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THE INTERACTION
BETWEEN SCIENCE
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edited by

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The Interaction between science and philosophy.

Papers from a symposium sponsored by the Israel Academy of Sciences and Humanities, the Hebrew University of Jerusalem, and the Van Leer Foundation for the Advancement of Human Culture, held in Jerusalem in January 1971 to honor Professor Samuel Sambursky on his 70th birthday.

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PREFACE

In January 1971 in conditions of unusually warm and sunny winter weather, a group of scholars from several parts of the world met in Jerusalem to honor the seventieth birthday of Professor Shmuel Sambursky. Their diverse fields were represented in the papers read in homage to Professor Sambursky and in the vigorous discussion that was immediately stimulated, continued over meals and other social gatherings, and which persisted even months later by correspondence. These papers and the essence of these discussions are offered in the present volume.

The debt to Professor Sambursky himself is to be seen in many places. Thus scholars with such different interests as Professors Ne'eman and Toulmin explicitly record their debt to him. A more subtle form of homage is the perhaps unconscious adoption of methods of analysis deriving from Professor Sambursky. As an example of this one might cite the suggestion of Professor Rosenfeld, in the discussion following Professor Toulmin's paper, that quantum mechanics peculiarly combines and reconciles the scientific ideals of Parmenides and Heraclitus. If Professor Sambursky's most well-known contribution was to employ concepts from modern science to obtain insights into the theories of Greek science, then here is the same approach applied in reverse to obtain a fresh insight into the nature of modern science.

Professor Sambursky's own paper provoked far from the least discussion at this meeting. So we may regard his seventieth birthday as indeed a cause of celebration, but only a stage in a life of continuing and developing activity.

The editorial blue pencil has been used with a measure of reticence on the discussions that followed each paper. The reasons for this are twofold. Firstly, in an age in which man is becoming increasingly concerned over his personal role in an increasingly scientific world, I felt that it would be a mistake to attempt to polish the style of these discussions solely as an academic exercise for the community of scientists, philosophers and historians. If these discussions can in their freshness contribute even an iota to wider general understanding, then they should be permitted to do so. Secondly, I would like to feel that this book will

make as lively, stimulating—and sometimes humorous—an experience as it was for the participants in the symposium. As example of this, allow me to print here two clerihews—one of W. H. Auden and oft-quoted by Shmuel Sambursky on Hegel, and the other composed on the occasion by John Murdoch very appropriately on Shmuel Sambursky.

It was impossible to inveigle
George Wilhelm Friedrich Hegel
Into offering the slightest apology
For his Phenomenology.

and the other:

Sambursky, historical scholar
Is no Aristotelian idolater
His view of science heroic
Is much more stoic.

On a more personal level, this is the best place to express my gratitude to and admiration for my teacher and my predecessor and the founder of the Hebrew University Department of the History and Philosophy of Science.

The Symposium was jointly sponsored by the Israel Academy of Sciences and Humanities, The Hebrew University of Jerusalem and the Van Leer Jerusalem Foundation. To all these institutions a sincere gratitude is expressed. Our gratitude is further due to the distinguished participants who came and made this volume possible.

I would also like to thank Mr. Malcolm Frederick Lowe for his great help in editing this volume.

YEHUDA ELKANA

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INTRODUCTION

The occasion for the Symposium was Professor Shmuel Sambursky's seventieth birthday; its intellectual focus was the perennial interaction between science and philosophy.

The volume before us will illustrate that indeed the roots of science are in metaphysics and the roots of scientific metaphysics are in the problem situations of the age. As Sir Karl Popper has said:¹ "Genuine philosophical problems are always rooted in urgent problems outside philosophy, and they die if these roots decay," while Agassi has argued that conversely problems that are considered urgent in science (and other fields outside philosophy) are so considered on philosophical grounds, and are influenced by philosophy.² In other words, there are not eternal philosophical problems and eternal scientific ones. In every age some questions are considered purely scientific while others are delegated to philosophy. It is the interaction between the two which is perennial, and the most fascinating study. It is this interaction which helps in seeing our own age in science and other fields in historical perspective; also it is this interaction which helps us keep in mind that every age has its "modern" and its "ancient" thinkers, its great riddles, urgent scientific problems and those problems which it considers solved. Even the knowledge that scientific solutions are only temporary is not a twentieth century invention: we find in every age great thinkers who profess to this temporariness; the most we can say is that some ages are more time-conscious and historically-minded than others. One is tempted to say that we live in an age which has completely absorbed the theory of evolution, and are indeed very history-conscious; but could this be said of the ruling school in philosophy and the ruling physical theory? It is disputable whether logical positivism is still the ruling school of philosophy, and in the view of many it is on the decline, but there is no question about its anti-historical character. As to quantum mechanics: let me only emphasize that more and more important papers on new approaches to the impasse in this field deal with concepts of time.

It was a glorious mistake on the part of Descartes to think that one could solve once and for all the fundamental questions—those about the

¹Reprinted in *Conjectures and Refutations* under the title: "The Nature of Philosophical Problems and their Roots in Science," London, 1963, p. 72.

²See his "Scientific Problems and their Roots in Metaphysics," in M. Bunge ed., *The Critical Approach to Science and Philosophy*, 1964.

metaphysical foundations of natural science—and then let science build up its structure from that point onwards. Modern science has learned not to pose those questions, and indeed, if it did, it could not (as presently constituted) progress at all. Today's science student is brought up on highly refined and sophisticated mathematical and experimental techniques, with an enormous abundance of facts which he has to master, and in his curriculum there is no place for fundamental questions. If he asks at all about the connection between those sophisticated techniques and so-called "physical reality" he is given a brief exposé of the Copenhagen interpretation of quantum mechanics. If we consider modern science, and especially physics, in 1972 as a successful enterprise, then the present approach is justified; in this case scientists are even justified saying that in their everyday activity philosophy would only disturb them, not aid them. But is it an unquestionably successful enterprise?

It was different with regard to the few great steps in the development of our modern world picture: classical mechanics, relativity and quantum theory (to mention only the most comprehensive complexes of issues). These developments grew out of fundamental questions, and very probably would have never taken place without raising those questions. From this point of view, history of science is not to be disentangled from philosophy of science, and the interaction between science and philosophy is strong and very illuminating.

The interaction between science and philosophy takes place on two different planes. On the one plane philosophy is embedded in or rather rooted in science as in the cases of Kant, Schlegel, Fichte, even Hegel, Comte, Mach and Poincaré. On the other plane it is vice versa: here science is imbedded in or rather rooted in philosophy as in the case of Galileo, Newton, Liebig, Johannes Müller, Helmholtz, Faraday. Naturally, such a dichotomy is oversimplified; and yet there is some basic truth in it: though we do compare Descartes' system with that of Newton, nobody questions that basically Descartes was a philosopher while Newton was fundamentally a scientist.

It is exactly this basic if oversimplified dichotomy which I have in mind when talking of two different planes. On the first plane, Kant was a philosopher whose problem situation came from Newtonian and later from Eulerian physics; that was the case in my opinion also with the "Naturphilosophen"; Comte fed on Fourier's physics; Faraday's meta-

physical preoccupation is well-known today; Mach and Poincaré were in the first place philosophers even if their activities were often in the field of science. That this emphasis is not widely accepted at least for these last two is, I believe, only due to the fact that they are still near in time: in two hundred years they will be considered as philosophers with minor contributions to science; likewise Descartes is now remembered for his philosophy, not for his law of inertia or law of sines. (As for pure mathematics, both Descartes and Poincaré fill a special place, but even here one could easily show the dependence of their mathematics on their philosophical principles).

On the other plane we have those great physicists whose conceptual contributions were derived from having posed the basic philosophical questions: "biology," which, is a major conceptual development, was created by Liebig, Johannes Müller and their co-workers; the "field" concept was born in Faraday's scientific metaphysics, and the concept of energy originated with Helmholtz. There were great contributions by many others: the French mathematical physicists, Maxwell and Kelvin, whose physics was rooted to a much lesser extent in philosophy, and they gave new mathematical formulations or great syntheses, but rarely created new concepts.

Finally, such an approach as is given here carries with it, needless to say, a distrust of such historical studies as are written from the point of view of sudden revolutions. The slow mutual influence and interaction between science and philosophy presupposes a different kind of development. A highly interesting philosophical discussion on this topic has been going on in the last few years. I. Bernard Cohen, following Whewell, says in his Introduction to his Wiles Lectures:

"Revolutionary advances in science may consist less of sudden and dramatic revolutions than a series of transformations, of which the revolutionary significance is not realized (except by historians) until the last great final achievement. Thus the full significance of a most radical step may not even be manifest to its author."³

* * *

³I. B. Cohen: The Wiles Lectures, held at the Queen's University, Belfast in 1966, to be published soon.

Let us look briefly in more detail at one of the three great developments that were alluded to above as one in which the interaction of science and philosophy was strong, namely at the development of classical physics. Due to Newton's success-story, this is often seen in the light of the debate between Newtonian and anti-Newtonian science.⁴ But what is to count as Newtonian and what as anti-Newtonian? This is not a rhetorical question but a historiographical problem of fundamental importance, any reasonable answer to which would presuppose a comprehensive knowledge of the growth of science from the seventeenth century onwards.

