories are rationally connected, say by reduction, in structuralist terms, they might nevertheless be incommensurable in our terms. It will not suffice to indicate what reduction amounts to in the structural sense, we also have to show what the same relation *does not* entail at the level of language. The problem should certainly have an answer. If, as we supposed, the two metascientific paradigms are comparable, then a concept defined in the structuralist framework will have a more or less precise meaning when the linguistic dimension of theories is taken into account. Indeed it has. And I am afraid that the result looks anything but favourable for the structuralist theory. To put the point in a nutshell, a structuralist reduction relation implies the presence of a translation and even a logical inference between the related theories. Thus it scarcely supports the thesis that they might be incommensurable.

This is the conclusion, at any rate, that I have reached. I have argued for it at length, and on several occasions, so I shall spare the reader a repetition of the details here.[7] Of course, no conclusion of this kind can be proved beyond doubt. A comparison of metatheoretical frameworks, like an analysis of relations between scientific theories, requires assumptions some of which may be debatable. And 'incommensurability' is itself a slippery concept. But in this case I think that the 'evidence' is just about as incontrovertible as it can ever be. Structuralists are naturally reluctant to concede this point, and Wolgang Balzer has argued vigorously in

their defence. But this has achieved little except to compound old confusions with fresh ones.[8]

The new structuralist response to the problem of incommensurability is roughly this: To establish the commensurability of two theories, a translation between their languages would have to satisfy certain, precise conditions; but these conditions are not generally fulfilled by the kind of translation, if any, that a structural reduction relation induces. It follows that we *can* use the concept of reduction to obtain a cognitive connection between theories that are linguistically incommensurable. This is surely clutching at straws. To begin with, this answer virtually concedes wholesale the main point of my argument which was to show that in analyzing questions of commensurability and reduction we need model theory in the full sense (rather than only structures) besides an adequate account of translation. The introduction of an irreducible linguistic component in the shape of syntactic translation is far more than a minor adjustment to the structuralists' framework. It masks a very basic change of their paradigm. The chief grounds in favour of structuralism were that it allowed one to forgo logic and language, settle for a simpler and supposedly more accurate representation of theories and, in so doing, resolve many of the philosophical disputes over the rationality of scientific progress. All this is now repealed at a stroke with the admission that commensurability questions must be tackled by means of intertheory translation after all.

It is also, I believe, questionable whether an *exact* definition of (in)commensurability in terms of translatability conditions would serve any purpose. It is valuable, of course, to know what kind of translation, if any, links the languages of two rival theories. And it makes sense to classify translations of different types and investigate what degrees of logical contact they establish; this is essential for appraising rational choices. But to suggest that translations of such-and-such types unfailingly result in commensurability where other types do not is, I think, to draw a largely arbitrary

distinction of kind where what is really involved is a matter of degree. Secondly, such a definition is likely to distort the original sense of the term 'incommensurable'. If we wanted to characterize Kuhn's notion of incommensurable we would have to distinguish translations that preserve *meaning* from those that do not, and I fail to see how the structuralist framework can express the difference. Their definition does not, in any case, seem to capture Kuhn's nor any other reasonable version of what incommensurability consists of.

Though I have argued that Laudan's and Stegmüller's accounts of progress come to grief over the problem of incommensurability, I do not believe that their theories need be abandoned in their entirety. There is a good deal of Laudan's analysis of research traditions and problem solving that strikes me as valuable and essentially correct. And the structuralist approach, to its credit, has shown how we can begin to get a reasonably precise formal model of theory change off the ground. Where it scores, I think, is in its realization that comprehensive scientific systems like classical mechanics are best construed as structured networks of interrelated theories (so-called 'theory nets').[9] The idea is familiar, of course, in ordinary textbook treatments of mechanics. One is presented with some basic laws or equations that are fundamental to the research tradition in question (Newtonian, say, or relativistic). The laws are then further extended and elaborated to solve empirical problems in specific domains. 'Special' equations are devised for this purpose in which particular 'types' of forces, together with physical constants and boundary conditions appropriate to the domain, are characterized. Each problem domain (or set of structurally similar domains) is thus covered by a certain theory in the tradition; and, looked at globally, the tradition consists of an interconnected complex of theories all sharing some common primitive terms and assumptions.

Though structuralists are wobbly over some of the details about how such networks of theories could be characterized, the basic

approach is worth developing. In fact, the kernel of a scientific research tradition in Laudan's sense can, I think, be reconstructed as a theory complex of roughly this kind, which I call an 'ensemble'. When the fine structure of a research tradition is exhibited in this way, and the various problem domains distinguished, we can model the problem-solving process more exactly and compare different research traditions for their problem-solving effectiveness.

The role of translation is fundamental here. Unlike Laudan, I do not hold that to establish a degree of commensurability between two research traditions one should be able to isolate a class of shared problems each member of which can be described independently of all the theories of the traditions in question. What seems to be essential for problem sharing is that a statement which expresses a given problem in the context of one of the traditions has an acceptable translation into the language of the other. This may occur, for example, if some of the theories belonging to one of the traditions are in correspondence with counterpart theories from the other; the correspondences being defined through a combination of syntactic translation and model-theoretic correlations.

This way of casting the logical relations between research traditions (or between their formal analogues, the theory ensembles) does more than merely highlight the extent of their comparability and commensurability. It is a first step in the direction of being able to determine *when* a given problem solution within one research tradition is successfully transferred into the framework of a rival. Thus it enables us to keep track of that common pool of problems adequately solved by both traditions which is basic to the calculation of their respective PSEs. Likewise it gives us a grip on their *anomalies*. It is insufficient to maintain, along with Laudan, that an anomaly for a given research tradition T is simply an unsolved problem that has been answered by a rival tradition. To know that a problem is anomalous for T we first have to ascertain that it is recognized by T *as a bona fide problem* and, moreover,

that the solution proposed by T's competitor is from T's standpoint successful. A problem solution that cannot be translated into the framework of T, or a solution that under translation is seen to be inadequate because it fails to meet certain standards of accuracy or because it contains premisses that clash with some fundamental tenet of T can hardly be treated as an anomaly for that tradition. However, the logical relations between the ensembles representing T and its rival should clarify precisely when these conditions are met, namely when the translated problem solution is 'meaningful' for T and therefore potentially anomalous for it.

I cannot enter here into any discussion of how these suggestions can be worked out in detail. Elsewhere I have given, with Veikko Rantala, an outline of a formal model that incorporates some of the central features of research traditions and analyzes some of the questions of comparability, translatability and problem solving just touched upon.[10] That work is tentative and still at an exploratory stage, and a general account of scientific progress is some way off. My remarks here are intended as a reminder to instrumentalists like Laudan and Stegmüller that by passing over questions of translation their theories remain essentially incomplete and their accounts of rational growth in science stray off course. I have hinted at some of the ways in which a proper analysis of intertheory translation could lead to a more adequate theory of scientific progress. I see that theory, if it were to be further adumbrated, as substantially instrumentalist in tone. It would, I think, share with Laudan's a methodological instrumentalism that focusses on the problem-solving, question-answering aspects of scientific inquiry. Like Stegmüller's, it would conceive the central metascientific concepts in structural, though not structuralist, terms. It would not, I imagine, share Stegmüller's particular variety of semantic instrumentalism, but the label 'realism' would probably not apply to it either.

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NOTES

[1] Though they both share 'incommensurability' as a central theme, this paper differs from the one I read at the 5th Joint International Conference on the History and Philosophy of Science in Veszprém. The final version of the latter grew too long to be included in the Conference Proceedings; it has appeared instead in my *Roads To Commensurability*, Reidel, Dordrecht 1987. Most of the points raised in the present essay are dealt with in greater detail in that book.