To describe the world in terms of discrete particles between which central forces are acting at a distance is certainly Newtonian—this is the metaphysical core of his *Principia*. The programme of mathematization of mechanics, as perfected by the French school of rational mechanics is also Newtonian. So are the various matter theories of the eighteenth century following the Queries appended to Newton's *Opticks* and Newton's letters to Bentley. The idea of chemical affinities is Newtonian, but so was the Daltonian revolution which rejected the affinities. Lavoisier was a Newtonian of sorts, and so was Priestly whom Lavoisier rejected, and so was Humphrey Davy who refuted Lavoisier's central doctrine, namely that all "elements" contain oxygen.

Some of these great natural philosophers called themselves Newtonians because they adhered to a world-view where the most important force was gravitation acting-at-a-distance; others accepted a material substratum, the ether, which transmits all physical action and this was the reason why they considered themselves Newtonians; others again made the same claim because they believed themselves to be doing scientific work in the hypothetico-deductive way, which they considered to be the hallmark of Newtonianism. Needless to say there is very little similarity between Newton's thoughts and speculations and the conceptual framework which they thought to be Newtonian. In addition to those who thought themselves bona fide Newtonians, there are others who used the label "Newtonian" politically to gain legitimization for their theories; an example is Thomas Young, a disciple of Euler and Huygens, who introduced his famous paper on interference of light by attributing

⁴For this argument in greater detail and applied specifically to the eighteenth-century see my account in the Brit. J. Phil. Sc., August 1971.

the main ideas to Newton. Finally there were the continental natural philosophers who accepted Newtonian mechanics, but attempted to blend it into their Cartesian or Leibnizian conceptual frameworks; later historians in their positivistic whitewashing exercises called them "Newtonian"; such were, to name only a few, Boscovich, Euler and Kant.

In my opinion, on the other hand, a clearer picture is seen when we recognize that there were at least three great traditions or scientific *research programmes* competing for primacy in science. These were the Cartesian, the Newtonian and the Leibnizian research programmes. The critical dialogue between these three was conducted in pairs: Newtonianism v. Leibnizianism; Newtonianism v. Cartesianism; and again separately Leibnizianism v. Cartesianism. Or rarely, two joined forces against the third. To lump all general explanatory hypotheses which are not Newtonian together under the heading "anti-Newtonianism" is an oversimplification.

Those conceptual frameworks which can justly be labelled as anti-Newtonian focus their opposition either on Newtonian science or Newtonian methodology. Yet anti-Newtonians proper and Newtonians share a fundamental problem-situation: Should or could one describe the universe in terms of discrete particles with forces acting between them; can force act through a vacuum; are forces essential properties of matter? On the other hand the eighteenth-century Leibnizians and the two different brands of Cartesians, which separated out of the original Cartesian framework at the turn of the century,⁵ had to face different problem-situations and had a different scientific research program than the eighteenth-century Newtonians.

The two Cartesian groups were the Cartesian *mathematical rationalists* like d'Alembert, Diderot and later Lagrange, and the Cartesian *matter-theorists* like Maupertuis, Euler and John Bernoulli. Cartesian mathematical rationalism developed a programme aimed at subsuming all phenomena under mathematically formulated laws. Here there was no discussion of fundamental concepts, no search for underlying principles, and the criterion of truth was rarely empirical. Rather, mathematical formalisability and elegance became signs of truth. These Cartesians were

⁵Rohault's 'Traité de Physique,' which appeared in 1671 in Paris, is probably the last Cartesian work before the above mentioned separation.

occupied in developing mechanics as a branch of mathematics and concentrated on attacking the Leibnizians rather than the Newtonians.

The main argument between the matter-theorist Cartesians and the Newtonians centered on the primacy of the concept of force. These Cartesians too accepted Newton's results, that is, the laws of mechanics and the law of gravitation,⁶ but they insisted that there are essential qualities of bodies to which forces can be reduced. If forces were introduced into the Cartesian program, they were considered as mathematical abstractions useful for smooth calculations—an attitude somewhat similar to Heinrich Hertz's a hundred and fifty years later.

The mind-body dichotomy was part of the Cartesian tradition but it played only a very minor role in the controversy with the Newtonians. This problem was, however, the core of the Cartesian-Leibnizian critical dialogue. The Cartesians separated mind and body, and also scientific metaphysics (that is, those views on the structure and genesis of the physical world which are in principle untestable, but form the core of the research program) from theology. Both the Newtonians and the Leibnizians, on the other hand, attempted to justify their scientific metaphysics by their theology. This justification became one of the foci of the Newtonian-Leibnizian critical dialogue, as exemplified in the Leibniz-Clarke correspondence and as continued by Euler in the 'Letters to a German Princess' written in the 1770's.

The central Newtonian conception is that of force, whether acting-at-a-distance or at a short range by contact. Newtonian physics, astronomy, chemistry and physiology all involve forces. Whether the forces are inherent in matter or reducible to their relational properties is another focus of the dialogue between Newtonians and Leibnizians. On the other hand, the concept of force is as foreign to the Cartesian as it is inseparable from both the Newtonian and the Leibnizian research programs.

Another difference between Newtonians and Leibnizians is that conservation principles are alien to the former but fundamental to the latter. Even though an anti-conservation-principles attitude is not explicit in Newton's writing, it seems to me to be one of his deep-seated anti-Car-

⁶However, they did not accept Newtonian optics, but rather fought the Newtonians on the issue of essential qualities and the nature of light jointly with the Leibnizians.

tesian biases. He, unlike Descartes, will not address himself lengthily to questions like—"What are fundamental entities?", "Are they conserved?" etc. He takes four "fundamental notions"—space, time, mass and force—for granted and operates with them. For Leibniz, too, the concept of force is fundamental, but it is rather conservation which is at the core of his scientific metaphysics. It is the idea of the conservation of force which served Leibniz in doing away with the Cartesian mind-body dualism and helped him develop his monistic theory.

In short, in order to gain any reliable picture of the growth of science one has to explore at least three competing traditions; all three left their indelible mark on the developments of science in the nineteenth and even the twentieth century, each at times had the upper hand in the long critical dialogues between them. Newtonianism is now the paradigm of success in terms of positive scientific results. The positivistic attitude therefore does not find place for either the Cartesians or the Leibnizians in the history of science. Thus "Newtonian" v. "anti-Newtonian" covers the ground adequately only if we judge the development of science presupposing that science grows by accumulation. If we view the growth of knowledge as a result of a dialogue between competing research programmes, we must think in terms of at least the above-mentioned three traditions.

* * *

The problem of the growth of knowledge is actually the most conspicuous problem running through this whole volume, irrespective of the terminology in which it is phrased. However, it also leads on to, or reacts with, several other problems.

There is for instance a critical dialogue between the participants on whether creativity in science depends in any way on the basic attitude of the scientist, e.g. whether a realist or an instrumentalist contributes more to the growth of knowledge. While nobody takes a stand on the question in this extreme formulation, most of the discussion (from Prof. Ne'eman's opening paper onwards) on "concrete" v. "abstract" imagery in science is basically about realism v. instrumentalism. The problem could be formulated in another fashion (more psychologically perhaps): can one expect a creative scientist to juggle with theoretical frameworks (i.e. to think in abstract terms) in order to see which would work better, or is commitment to a "concrete" picture indispensable? While Prof.

Ne'eman comes out strongly on behalf of the jugglers, at least at certain crucial stages of development, several of the historians are on the other side.

I myself feel strongly that most of the contributions to the growth of knowledge (including those which from our vantage-point seem errors) were made by the Copernican realists of this world and not by the Osiander instrumentalists. Toulmin's similar view on this question is only hinted at here, but it is dealt with more fully in his recent book on Human Understanding. As against this John Murdoch, who does not separate science from philosophy in the 14th century, sees the achievement of that period as more mathematical (i.e. abstract) than concrete. Yet the thought experiments to which he refers in the 14th century, exactly as those in the 19th century (as we learn later from Erwin Hiebert), are rooted in concrete pictures. How could it be otherwise? It is interesting that Shmuel Sambursky, whose insights are drawn mainly from having been an active physicist, also comes down on the side of the creativity of concrete imagery, which he, however, sees in scientific metaphysics. This is the formulation to which I also subscribe.

Another problem of the highest importance is the question of whether one can view the development of science from the late Middle Ages on to the early 17th century as a continuous growth of knowledge, or whether there was a qualitative change, a discontinuous leap. The problem is brought to the surface by John Murdoch's paper, and a lively debate follows it, but if carefully examined no paper in this volume evades the issue. For whatever stand one takes on the interaction between science and philosophy, the stand taken will more or less dictate one's approach to this question too. For myself, though I try as it were to photograph the instantaneous state of science and then differentiate between the body of knowledge, its social image and the external influences on it, it is clear that on the vertical-historical line no such differentiation can be carried through. Today's image of science will form the hard core metaphysics of tomorrow's scientific research programme; today's scientific metaphysics will influence heavily the politico-social "external" world. The growth of knowledge is due to a continuing critical dialogue between competing scientific metaphysics in the body of knowledge and competing images of science. This is not far from (though not identical) with Rosenfeld's "dialectic evolution which implies both continuity and

INTRODUCTION

qualitative change" with which it seems to me that both John Murdoch and Toulmin will agree. Even the disappearance of the permanent entities underlying Toulmin's Parmenidean Era, and its giving way to the Heraclitean Era, is the result if not of evolution, at least of a continuing revolution.