[2] Notably in Popper's 'Normal Science and its Dangers', in I. Lakatos and A. Musgrave (eds), *Criticism and the Growth of Knowledge*, Cambridge Univ. Press 1970, and in Davidson's 'On the Very Idea of a Conceptual Scheme', reprinted in *Inquiries into Truth and Interpretation*, Clarendon Press, Oxford 1984.

[3] For a history and critical analysis of the conventionalist epistemology and methodology of science, see J. Giedymin, *Science and Convention*, Pergamon Press, Oxford 1982. The importance of the conventionalist tradition for the genesis and evolution of Fleck's ideas is the subject of Giedymin's 'Polish Philosophy in the Inter-War Period and L. Fleck's Theory of Thought-Styles and Thought-Collectives', in R. S. Cohen and T. Schnelle (eds), *Cognition and Fact-Materials on Ludwig Fleck*, Reidel, Dordrecht 1985.

[4] The positions of Laudan and Stegmüller that I ciriticize are those represented in L. Laudan, *Progress and its Problems*, Univ. of California Press 1977, and in W. Stegmüller, *The Structure and Dynamics of Theories*, Springer Verlag, New York 1976, and *The Structuralist View of Theories*, Springer Verlag, New York 1979.

[5] For example in K. Popper, 'Three Views Concerning Human Knowledge', reprinted in *Conjectures and Refutations*, RKP, London 1963.

[6] The only alternative to this conclusion would seem to be that rationality cannot be identified with progressiveness; but to accept this would require a major shift of position on Laudan's part; see my 'Research Traditions, Incommensurability and Scientific Progress', *Zeitschrift für allgemeine Wissenschaftstheorie* 15 (1984), 261–271.

[7] See my 'Stegmüller on Kuhn and Incommensurability', *Brit. J. Phil. Sci.* 33 (1982), 389–396; and 'Logical Properties of the Structuralist Concept of Reduction', *Erkenntnis* 18 (1982), 307–333.

[8] See W. Balzer, 'Incommensurability, Reduction and Translation', *Erkenntnis* 23 (1985), 255–267 and my 'Incommensurability and Reduction Reconsidered', *Erkenntnis* 24 (1986), 293–308.

[9] See especially W. Balzer and J. D. Sneed, 'Generalized Net Structures for Empirical Theories', I and II, *Studia Logica* 36 (1977), 195–211, and 37 (1978), 167–194.

[10] See our 'Constructing General Models of Theory Dynamics', *Studia Logica* 42 (1983), 347–362, and 'Scientific Change, Continuity and Problem Solving', *Philosophia Naturalis* 21 (1984), 389–399 and my *Roads To Commensurability, op. cit.*

VEIKKO RANTALA

SCIENTIFIC CHANGE AND COUNTERFACTUALS

According to an earlier and widely-held view of scientific change, a scientific theory is conceptually and deductively connected with its predecessor; the earlier theory is reducible to the later one. Very influential was E. Nagel's suggestion that the laws of the reduced theory can be derived from those of the reducing theory together with auxiliary assumptions, which may contain rules connecting the languages of the two theories. In this way the reducing theory explains the reduced one. This view has been challenged by several critics, however. It has been objected, for instance, that the two theories can be incompatible, whence the derivation or explanation can only be approximate or limiting; or that the auxiliary assumptions are counterfactual or incompatible with the reducing theory, whence the *explanans* is false or contradictory; or that the two theories are incommensurable, whence there is no deductive or conceptual relationship between them. This criticism has resulted in a number of refined and modified notions of reduction, such as approximate reduction, structural models of reduction, models using idealization and factualization, etc., which, however, have also been subjected to criticism.

Glymour (1970) suggests a counterfactual model of reduction to handle these controversial cases: "One does not explain one theory from another by showing why the first is true; a theory is explained by showing under what conditions it *would be* true, and by contrasting those conditions with the conditions which actually obtain." A counterfactual account may help one to understand an intertheory relation involving incompatible or contrary-to-fact assumptions. Sor far no counterfactual approach has been analyzed in explicit and sufficiently general terms, however. Therefore it is questionable whether such an account has actually increased our

Imre Hronszky, Márta Fehér and Balázs Dajka (eds), Scientific Knowledge Socialized, 399–408.
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understanding of the situation, and whether it, after all, differs in any essential sense from the derivational and explanatory accounts or is less obscure.

In earlier papers by David Pearce and myself, generalized and exact notions of theory and intertheory relation have been introduced and applied to actual scientific theories (e.g., Pearce and Rantala 1983a and 1984abc). In particular, by using a generalized notion of logic and concepts from general model theory and non-standard analysis, a simple and natural construal of limiting-case correspondence has been offered, which, as it is claimed in Pearce and Rantala (1985), conforms to the explanatory and derivational pictures of intertheory reduction and at the same time lacks the difficulties involved in the earlier accounts. In Sections 1–2, below, I shall recapitulate this account without any comments; for a more comprehensive discussion, I refer to our papers mentioned above. In Sections 3–4, I shall show that this account offers us a way to develop a counterfactual model for limiting-case correspondence. That is, on its basis it is possible to obtain an exact counterfactual analysis, in the sense of Lewis (1973), for the revolutionary scientific change. It can be seen, then, that the explanatory account of revolutionary scientific change goes together with the counterfactual one. As soon as suitable metatheoretic tools are used to explicate the situation, it can be seen that the latter is not just a way to compensate the failure of the former.

1. CORRESPONDENCE RELATION

A theory is regarded as a *structural* entity to which syntactical, empirical, and other relevant considerations are applicable. More precisely, a *theory* in this sense is a structure

$$T = <\tau, N, M, R>$$

where (i) τ is a similarity type in the sense of general model theory (i.e., a set consisting of a vocabulary and sorts for domains of individuals); (ii) N is a collection of models of type τ; (iii) $M \subseteq N$; (iv) R is a collection of appropriate relations holding between models in N and their individuals (see Pearce and Rantala 1984a); (v) the collections N, M, and R are set-theoretically definable (in a relevant metatheory); N and M are definable in an appropriate logic L, that is, they are L-elementary classes.

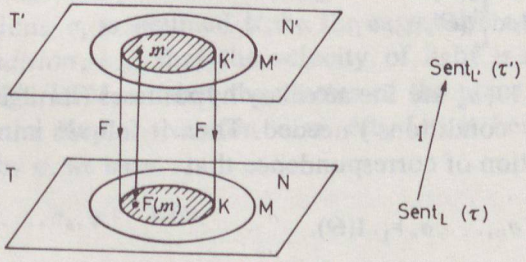
Any model $m \in M$ is a *model of* T. Any logic like L is called *adequate for* T. L (and the other adequate logics) is a logic in which e.g. the axioms of T defining M can be formulated, that is, they can be expressed by means of sentences belonging to $\mathrm{Sent}_L(\tau)$, the set of all L-sentences of type τ.

Let $T = <\tau, N, M, R>$ and $T' = <\tau' N', M', R'>$ be two theories and let L and L' be two logics such that L is adequate for T and L' adequate for T'. Then a *correspondence of* T *to* T', *relative to* $< L, L' >$, is a pair $<F, I>$ of mappings

$$F: K' \xrightarrow{\text{onto}} K \quad (K \subseteq M, K' \subseteq M'; K \neq \emptyset, K' \neq \emptyset),$$
$$I: \mathrm{Sent}_L(\tau) \longrightarrow \mathrm{Sent}_{L'}(\tau')$$

such that K is *definable* in L and K' in L', and F and I are *truth preserving* in the sense that for all $m \in K'$, $\varphi \in \mathrm{Sent}_L(\tau)$,

$$F(m) \models_L \varphi \Leftrightarrow m \models_{L'} I(\varphi).$$



F is called a *stuctural correlation* and I a *translation*. If L=L', the correspondence is called *relative to* L.