The terminology of "scientific research programmes" is permeating more and more into the intellectual exchange between the historians and philosophers of science. Started by Agassi and Lakatos, it proves (as my own paper aims to illustrate) to be a very useful tool in helping the historian to deal with large complexes of theories instead of just isolated theoretical answers to specific problems. This is also useful in focussing our attention on problem-situations rather than on problems.

The Bechler-Lakatos philosophical debate on validity v. consistency, together with Prof. I. B. Cohen's rich historical treatment of the philosophical issue in his Kepler-Newton case study, will contribute to further clarification of this "classical" problem.

While most of the participants exhibited a strong anti-positivistic bias, Hiebert's and Mendelsohn's papers will serve as convincing illustrations of how, now that the horse of positivism has been flogged to death, we should start to rewrite long-argued chapters in the history of 19th century science from an anti-positivistic point of view.

Gerry Holton's psychobiographical essay on Einstein is a fascinating foretaste of his book on Einstein to appear soon.

Ne'eman's, Yahil's, Rosenfeld's and Jammer's papers, while imbedded in physical theory, exhibit also very clearly how the interaction between physics and philosophy is a part of the body of knowledge in physics, considering that the problems chosen by them are "good physics" according to the present image of science. These may be regarded as examples of what was called above the second plane of interaction.

Sociological views of the science-philosophy interaction take a prominent part of the bulk of the volume. If one compares this with similar volumes like those of Clagett (1957), Crombie (1961) and Lakatos and Musgrave (1965) one can get a good grasp of the direction in which the profession is moving.

THE INTERACTION BETWEEN
SCIENCE AND PHILOSOPHY

CONCRETE VERSUS ABSTRACT THEORETICAL MODELS

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A. INTRODUCTION: CONCRETE AND ABSTRACT IN KEPLER'S CONTRIBUTION

This paper represents an attempt to abstract a lesson in the method of science—in particular theoretical science—from some of the more recent developments in physics. The main examples are taken from the study of Gravitation, and at greater length from the Physics of Particles and Fields. Presumably, these examples only emphasize well-known principles. However, it still appears that the lessons to be learnt have not been really absorbed in the conventional methodology. This is why they seem worth stressing.

Generally, the term “concrete” as used in reference to science should not be restricted to what appears concrete to a physicist in the second half of the twentieth century. To the ancients, concreteness implied the appearance of familiar objects in the structure of the universe. The sky had to be a giant tent or vault with little holes. Planets were later made to move on wheels, the wheels on other wheels or circles. To Kepler it appeared necessary at one time to postulate the existence of perfect solids embedded in each other—another type of clockwork.

Since, however, my major examples relate to recent developments, the term “concrete” will designate in their case conceptions where a so-called “good physical picture” appears in the background, namely a picture in terms of received relativistic and quantum effects: the causal propagation of an interaction, the inexistence of preferred reference frames, particle realization of fields satisfying the orthodox spin-statistics correlations, geometrized space-time. The “abstract” counterparts will appear in the form of mathematical formalism lacking such a clear “phys-

ical" interpretation. In time, this formalism will itself become "concretized" through the perception of some new and appropriate physical picture. This generation's abstract becomes the next generation's concrete, except that at the present rate of development, a "generation" may last as little as ten years! My basic thesis in this paper is that this kind of progress in which the abstract anticipates the concrete is commonly part of fruitful developments in science, while a premature demand for concrete formulations in accordance with some predetermined scheme may easily delay such developments.

For instance, Kepler's real contribution consisted of his three laws of planetary motion, all three of them unrelated at the time to any concrete model. That the planets should choose to move in elliptical rather than circular trajectories fitted no clockwork and even violated Kepler's aesthetic bias. His intuitive ideas—we would say nowadays "his physical intuition"—yielded nothing. Instead, it was only when he transcended his sixteenth century models that he finally produced a major advance in physics. After Newton, ellipses and velocity-area ratios ceased to be the abstract phenomenological regularities they had been to Kepler. They merged into the concrete physical picture now evoked in any mind trained in "classical" physics.

The remainder of this paper will discuss two further examples in more detail, one of which is already in large part historical, and one in which I am still myself involved.

B. EINSTEIN'S THEORY OF GRAVITATION AND MACH'S PRINCIPLE

1. *The Direct Experimental Predictions*

This is a case which has already been discussed in part in the past, for instance by R. H. Dicke.¹ I shall review his analysis and emphasize some results which have been derived more recently. As enunciated by Mach² himself, "Mach's Principle" embodies the idea of relativity, and carries it over to the interpretation of inertial forces,

¹R. H. Dicke, "The Many Faces of Mach" in H-Y Chin and W. F. Hoffman eds., *Gravitation and Relativity*, W. A. Benjamin, New York and Amsterdam, 1964, p. 121.

²E. Mach, *The Science of Mechanics*, 5th English edition, La Salle, Ill., 1942, Ch. 1, as quoted in ref. 1.

"For me only relative motions exist. . . . When a body rotates relatively to the fixed stars, centrifugal forces are produced. . . ."

This idea, which dates back to Bishop Berkeley, certainly appeals to our physical intuition, once the ether has been removed and we have no absolute space to stick to. The water in the rotating bucket climbs at the periphery because of an interaction with the distant masses of the universe, since there is no absolute space. It was thus natural that Einstein should feel so strongly about Mach's Principle. Indeed, he considered the fact that his new theory of gravitation reflected this principle as its main advantage, and named it accordingly the General Theory of Relativity. Quoting Einstein,³

. . . "the theory of relativity makes it appear probable that Mach was on the right road in his thought, that inertia depends upon a mutual action of matter. . . ."

Einstein went on to list three effects "to be expected" (i.e. by "concrete" thinking) if Mach's principle was valid:

1) a body must experience an accelerating force when neighbouring masses are accelerated, in the direction of that acceleration, for instance: a body inside a massive hollow sphere should experience an acceleration when we accelerate the sphere;

2) similarly, if we rotate the hollow sphere, we should produce Coriolis and centrifugal forces on the body inside it;

3) a body's inertia should augment (though to a very small extent) when additional masses are added in its neighborhood.

General relativity indeed predicts the first two effects. Einstein thought that it predicted all three, but he seems to have been mistaken about the third.⁴ The correction to Einstein's theory suggested by Brans and Dicke¹ does add such an effect. However, I think most workers in the field would be willing to take at least equal bets about the question of whether or not experiments will vindicate Einstein's version of the

³A. Einstein, *The Meaning of Relativity*, Princeton University Press, Princeton, N.J., 1955, p. 100.

⁴C. Brans, *Phys. Rev.*, 125 (1962), 2194.

theory, rather than the Brans-Dicke modification.⁵ Point 3 is then a first point in our list here, where the actual mathematical-physical theory differs from the intuitive thinking which led to it, and may even be the correct answer. That “concrete” thinking which led to the above mentioned third point may turn out to have been just “hand-waving.” (I use this expression as a convenient way of referring to superficial reasoning which gives a plausible impression that a logical difficulty has been removed, or that an explanation has been supplied in terms of accepted principles—but which fails the test of a thorough and more formal and quantitative analysis.) It is indeed difficult to distinguish between hand-waving and deductive thinking when the foundation is an “intuitive” idea rather than a mathematical theory. The issue will of course be settled by experiment—and the mathematical theory adopted accordingly. Then, a new “concrete” picture will follow as an interpretation.

2. Gödel's Universe

We now go on to “Gödel's universe.” This is a solution to Einstein's equation, discovered⁶ by K. Gödel in 1949. It was the first model to display esoteric effects due to space-time structure in the large. Many of these effects run counter to our “intuitive” views, e.g. a closed time-coordinate where the future connects back to the past. In addition, the model showed that in an infinite space, the matter of the universe can be made to rotate *absolutely*, i.e. *not* with respect to any distant masses (since they themselves are rotating). Here is a result which is certainly in violation of Mach's Principle; however, it derives from the equations of a theory which was supposed to embody that idea. Again, I think most physicists would change nothing of Einstein's equations. Clearly, we might add some boundary conditions (e.g. no such rotation!) which will preserve the principle. Nevertheless, even this ad-hoc insertion should wait until we first check the actual behavior of the universe phenomenologically. It might even be rotating!

⁵In 1967, Dicke published the results of an experiment showing that the sun is oblate. Assuming this to be correct, he found fault with the value of the precession of Mercury, as given by Einsteinian gravitation theory. In 1970, a J.P.L. measurement of the bending of radio signals from Mariners 6 and 7, when the spacecraft were passing behind the sun, has just brought at least equally strong evidence in favor of Einstein.

⁶K. Gödel, Proc. 1952 Inter. Congr. Maths (Cambridge, Mass.), Vol. 1, p. 175.

3. Inertia in an Empty Universe

After Gödel, this entire field of non-Friedmannian models has flourished.⁷ The most relevant model for our discussion is the Ozsváth-Schücking “Anti-Mach” metric.⁸ This is a universe with no matter in it, but it does have a non-vanishing Riemann tensor, i.e. it will display inertial features! Isn't that terrible for the original intuition leading to Mach's principle? However, the physical picture can now perhaps be saved through a “broader” interpretation: this is a world filled with gravitational radiation, entirely self-generated. To particle physicists who think in terms of gravitons, this isn't very strange, since the gravitons couple to anything carrying energy, including gravitons. It is just like a Yang-Mills Isospin gauge field,⁹ which is coupled to itself since it carries isospin. Indeed, the replacement of current-commutators by the corresponding Yang-Mills field commutators¹⁰ is an exact analog, since it represents a hadron system in which only the Yang-Mills field is left. Returning to Mach, we now feel that Mach's original idea was at least somewhat ambiguous!

4. The Actual World Obeys Einstein's Theory and Is not Relativistic in the General Sense

Our fourth step in this survey of the non-Machian aspects of Einstein's theory of gravitation relates to space-time itself. Clearly, if absolute rotation is allowed by the theory, we should be prepared to discover additional anti-relativistic aspects in this supposed General Theory of Relativity! Indeed, this has led Synge¹¹ to rewrite the theory, emphasizing his view (or Minkowski's, according to Synge) which considers Einstein's theory of gravitation as a theory based on an *absolute space-time*,

⁷See for example, O. Hackmann and E. Schücking, “Relativistic Cosmology” in L. Witten ed., *Gravitation*, J. Wiley and Sons, New York and London, 1962, Ch. 11, p. 438.

⁸Ozsváth and E. Schücking, “An Anti-Mach Metric,” in *Recent Developments in General Relativity* (The Infeld Festschrift), Pergamon Press, New York—Oxford-Paris-Frankfurt a.M. and PWN (Polish Scientific Publishers), 1962, p. 339.

⁹C. N. Yang and R. L. Mills, Phys. Rev., 96 (1954), 191.

¹⁰T. D. Lee, S. Weinberg and B. Zumino, Phys. Rev. Letters, 18 (1967), 1029.

¹¹J. L. Synge, *Relativity: the General Theory*, North-Holland, Amsterdam, 1964, preface p. 9.

. . . "However, we need not bother about the name (Relativity), for the word 'relativity' now means primary 'Einstein's theory,' and only secondarily the obscure philosophy which may have suggested it originally."

N. Rosen has recently inspected the inertial systems realized in an Expanding Universe.¹² This is now a plain non-exotic Conformally-flat Universe; still, Rosen finds that even though local gravitation is entirely "relativistic," the Universe is not. Anisotropy experiments, and even a Michelson-Morley experiment, will detect motion with respect to that "cosmic" frame. Indeed, the times T_1 and T_2 for the return of the two light-rays travelling parallel and perpendicular to the earth's motion will differ by a very small amount, depending entirely upon the expansion rate of the universe. For paths of the order of the distance to the nearest quasar, 3C 273 (about 3 billion parsecs) it amounts to some 2 hours! Experiments are indeed being done now to detect our "proper motion," through the anisotropy it should create in the 3°K "background" radiation filling the universe.

The fact that we are now discussing the whole, rather than the part, goes beyond Einstein's original philosophy. As long as we do not introduce intrinsic structure in space-time via the choice of a Gödel type cosmology, this is indeed the General Theory of Relativity, when applied to a localized problem. However, the same equations enabled Einstein to make a further conceptual jump, and to invent Cosmology. It seems that it is at this stage that he "unrelativitized" his theory. Mathematically, we are only feeding in the "particular" data corresponding to this problem. However, there is only one universe, and it seems improper to regard it as just one particular set of data. Rosen shows that it does define a preferred frame of reference; of course, this is the one corresponding to the matter at large, which is just what was needed to make the local inertial effects become relativistic.

I think that at this stage we can admit that Mach's Principle, though an excellent trigger to Einstein's creativity, has by now been overtaken by the resulting theory, and left behind conceptually. Intuitive pictures are essential to a theoretician's progress, but his mathematically for-

¹²N. Rosen, "Inertial Systems in an Expanding Universe," Proceedings of the Israel Academy of Sciences and Humanities (Section of Sciences), 12 (1968).

mulated creations transcend that kind of thinking and bring in new pictures, ever closer to the physical world he describes.

C. UNITARY SYMMETRY AND THE STRUCTURE OF HADRONS

1. Unitary Symmetry

I shall now try to analyze objectively (I hope this is possible, even though I have been involved at the personal level) the sequence of ideas which led to the introduction of Unitary Symmetry in hadron physics. Hadrons are particles which react to the Strong Interaction (i.e. the strong nuclear force). We now have experimental proof that they can be classified^{13, 14} according to the unitary representations of the group U(3), for each spin, each parity and charge-parity assignment. Hence, since classification implies a clustering in the energy levels, one was led to believe in an approximate symmetry of the strong Hamiltonian (or

¹³U(3) is the group of unitary 3-dimensional matrices; its simple subgroup is SU(3), the group of unitary unimodular 3-dimensional matrices. The observed hadrons all belong to representations of a subgroup $SU(3)/Z(3)$ of SU(3), in which Z(3) is the center, i.e. the subgroup commuting with the entire group. Z(3) contains the 3 elements $\{\exp(2\pi i/3), \exp(4\pi i/3), 1\}$.

The generator algebra of $SU(3)$ and $SU(3)/Z(3)$ is Cartan's A_2 , the traceless matrices in 3-dimensions over a complex field. In $U(3)$ there is in addition an identity-generator corresponding to baryon-number. For further details, see:

M. Gell-Mann and Y. Ne'eman, *The Eightfold Way*, W. A. Benjamin, N.Y., 1964;

Y. Ne'eman, *Algebraic Theory of Particle Physics*, W. A. Benjamin, N.Y., 1967.

¹⁴The $SU(3)$ classification was suggested as a formal realization of the Sakata model by:

M. Ikeda, S. Ogawa and Y. Ohnuki, *Prog. Theoret. Phys.*, 22 (1959), 715; Y. Yamaguchi, *Prog. Theoret. Phys. Suppl.*, 11 (1959), 1;

O. Klein, *Arkiv Fysik*, 16 (1959), 191;

W. Thirring, *Nucl. Phys.*, 10 (1959), 97;

J. E. Wess, *Nuovo Cimento*, 15 (1960), 52;

It was independently developed on the basis of the (later observationally confirmed) octet model for baryons (the "Eightfold Way") by:

Y. Ne'eman, *Nucl. Phys.*, 26 (1961), 222;

M. Gell-Mann, Cal Tech report CTS 20 (1961), unpublished at the time. The original draft of my above mentioned paper was an entirely independent identification of the role of $SU(3)$ in hadrons. I was not aware of the Japanese work at the time. It was upon submitting my results to A. Salam that I heard from him about Ohnuki's talk at the 1960 Rochester conference and was given the Ikeda et al. preprints. I then cut out of my paper the entire mathematical introduction (thus making it almost unreadable . . .) and reexpressed my simple matrices as linear combinations of the rather more complicated matrices of the Nagoya group.

S-matrix) under the isomorphisms of $U(3)$. All this emerged in 1959-1961; at the present time we have indeed direct evidence of the symmetry itself, in the form of symmetric couplings, i.e. a symmetric law of force. Moreover, by Noether's theorem we can regard Unitary Symmetry as indicative of the conservation of Unitary Spin (the Generator Algebra of $SU(3)$), an eightfold complex, in addition to the well-known conservation of baryon number.

2. Emergence of Strong Interactions and the First Hadron Model

The story of the discovery of the strong interaction goes back to Chadwick's discovery of the neutron in 1932 and the subsequent realization of the need for a new force to bind it (and the protons) in nuclei. This imaginative jump was taken by Yukawa and Stuckelberg in 1935, in the form of generalization of the idea of the electromagnetic potential: the nuclear force was assumed to be generated by a short-range potential

$$\phi(r) = - \frac{G}{4\pi} \frac{e^{-\mu r}}{r}$$

The parameter μ which fixes the range ($1/\mu$) corresponds to a mass. In a corpuscular picture, this is then the mass of the "exchanged" particle. With the subsequent discovery of the pion by Lattes, Occhialini and Powell in 1947 (after the π - μ confusion to which we shall later return) we also observe the emergence¹⁵ of the first hadron "model," suggested by Fermi and Yang in 1949. This hypothesizes that the pions are bound states of the nucleon-antinucleon system, and attempts to calculate the parameters of the necessary binding force. Such a calculation is still problematic today, even though our knowledge of strong interaction dynamics has increased tremendously. It was certainly very speculative in 1949. However, all quantum numbers other than the mass (or energy level) could be reproduced by this "model."