It is said that <F, I> is a *limiting(-case)* correspondence if K' is a class of *non-standard models* of T', satisfying appropriate finiteness conditions, and for all $m \in K'$, $F(m)$ is a *standard approximation* of m. In ordinary quantitative cases, a model of a theory is called *non-standard* if its 'mathematical part' includes non-standard analysis, in the sense of Abraham Robinson; otherwise *standard*. But this notion can be generalized for arbitrary theories. That $F(m)$ is a standard approximation of m means, roughly, that it is a standard model which is 'very close' to m. No precise definitions can be given here (for them see Robinson 1966; Bell and Machover 1977; Pearce and Rantala 1984a).

Relations in R and R' may also play a role in a correspondence, in particular, if they are *symmetries*. But as this feature is not needed in the present paper, they will not be discussed here (cf. Pearce and Rantala 1983b, 1984a).

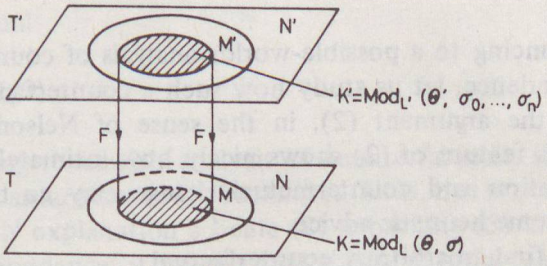
2. CORRESPONDENCE AND EXPLANATION

Let T and T' be two theories and let <F, I> be a correspondence of T to T' relative to <L, L'>, as indicated above. Let

$$M = \mathrm{Mod}_L(\Theta)$$
$$M' = \mathrm{Mod}_{L'}(\Theta')$$
$$K' = \mathrm{Mod}_{L'}(\Theta', \sigma_0, \sigma_1, \ldots, \sigma_n).$$

Here $\sigma_0, \ldots, \sigma_n$ are the auxiliary hypotheses ('bridge assumptions', 'boundary conditions') needed. Then it follows immediately from the definition of correspondence that

(1) $\Theta', \sigma_0, \ldots, \sigma_n \models_{L'} I(\Theta)$.



Thus, even though the respective scientific change, represented by the correspondence in question, were *revolutionary*, it may happen that the new (supplanting) theory *explains* the old (supplanted) theory in the sense of D-N explanation; we may roughly say that the new theory together with some additional conditions logically implies the *translation* of the old one. (This is, of course, a semantical version of D-N explanation; cf. Pearce and Rantala 1985.)

Whether (1) really yields an adequate explanation of the old theory depends on several aspects: how natural the bridge assumptions are, how effective the translation is, how workable the logic is, and so on. But in any case, this schema indicates that even in a radical change some amount of *continuity may obtain* (cf. Pearce and Rantala 1984b).

If, in particular, $<F, I>$ is a limiting-case correspondence, one of the conditions σ_i is assumed to be the respective contrary-to-fact *limit condition*, e.g. that the velocity of light is infinite, in the case of CPM/RPM, or that the masses of the planets are infinitesimal, in the Kepler–Newton case, etc. Let it be σ_0; if we denote it now by φ, we have:

(2) $\Theta', \sigma_1, \ldots, \sigma_n, \varphi \models_{L'} I(\Theta)$.

3. BACKING A COUNTERFACTUAL

Before advancing to a possible-worlds analysis of counterfactuals in correspondence, let us study how such a counterfactual is supported by the argument (2), in the sense of Nelson Goodman (1947). This feature of (2) shows nicely how intimately intertheory explanation and counterfactual change may go together. It also yields some heuristic advice.

Consider first an arbitrary counterfactual

(3) $\varphi \mathrel{\square\!\!\rightarrow} \psi$.