3. Nagoya Dialectics

I shall describe in succeeding paragraphs a certain sectarian approach which developed in Japan after World War II. It is interesting that in

¹⁵E. Fermi and C. N. Yang, Phys. Rec., 76 (1949), 1739.

the over-enthusiasm of a politically-motivated telling of history, the Fermi-Yang model has been totally disregarded* (see p. 23 of ref.¹⁶). Instead, Fujimoto told the Lenin Symposium that the 1955 introduction of the Sakata model¹⁷ was a "revolutionary development" because it suggested a "composite model of elementary particles." As we shall see, the Sakata model only differs from the Fermi-Yang idea by the addition of the Λ hyperon as the third sub-particle, a necessary extension after the discovery of strange particles. Quoting Fujimoto, however, Sakata's "revolutionary" step was

. . . "a step forward with the dialectic-philosophical view of Nature—the strata-structure of Nature—and destroyed an old belief that the elementary particles were the ultimate element of matter, and he considered the existence of the fundamental particles as the substance of the stratum existing deeper beneath the stratum of elementary particles."

In the next sentence, Fujimoto relates this view to his (and Sakata's) basic dogma

"One may say that he (Sakata) re-discovered in modern physics the *inexhaustibility of an electron* expressed by a famous phrase of Lenin's."

4. Strange Particles

Let us return to the historical sequence, before discussing this so-called "Nagoya" philosophy.

At about the time that the pion was discovered, Rochester and Butler¹⁸ observed "V events." Various hyperons and mesons were grad-

*I have since come across the following illustrations of this point "A large difference should be pointed out between the Sakata theory and the theory of Fermi and Yang. Main concern of Fermi and Yang's theory was on calculation of a bound state of a nucleon pair which could correspond to a π -meson, while the Sakata theory was proposed to disclose internal structure of elementary particles in terms of their composite nature" (Y. Fujimoto, appendix to Taketani's letter to the Nobel Committee, published in *Soryushiron Kenkyu* (Study of Elementary Particles) Nov. 1970, p. 262). This seems to me like empty gobbley-gook.

¹⁶Y. Fujimoto: Presentation of S. Sakata's "Theory of Elementary Particles and Philosophy" to the Lenin Symposium, 1970. (English Version published by the Department of Physics of Nagoya University.)

¹⁷S. Sakata, Prog. Theoret. Phys., 16 (1956), 686.

¹⁸G. D. Rochester and V. V. Butler, Nature, 160 (1947), 855. A previous event had been observed in 1944 by Leprince-Ringuet.

ually identified, but one was faced with the puzzle of their rapid production and slow decays. This was solved in two steps between 1952–53: associated production¹⁹ and strangeness.²⁰ Attempts had just been made at an explanation which would be based upon “known” features only, such as centrifugal barriers. However, a phenomenological analysis and the various evidence about spins etc. seemed to indicate that the symmetries of space-time weren’t involved. Realizing that in a new field of exploration it is legitimate and necessary to generate new concepts as you progress, Nakano and Nishijima and independently Gell-Mann introduced a new “internal” quantum number, strangeness or hypercharge. “Internal” is a misnomer; it simply implies a quantum number which doesn’t involve the structure of space-time, such as electric charge (at the present stage of the development of physics, at least). Here was a Keplerian advance, the observation of a regularity and its mathematical description, without first attempting to suggest a “good physical explanation.” Such suggestions did follow—for instance, Feshbach’s generation of strangeness as an effective selection rule due to para-statistical behavior of the strange particles (a hypothesis which has been disproved experimentally); none worked, and strangeness is still an abstract concept to date. It proved—and still is—extremely useful, though it hasn’t yet found its Newton, taking the analogy with Kepler’s laws of planetary motion.

5. The Sakata and Octet Models

Now back to the question of a hadron “model.” With strange particles around, the Fermi-Yang model had to be extended. Goldhaber and others²¹ replaced it by the sets (NK) or (Λ, K^+, K^0), i.e. three or four basic states rather than two as in the nucleon-pion case. Sakata¹⁷ propounded the more elegant solution of (p, n, Λ). Since the dynamical theory of the binding was obscure, the whole approach made very little progress until it received in 1959–60 an algebraic formulation in the form of Unitary Symmetry.¹⁴ The fact that 3 basic complex fermion states could reproduce the quantum numbers predicted by the Gell-Mann Nis-

¹⁹A. Pais, Phys. Rev., 86 (1952), 663. A similar though less formalized suggestion was made by Nambu, Yamaguchi, Nishijima and Oneda in 1951.

²⁰T. Nakano and K. Nishijima, Prog. Theoret. Phys., 10 (1953), 581; M. Gell-Mann, Phys. Rev., 92 (1953), 833.

²¹M. Goldhaber, Phys. Rev., 101 (1956), 433. Similar suggestions were made by G. Györgyi, R. Christy, G. Dérdi, M. A. Markov and Y. B. Zel'dovich.

hijima formula—and that the basic states were close in their energy levels —this fact could be abstracted in a generalized higher internal symmetry based on the Lie group $U(3)$. This also answered a quest for a “global” symmetry, started by Gell-Mann and Schwinger in 1956, a quest for a law of force which would relate π and K couplings.

However, the Sakata model was based on the assignment of a special role to the Λ hyperon, relatively to the Σ or Ξ . There seemed to be no experimental justification for such a choice. Indeed, the model assigned the Σ to a multiplet where it had to appear with another nucleon-like state and a Z^+ , positive strangeness hyperon. For the Ξ , it had to assign it to the same multiplet as the famous Fermi $I = 3/2, J = 3/2$ resonance of the $N\pi$ system. It thus predicted that its spin would be $J = 3/2$, like that (Δ) resonance. One is reminded of Newlands’ “model of the octaves” in pre-Mendeleev chemistry, where gold was put in a column with chlorine, disregarding the experimental situation. The interest in a materialist lower stratum as predicted by Dialectical Materialism had indeed been useful in triggering the introduction of the $U(3)$ group. However, the motivation was so overwhelming that it overshadowed the experimental facts.

Independently of these developments which were based on the Sakata model, and of which I was entirely unaware, I was trying in the second half of 1960 to find that “global symmetry.” In the context of a methodical search through the classification of Lie algebras, I hit upon the possibility of identifying $(N, \Lambda, \Sigma, \Xi)$ as an $SU(3)$ octet; this still enabled one to assign the mesons to a similar octet, since the product of baryon and antibaryon octets contained octets,

$$\underline{8} \times \underline{8} = \underline{8} + \underline{10} + \underline{10^*} + \underline{8} + \underline{27} + \underline{1}$$

(just as in the Sakata case, where $3 \times 3 = 8 + 1$). Thus both models predicted the existence of the eighth meson (the η at 560 MeV, discovered in 1961) but the octet (also suggested simultaneously by M. Gell-Mann in his unpublished “Eightfold Way”) predicted $J = \frac{1}{2}^+$ for the Ξ , and even $\Sigma-\Lambda$ relative parity, and put Fermi’s Δ resonance in a 10 or a 27. The latter choice was subsequently excluded by the observation of the Goldhaber gap, i.e. the inexistence of positive-strangeness resonances between 1000–1800 MeV, so that Gell-Mann and I

both settled on the 10 and predicted the existence of the Omega Minus.²²

As a lesson in the psychology of research, it is worth noting that nobody knew the ins and outs of $SU(3)$ in 1960 as well as the Nagoya group. Still, they did not observe the seemingly obvious better fit provided for the baryons by that same octet representation that they were using for mesons! Yamaguchi, who was at that time at CERN, tells me he did indeed think of that possibility though he never published it. Even in 1962 he still considered it so implausible that as chairman of the Symmetries session of the CERN ("Rochester") conference, he did not think that the octet model deserved discussion at the meeting, and I was denied the possibility of presenting the first strong evidence of the failure (in pp annihilation into 2 mesons) of the Sakata model (a selection rule discovered by Lipkin et al.) and of the good fit provided by the octet.²³ The observation of the Ω^- hyperon at the predicted energy (with strangeness -3) provided a final spectacular confirmation of the octet model²⁴ even though it had practically been confirmed adequately by that time through a variety of other predictions.

6. More about Nagoya Dogmas

I cannot pretend to be an expert on the history of that unusual group in theoretical physics centered around S. Sakata and M. Taketani. Let me first state that both these physicists made important contributions to the development of nuclear and particle physics. S. Sakata, who died in 1970, predicted the existence of π^0 in 1937; in 1946, he published²⁵ with T. Inoue a solution to the riddle posed by the muon's properties (which had proved very different from those expected from Yukawa's

²²M. Gell-Mann made the prediction in a remark from the floor at a final plenary session (Proceedings 1962 International Conference on High-Energy Physics at CERN, p. 805). I had submitted a similar suggestion in a written communication presented to G. Goldhaber earlier during the meeting.

²³P. T. Matthews, M. Rashid, A. Salam and H. J. Lipkin, C. A. Levinson and S. Meshkov, *Phys. Letters*, **1** (1962), 125.

²⁴V. E. Barnes et al., *Phys. Rev. Letters*, **12** (1964), 204. Since then about 30 Q events have been observed. At the time of the writing of this article, G. Goldhaber et al. have just announced the observation of a Q in a SLAC bubble-chamber picture.

²⁵S. Sakata and T. Inoue, *Prog. Theoret. Phys.*, **1** (1946), 143;
Y. Tanikawa, *Prog. Theoret. Phys.*, **1** (1946), 200;
M. Taketani, S. Nakamura, K. Ono and M. Sasaki, *Phys. Rev.*, **76** (1969), 60;
R. E. Marshak and H. A. Bethe, *Phys. Rev.*, **72** (1947), 506

meson, with which it had wrongly been identified). This was the "two meson theory," suggested independently and somewhat later by Marshak and Bethe in the U.S. Sakata also made contributions to the renormalization problem (his was a "regulator" type solution) etc. As to Taketani, besides important contributions to the study of nuclear forces, he has also authored a theory of the methodology of science. His theory identifies a recurring cycle made of three stages—phenomenological, substantial and essential (see pp. 4–9 of ref. ¹⁶). Thus my emphasis upon patterns would relate to a detail in their phenomenological stage, while Sakata and Taketani emphasize the next step in which one looks for structure. Indeed they really jump from experiment to structure and point to cases where this happened, such as in Yukawa's meson idea.

The Nagoya dialectics school suffers from a strong bias against the U.S. and the West in general. This may in part be a residue of the Second World War, but there are also personal grudges. The "comments" by Taketani in the 1965 Yukawa conference²⁶ are enlightening. Let us quote "comment 2" in full;

"Some English speaking people talk ten words when we talk one word, and do hardly take care of one word which we talk. This point is one of our complaints in any international conference. We should like to ask English speaking people to hear about our talks with the special care. Otherwise our attendance to the international conferences would lose its true meaning, and we are led to consider that we are not welcomed as a matter of fact. To my present lecture the above statement will be applied. The disadvantageous conditions which are imposed upon us will not be essentially improved even if a certain person would take trouble to invite some of us as a result of personal good will." "Comment 1" refers to the two-meson theory. We quote the essential points;

"The theory of two mesons and two neutrinos proposed by Sakata and his co-workers should be noticed in its remarkable perspectives and completeness by itself. But the theory has been intentionally neglected by some of the foreign physicists (see, for instance, A. S. Wightman, *Britannica*, 1957, 343B) and also suffered from the unjust critics at its unimportant points, which I think is entirely an unfair matter. I shall, therefore, repeat here the essential points of the theory.

²⁶Proceedings of the International Conference on Elementary Particles (Kyoto 1965), p. 170.

"The Sakata theory was published in 1943 in Japanese. The translation of it in English was made public in 1946. Until 1940, it became clear that the cosmic meson just did not possess the properties of the Yukawa particle which had been introduced on the basis of the nuclear force. The difficulties of the meson theory, which were a puzzle at that time, were pointed out in the discrepancy between theory and experiment, for instance, on the cross sections of scattering and absorption of meson in matter, the energy loss of meson due to the electromagnetic interactions, and the decay lifetime of meson."

"Here they had to introduce the existence of the neutral meson (muon) n besides the charged ones m^\pm . About the possible properties of the neutral muon they remarked in the following way: 'neutral meson which is assumed in the following discussions to have a negligible mass, and consequently may be regarded as equivalent with the neutrino.'

"This statement clearly leads us to the two-neutrino theory, in which ν (a partner of e) is distinguished from n (a partner of m) and n is assumed to have the mass of the negligible magnitude and, therefore, can be equivalent with the neutrino.

"Is there anything to be added to their statement in order to give the correct theory for the two-meson problem? On the basis of the above argument, we proposed that n should be called Sahatorino while ν should be called Paulino."

"We believe that the paper of Sakata and Inoue were received by the workers in U.S. in the year 1947. In 1948, the other paper on the two-meson theory by Taketani, Nakamura, Sasaki and Ono was sent to U.S. It is regrettable for us to find some workers in the major country insisting that the article which they did not read could have no contribution to the progress of science. We know that the works done by Sakata and his co-workers made the important contribution at least to the progress of physics in Japan. No one will deny that the achievements made by Japanese workers played an important role in the international developments of the meson theory."

For completeness' sake, we also quote from Marshak's answer:²⁷

"I have been involved in this question of priority with some of my Japanese colleagues. Now Profs. Sakata and Inoue without question sug-

²⁷R. E. Marshak, on p. 180 of ref. 26.

gested the two-meson theory several years before I did in my paper with Prof. Bethe. But due to the war their paper did not reach to U.S. until 1948, which was at least 6 months after I presented my theory at the first Shelter Island Conference. In their paper, Sakata and Inoue, working within the framework of the Möller-Rosenfeld model, considered two mesons, a heavy and a light one, we call them π and μ now, and they deduced a lifetime of 10^{-21} sec for the decay of π . I don't blame them for deducing such a short lifetime, which is too fast by a factor 10^{18} , because they were basing their work on the scattering experiments. I had the benefit of the Conversi, Pancini and Piccioni experiment and I deduced a lifetime of 10^{-8} sec which turned out to be correct. On the other hand, Sakata and Inoue assumed the spin of π to be 0 and the spin of μ to be $1/2$ while in my paper I assumed the reverse in making an illustrative calculation of the lifetime. Ever since then, Sakata claims that he had the correct two-meson theory. I say that we were both wrong. I mean, he had the wrong lifetime, I had the wrong spin. Or perhaps a more flattering way of saying it is that we both made contributions and we were both equally thrilled by Powell's discovery of the pion."

It is thus a combination of an anti-West bias, suspicion of American physicists' motives, some just resentment over important contributions which were disregarded for a time in the West—all these mingle with genuine belief in the ideas of Marx, Engels, Lenin as relating to atomic physics. It is interesting that the latter belief is an extreme orthodoxy which can only be compared with a fundamentalist's attachment to the biblical story of Genesis, or the resistance to the study of the theory of evolution in some southern states in the U.S. The dogma itself is simple enough, as can be seen in this quotation from Sakata's 1965 paper:²⁸

"One may quote the following two points as remarkable features of the physics of the present century. The first is the *recognition of the strata-structure of Nature*, in particular the discovery of a series of new strata of the microscopic world, namely, molecules—atoms—atomic nuclei—elementary particles. The second is the recognition of a limit of validity of the physical laws, in particular the discovery that the Newtonian mechanics is not the eternal truth of perfection. As a result

²⁸Published in Japanese in Kagaku, on the occasion of the thirtieth anniversary of Yukawa's theory. English translation included in ref. 16; see pp. 3–4 of that source.

it established the following point of view for Nature: *there exist in Nature an infinite number of strata with different qualities amongst each other, including the nebulae and the esolar system as examples of the large scale, and the molecules, the atoms and the elementary particles as examples of the small scale.* Each of those strata is governed by its respective and proper laws of physics, and all of the strata are always in the middle of creation and annihilation, and they compose Nature as the one and whole unified existence through their correlation and mutual dependence among themselves. *This point of view is called the dialectic-philosophical view of Nature, and it was already put forward by Engels at the end of the nineteenth century.* One may say as a conclusion that the atomic physics of the twentieth century re-discovered the dialectic-philosophical view of Nature.”

7. The Doctrine of Inexhaustibility and the Bootstrap

The doctrine would be tame, were it not for the assertion of the existence of an *infinite* sequence of strata. It may of course be true: if nature is not an a-priori mathematical construct, it is possible that our physical analysis is just some kind of series expansion, with an infinity of terms. However, considering for example that we know nothing about happenings under 10^{-16} cm, any such statement is entirely non-scientific. Indeed, there are even indications that it may well be wrong: on the one hand, if we extend the quantization procedure to gravitons, space-time itself becomes quantized at 10^{-33} cm. This would then set a lower limit. On the other hand, we are now witnessing at Berkeley and elsewhere another attempt at the description of hadron matter, pursued with an almost equally dogmatic single-mindedness, and in which the basic motivation stems from the belief that we have already reached the end of the way. This is the “bootstrap” movement, in which all hadrons are believed to be dynamical constructs satisfying self-consistency conditions. This is like Aristotle’s hyle, in contradiction to the “atomistic” (or strata) approach. Quoting Chew:²⁹

“The revolutionary character of *nuclear particle democracy* is best appreciated by contrasting the aristocratic structure of atomic physics

²⁹G. F. Chew, on pp. 105–106 of M. Jacob and G. F. Chew, *Strong Interaction Physics*, W. A. Benjamin, New York, 1964.

as governed by quantum electrodynamics. No attempt is made there to explain the existence and properties of the electron and the photon; one has always accepted their masses, spins, etc., together with the fine-structure constant, as given parameters. There exist composite atomic particles, such as positronium, whose properties are calculable from the forces holding them together, but so far one does not see a plausible basis, even in principle, for computing the properties of photon and electron as we compute those of positronium. In particular the zero photon mass and the small magnitude of the fine-structure constant appear unlikely to emerge purely from dynamics. Among strongly interacting particles, on the other hand, we have yet to see very small masses or other properties that cannot plausibly be attributed to a dynamical origin.