It is said that this counterfactual is *backed* by a valid argument

(4) $\dfrac{\chi_1, \ldots, \chi_k, \varphi}{\psi}$

where the line indicates a logical inference of some kind, if each χ_i is true and *cotenable* with φ (see Goodman 1947; Lewis 1973). What it means to say that a statement χ is cotenable with φ can be explained in terms of possible worlds, but intuitively it means a kind of 'strong' compatibility of χ with φ: χ is 'necessarily true' to the extent to which it is needed in order that it be compatible (jointly satisfiable) with φ. This can also be expressed as follows: χ and φ are both true on some given occasion and χ is true on every occasion that is 'closer' to the 'actual' occasion than the given one is.

In applications of the above reconstruction to actual cases of limiting-case correspondence, the assumptions $\sigma_1, \ldots, \sigma_n$ are generally conditions on non-standard models of T' saying that these models should resemble in certain respects 'well-behaved' standard models as much as possible; and they are true in such standard models. This suggests immediately that they might be cotenable with the limit condition, since, furthermore, they are compatible

with φ. This, in turn, suggests that the argument (2) is backing the counterfactual

(5) $\varphi \boxdot\!\!\rightarrow I(\Theta)$.

But according to Lewis's analysis of counterfactuals, (3) is *true* if there exists an argument (4) backing it. Thus, if it is correct to say that the D-N explanation schema (2) is backing (5) in a limiting-case correspondence, it follows that this counterfactual is true. It can be shown that this is really the case in many (reconstructed) instances of limiting-case correspondence between actual scientific theories. I shall discuss here only one example, but the other, or at least many of the other, cases are similar.

First, however, I indicate briefly how the foregoing can be made more precise; I just recapitulate Lewis's theory (cf. Lewis 1973). For simplicity, I consider only propositional logic, expanded by the counterfactual operator $\boxdot\!\!\rightarrow$. Let it be L_0. Let us say that a *Lewis model* for L_0 is a Kripke model of the form

$$\mathcal{W} = \langle W, w_0, R, \lesssim, V \rangle$$

such that

W is a non-empty set (possible worlds);
$w_0 \in W$ (actual world);
$R: W \rightarrow \mathcal{P}W$ (accessibility mapping);
$\lesssim \subseteq W^3$ (similarity relation);
V: Prop $\longrightarrow \mathcal{P}W$ (valuation), where Prop is the set of all propositional letters of L_0.

The condition $\langle w, v, u \rangle \in \lesssim$ will be denoted by $v \lesssim_w u$ and construed as saying that v is at least as *similar* to w as u is, or that v is at least as *close* to w as u is. It is assumed that $u \in R(w)$ and $v \lesssim_w u$ imply that $v \in R(w)$.

The *truth* condition for the counterfactual is now defined as follows:

$$\vDash_w \varphi \,\Box\!\!\rightarrow \psi \Leftrightarrow [\exists u \in R(w)(\vDash_u \varphi) \Rightarrow \exists u \in R(w)(\vDash_u \varphi \,\&\, \forall v \in W(v \lesssim_w u \Rightarrow \vDash_v \varphi \rightarrow \psi))]$$

that is, the counterfactual is true at w iff the material implication $\varphi \rightarrow \psi$ holds at every accessible world which is sufficiently close to w.

The notion of cotenability can be now defined as follows: Assume that a formula φ is 'possible' at $w \in W$, that is, that $\vDash_u \varphi$ for some $u \in R(w)$. Then χ is *cotenable with* φ *at* w if $\vDash_v \chi$ for all $v \in W$ such that $v \lesssim_w u$. If φ is not possible at w, then it is just required that χ is 'necessary' at w; that is, that $\vDash_u \chi$ for all $u \in R(w)$.