“The bootstrap concept is tightly bound up with the notion of a democracy governed by dynamics. Each nuclear particle is conjectured to be a bound state of those S-matrix channels with which it communicates, arising from forces associated with the exchange of particles that communicate with ‘crossed’ channels. (The principle of crossing is reviewed in Chapter 1.) Each of these latter particles in turn owes its existence to a set of forces to which the original particle makes a contribution. In other words, *each particle helps to generate other particles which in turn generate it*. In this circular and violently nonlinear situation we shall see that quite plausibly no free parameters appear, the only self-consistent set of strongly interacting particles being the one we find in nature.

“If the system is in fact self-determining perhaps the special strong-interaction symmetries are not arbitrarily to be imposed. No convincing explanation has yet been given for the origin of isotopic spin, strangeness, or the newly discovered eightfold way, but many physicists believe that the secret will emerge from requirements of self-consistency in a democracy. Hopefully the origin of these symmetries will be understood at the same moment we understand the pattern of masses and spins for strongly interacting particles—both aspects of the system emerging from the dynamics of the bootstrap.”

It is interesting to observe that each party likes to think of its approach as “revolutionary.” Actually, the drive provided by the bootstrap approach has indeed been extremely useful for the phenomenolog-

ical charting of the high-energy domain, through the introduction of concepts such as poles in the complex angular-momentum plane (these represent dynamical metastable particles with lifetime sometimes shorter than 10^{-23} sec). Much has also been learned with respect to the dynamics of strong interactions, the structure of the S-matrix and its analytical properties, etc. It is thus not excluded that for hadrons, the sequence of strata may have reached its end! However, this is at present also very far from certain, and seems contradicted by the quark hypothesis, which we shall discuss in coming paragraphs. Moreover, since the basic idea of the bootstrap is that self-consistency will allow the calculation of all parameters, quoting Salam:⁸⁰ "We may yet find that we are living (with Voltaire) not only in the best of all possible worlds, but indeed in the *only* possible world."

Now since any calculation is bound to leave out some feature in order to be manageable, its failure can be considered here as a success, as pointed out humoristically (on various occasions) by Salam and by Gell-Mann, and sometimes naively by the "bootstrapists" themselves. Actually, either the self-consistent system should break up into a number of such independently self-consistent systems, or else it can only be useful for the description of some particular general features, and can never become a physical theory, even if it is true in some absolute sense.

Returning to the doctrine of inexhaustibility of the strata, it is now clear that its exact negation has turned out to be just as useful a motivation in the study of hadrons!

The term "inexhaustibility" has been taken from a quotation often encountered in the writings of the Nagoya school: "Even an electron is an inexhaustible as an atom," (V. I. Lenin, in *Materialism and Empirico-Criticism*.) Two points should be made with respect to that quotation. First, it is often encountered in general articles in physics published in Communist countries—completely unbiased articles in which the actual scientific treatment is superbly free of dogmatic thinking. In such cases it just represents either a bow to a greater National or International leader, or a protective gesture in times when physicists have had reason to fear hostility from the regime. However, in the case of the Nagoya school, it is taken literally to represent revelation. It is somewhat in-

⁸⁰A. Salam, on p. 34 of *Contemporary Physics*, Vol. II (Trieste Symposium 1968), I.A.E.A. Vienna 1969.

congruous when Sakata, Taketani or Fujimoto refer to bootstrap physics as being religiously oriented, without noticing their own dogmatism:⁸¹

"In so far as the current view is adopted, various properties of elementary particles, which are introduced *ad hoc* into the theory, for example, masses, spins, symmetries, strengths and types of interactions and so forth, should be regarded as to be given by the *Providence of God*. Although there are recently some attempts to derive these elements by a self-supporting mechanism such as a bootstrap approach, the situation remains to be unaltered. Because such attempts, combined with the current view, will lead us to the philosophy of Leibniz that universe is in a pre-established harmony. Thus the current view will always introduce religious elements into the science and stop the scientific thinking at that stage."

Sakata held the same opinion about the phenomenological approach, if it did not bow to the inexhaustibility dogma:⁸²

"Here, a current abstract method of the group-theoretical approach will be useful only in preventing fixation of a certain concrete model gained at a certain stage of the experimental progress. Once one will forget this remark and will fall into a way of abstraction without any precaution, one will spread an inverted viewpoint of believing the ultimate aim to be a discovery of the symmetry properties as the 'providence of God,' and then the physics will fall down into one of the theologies."

The first quotation is taken from a paper which, after more of the same, goes on to present some extremely pertinent remarks on quarks and the like. It led F. Bopp, who was chairing the session, to make the following comment,⁸³ when opening the discussion which followed that paper presented by Sakata:

"Thank you very much Prof. Sakata for your talk. On an occasion of New Year's day in 1611, Kepler had written a letter to a friend on the sexangular snow. In this letter he tried to explain the structure of the snow crystals according to the view that it must be what we call today cubic package of spheres and by good observation and sharp reasoning, he came to the result that this was impossible and that he must

⁸¹Z. Maki, Y. Ohnuki and S. Sakata, p. 109 of ref. 26.

⁸²S. Sakata, on p. 18 of ref. 16.

⁸³F. Bopp, p. 119 of ref. 26.

replace the cubic package of spheres by a most dense one as we know it today. But this was impossible for him to believe and so he said that the atomic view must be from the Devil. I am feeling that we all have our leading ideas and we are all in danger to say that other ideas are from the Devil. But we are feeling from this letter of Kepler that by good observation and hard arguments we are coming to good results. So I propose to open the discussion with hard arguments. Thank you.

8. Effects of the Doctrine on Theory and Experiment

An important weakness which emerges from papers where the dogmatic elements preponderate is that they tend to encourage "hand-waving" at the expense of real theory. The ancient Greeks invented atomism; nevertheless it only became a theory when Dalton made it quantitative and related to observation and measurement. The Nagoya school now has a favorite doctrine of "*B* matter." This is something which turns a lepton (the electron, the muon and the two corresponding neutrinos) into a baryon. It was suggested by the Sakata triplet (p, n, Λ), which seemed "obtainable" from (e^-, e^+, μ^-) the supposed lepton triplet (prior to the discovery of the two neutrinos in 1962). The Sakata triplet can now be replaced by the hypothetical fundamental triplet suggested by Unitary Symmetry as the basis of hadron structure ("quarks"). However, there are four leptons! One can invent a variety of amendments, but since the entire idea contains nothing as far as actual dynamical computation, its only advantage was in the suggestive correlation of 3 hadrons to 3 leptons. Nevertheless, it still comes up as an example of "correct thinking" and a victory of the inexhaustibility dogma. There is no doubt that some future Dalton will indeed find the manner in which hadrons relate to leptons, and the inexhaustibilists will then claim the credit for their doctrine. . . .

Our next comment about that school relates to their attitude towards what they call³⁴ the "mist of positivism."

"The mist of positivism has been thick around all fields of science since the beginning of this century. It is a famous story that there were repeated fruitless discussions of skepticism about the objectivity of the atom among physicists, including Ostwald and Mach, even on the last night before the day when the internal structure of the atom was dis-

closed. The moment of proposal of Yukawa's theory was in the middle of a revolutionary development of nuclear physics, in which the positivistic philosophy of the Copenhagen school headed by Bohr was not able to have a correct perspective of this revolution."

There is no hesitation about quoting high energy experiments when they happen to lead to the validation of their own theories. However, they strongly resisted the building of a powerful accelerator in Japan in 1965, claiming that they could guess at all the answers anyhow, from their doctrine. A similar situation occurred in Europe in 1968, when Heisenberg opposed the building of the European 500 GeV machine. His main objection was based upon his "knowledge" that all the answers were already present anyhow in his nonlinear theory, another idea of the bootstrap type (the original one in fact) which being over-ambitious (an equation which should yield *all* particles, leptons and hadrons) has gone very little beyond the hand-waving stage in 15 years. These examples display some of the real dangers facing this part of physics whenever a doctrinaire approach wins over the combination of healthy unbiased experimental discovery and uncommitted abstract charting of new territory.

One last point relating to the "inexhaustibility" of the electron itself, as stated in Lenin's much quoted passage. There is nothing wrong, and perhaps even a certain perceptiveness, in a non-scientist such as Lenin guessing at further structure. It is however a sad development when a scientist falls back into the medieval way of preferring dogmas to actual physical theory. Fujimoto¹⁶ is scandalized at Gell-Mann:

"Repeatedly I want to mention that the Sakata theory has its essence in the philosophical method of discovering the 'logic of matter'—the fundamental particles—as a 'causa formalis' of the phenomenological regularities of the elementary particles, such as the symmetry property. In this point, there exists a fundamental difference between his point of view and views of the positivists. I might quote a statement of Gell-Mann as an example of the latter view. He stated in Tokyo in 1964 that he thinks not necessary to reject the point model of the elementary particle."

Well, it is a fact that present physical theory treats the electron as a point particle, and is entirely validated by experiments. It may well happen that indeed somewhere beyond present available transfer-momenta we shall discover structure in the electron, and Lenin will be vindicated. It hasn't happened yet, and the point picture is in fact the

³⁴S. Sakata, on p. 2 of ref. 16.

only available proper theory. To date we have no experimental indication of space-structure; neither do we have an alternative theory.

9. Quarks and $SU(6)$

We now return to developments in particle physics since 1962, with several more lessons pertaining to the irrelevance of concrete models.