4. AN EXAMPLE: CORRESPONDENCE OF CPM TO RPM

It can be shown that CPM is in correspondence to RPM in the sense defined above (Pearce and Rantala 1984a). In this case the sentences of the argument (2) are as follows:

Θ: the axioms of CPM, containing Newton's second law plus relevant mathematical conditions for the functions involved, etc.;

Θ': similarly for RPM; contains Minkowski's force law;

σ_1, ...: conditions for the non-standard systems (models) of particle mechanics involved; saving, e.g., that the relevant 'physical' entities (position, mass, force) are finite, that velocities and accelerations are bounded in the standard sense (i.e., bounded by some standard numbers), etc.;

φ: the velocity of light is infinite (or, alternatively, that the ratio of the velocity of any particle to that of light is infinitesimal);

I(Θ): Newton's second law 'almost' holds, that is, it holds with an infinitesimal accuracy, etc.

Consider now any standard system of RPM, say w, such that the velocities and accelerations of its particles are bounded. Then it satisfies the conditions expressed by $\Theta', \sigma_1, \ldots$. It follows that any relevant and natural similarity relation $\lesssim$ in a Lewis model whose possible worlds are systems of particle mechanics has to have the following property:

(6) If $\models_u \Theta', \sigma_1, \ldots$ and $v \lesssim_w u$, then $\models_v \Theta', \sigma_1, \ldots$.

Such similarity relations are easy to come by. On the other hand, there are natural conditions on systems of particle mechanics that readily suggest a relevant accessibility relation, such that the condition $\models_u \Theta', \sigma_1, \ldots$ holds for some $u \in R(w)$. For instance, one is naturally considering kinematical alternatives to a given system of particles; then one, of course, keeps the particles fixed; and keeps an eye on some other constancy properties concerning kinematical entities of the system.

What this discussion now amounts to is that on certain very *natural assumptions* each of the conditions $\Theta', \sigma_1, \ldots$ in the argument (2) – deriving from the correspondence relation between CPM and RPM – is *cotenable* with the assumption that the velocity of light is infinite, at any standard system like w, above; this assumption is contrary-to-fact at such a system. Thus, on these assumptions, it really holds that the corresponding counterfactual (5) is *backed* by the argument; whence it is *true* at every 'standard world', so to speak, of the kind indicated. So, if the 'actual' systems of particles, 'actual worlds', are among such standard systems – as they might well be according to the current scientific knowledge – we

may say that the counterfactual is *true*. In discursive terms, this counterfactual says, roughly, the following: 'If the velocity of light were infinite, then Newton's second law would *almost* hold'; or, perhaps: 'If the velocity of light were infinite, then the mass times the acceleration would almost equal the force.'

I would conclude on the basis of these results that even if someone did not accept the view that – in a case of revolutionary change – an argument like (2) would yield a proper D-N explanation of the supplanted theory by means of the supplanting one, he nevertheless might accept the fact that (2) yields a 'counterfactual explanation', representable in explicit terms.

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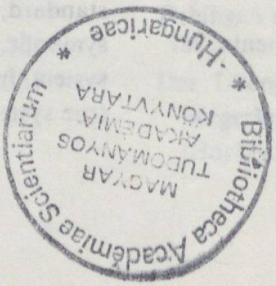

This volume contains a selection from papers presented to the 5th Joint International Conference on the History and Philosophy of Science. The Conference had as its general topic the impact of society on scientific cognition. The centre of interest was the recent 'sociological turn' in the philosophy of science brought about by the challenge of cognitive sociology. Contributors include M. Hesse, D. Bloor, and A. Gellatly, while other papers offer different perspectives on some of the most recent problems in philosophy of science (P. Janich, C. Dilworth, *et al.*). A special feature of this volume is a large number of contributions by scholars from socialist countries. These presentations enable Western scholars to get a glimpse of the wide spectrum of research interests and results in socialist countries.

This book will be of interest to philosophers, sociologists and historians of science and, hopefully, will help them to clarify their standpoints on a number of currently mooted problems. The volume can also be used as a textbook and as an important supplementary reading for upper level undergraduate and graduate courses.

ISBN 963 05 4771 6

5076237

SCIENTIFIC KNOWLEDGE SOCIALIZED

Edited by I. Hronszky, M. Fehér, B. Dajka