The Sakata model, based upon a concrete set of "fundamental particles" making up the "elementary particles" had failed. The more abstract octet model provided a map of the world of particles; however, an octet is already a mixed tensor of the second order in $SU(3)$ (i.e. a piece of the $3 \times 3^*$ product of basic covariant and contravariant triplets). Alternatively, it is an unmixed tensor of the third order in $SU(3)$ (i.e. a piece of the $3 \times 3 \times 3$ product of 3 contravariant—or 3 covariant-triplets). There was thus little hope of regarding the octet itself as a building block. Moreover, there were various indications of structure even in protons and neutrons, such as that revealed by the scattering of electrons on nucleons. In a study performed with Haim Goldberg (Auphir) in 1962 at the Israel AEC Soreq Research Laboratory,³⁵ we suggested a Neo-Sakata like view, in which the fundamental field (or "model") would be represented by a triplet with baryon number $B = \frac{1}{3}$, so that a nucleon would be made of 3 such objects. This article, which appeared in *Il Nuovo Cimento* only on 1.1.63 (having been lost for a time by an editor in some drawer) went by almost unnoticed. This was due to the general preponderance of the Sakata model at the time; it was also due to bad writing, since it was very formal and did not point at experimental conclusions. Indeed, we did not know how seriously we should take our own suggestion—would these $B = \frac{1}{3}$ fields actually materialize as particles (with fractional charges!), or would they just stay as a mnemonic device? Alternatively, a theory might develop in which they wouldn't appear as single particles, but they would still play a fundamental physical role.

Some time in 1963, Gell-Mann arrived at the same idea. He published it³⁶ in 1964 in *Physics Letters*. By the time it appeared, the Omega

³⁵H. Goldberg and Y. Ne'eman, *Nuovo Cimento*, 27 (1963), 1; and report IAEC 725 (Feb. 1962).

³⁶M. Gell-Mann, *Phys. Letters*, 8 (1964), 214; G. Zweig, unpublished CERN reports 8182/TH.401 and 8419/TH.412.

Minus experiment had vindicated the octet, and the world of physics became interested in these triplets, which he named "quarks." Gell-Mann's paper was also extremely readable, and carefully pointed to the possible existence of these fractional charge states:

"It is fun to speculate about the way quarks would behave if they were physical particles of finite mass (instead of purely mathematical entities as they would be in the limit of infinite mass). Since charge and baryon number are exactly conserved, one of the quarks (presumably $u^{\frac{2}{3}}$ or $d^{-\frac{1}{3}}$) would be absolutely stable, while the other member of the doublet would go into the first member very slowly by β -decay or K -capture. The isotopic singlet quark would presumably decay into the doublet by weak interactions, much as Λ goes into N . Ordinary matter near the earth's surface would be contaminated by stable quarks as a result of high energy cosmic ray events throughout the earth's history, but the contamination is estimated to be so small that it would never have been detected. A search for stable quarks of charge $-1/3$ or $+2/3$ and/or stable di-quarks of charge $-2/3$ or $+1/3$ or $+4/3$ at the highest energy accelerators would help to reassure us of the non-existence of real quarks." A similar suggestion was made by G. Zweig, then at CERN.³⁶

In terms of our thread, here then was a Sakata-like model, unsuspected to start with, derived from the abstract identification of the octet, unbiased by a concreteness complex. To date, we do not know what its final role will be; it is certainly much more sophisticated than the (p n Λ) choice. Funnily enough, however, an almost identical lesson was to follow, still relating to quarks.

With the experimental confirmation of $SU(3)$ in the octet version, many workers felt that this was the time to tie up this "internal" symmetry with the "external" ones of space-time, i.e. the Poincaré group. I shall later relate the story of my own attempt in this vein and the identical methodological error which made me miss the point. Let us first mention the work of Gürsey and Radicati,³⁷ of Zweig³⁸ and of Sakita.³⁹ All of these authors conceived the idea of combining Unitary Spin (SU

³⁷F. Gürsey and L. A. Radicati, *Phys. Rev. Letters*, 13 (1964), 173.

³⁸G. Zweig, *Proc. of the 1964 Intern. School of Physics "Ettore Majorana,"* Academic Press, New York 1965.

³⁹B. Sakita, *Phys. Rev.*, 136 (1964), B1756.

(3)) and space-spin (an $SU(2)$ subgroup of the Poincaré group) for particles at rest, by considering the six states of a quark (spin-up and spin-down for each quark) as a basis for the group $SU(6)$. The question then arose of the $SU(6)$ assignment for the physical baryons. Considering the quarks as fermions, the physical baryons should correspond to the totally-antisymmetric (in spin J and $SU(3)$ indices) product of 3 quarks. This has 20 components, including 16 for the baryon octet (with $J = 1/2$) and a unitary singlet with $J = 3/2$. B. Sakita did make this assignment, which was the direct choice if you believed in concrete quarks. The other authors tried other representations as well as the 20. They noticed that the totally-symmetric product with 56 components fitted perfectly the baryon $J = 1/2$ octet and $J = 3/2$ decimet (containing the Ω^-). They could see several interesting applications to this marriage, and thus assigned the baryons to 56. This forced the quarks to have para-statistics!⁴⁰

A short time after publication of the 56 assignment, spectacular results started to appear, all of them pertaining to this choice. It turned out that it predicted a ratio of $-3/2$ between the magnetic moments of the proton and the neutron; the experimental figure is $-1.46!$ Since then, the 56 assignment has been accepted everywhere. Again, it was the freedom of picking an abstract representation which produced the right result, rather than the “good physical picture” of 3 quarks. And again, one could now readapt that “physical picture,” by replacing the quarks by paraquarks, corresponding to a type of statistics as yet unseen. Why not? In the words of Sakata:²⁸

“As soon as scientific research penetrates into a new and unknown stratum of Nature, physical concept and laws established in the old strata lose quite often their validity.”

Now to my own mistake. With J. Rosen, we were trying in 1963–64 to combine $SU(3)$ and space-time aspects.⁴¹ To ensure that we would not make the mistake (common to various other attempts in 1963–65) of generating $SU(3)$ transformations which wouldn’t commute with the Lorentz group, we simply adjoined the two metrics. We added to the Minkowski metric another six real dimensions in which we could represent $SU(3)$. This led us to a geometrical model involving general rela-

⁴⁰O. W. Greenberg, Phys. Letters, 13 (1964), 598.

⁴¹Y. Ne’eman and J. Rosen, Ann. Phys., 31 (1965), 391.

tivity and cosmology. Engrossed in this “good physical picture,” I never checked the enveloping symmetry group (which of course contained $SU(6)$) and its representations. Instead of trying to keep aloof from a concrete model I fell into that same “concrete” pitfall.

CONCLUSION

Let me end this story of trial and error with two remarks. First, about physics in Japan, which has taken up so much space in these comments. The Japanese contribution to Particle Physics has been of the first rank. Taking the period since the mid-fifties, they have produced several of the main leaders in the field: Y. Nambu, K. Nishijima, B. Sakita, J. J. Sakurai, K. Iggy, M. Suzuki, H. Sugawara, Y. Hara etc. In axiomatic field theory, H. Araki is a central figure. Nambu’s role is second to none. It should be noted however that most of that work was done in the U.S. In Japan proper, only Tokyo was relatively free of the doctrinaire atmosphere. It is only now that a gradual normalization is taking place. So much for the ill effects of dogmas.

The interaction of science and philosophy—or rather the influence of philosophy on science—may be useful at the level of the individual scientist, in triggering ideas. The results may or may not be relevant to the original philosophy. It can become disastrous if the link between philosophy and science is enforced by the intellectual establishment.

DEDICATION

My last remark is a dedication. I “discovered” the beauty of physics in 1940, at the age of fifteen, while reading Jeans and Eddington. This led me to attend evening courses given by Prof. Sambursky in a Tel-Aviv school; they were inspiring and augmented my interest, which was to be given a final boost through the teachings of Prof. Ollendorff at the Technion in 1944/5. I am thus repaying a debt in this meeting.

HOW CONSTANT ARE THE CONSTANTS OF PHYSICS?

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Newtonian mechanics rested on three fundamental building blocks, namely the concepts of distance, time and mass. These were completely independent. Physics at the time was neither able to set an absolute scale to any of these units, nor to relate one to the other.

Maxwell's theory of electromagnetism was the first to introduce a fundamental constant into physics. The velocity of light c , which was a parameter of the theory, set a joint scale to distance and to time. Time intervals could be measured by the distances which light traveled during those intervals. One could ask why the speed of light was unique, and in particular independent of one's frame of reference. As is well known, this question led to the special theory of relativity. But there was still no point in asking why the speed of light was 300,000 km/sec, since the definition of the meter and the second were arbitrary. This question remained unanswerable in principle.

In order to set an absolute scale to both spacetime and mass one had to wait for two additional fundamental constants of atomic theory. There are, however, more of these than are needed for this purpose. Theoretical physicists like to use Planck's constant \hbar in order to relate mass and distance. To each mass m they associate its Compton wavelength $\lambda_m = \hbar/mc$. All that remains is to choose the unit of mass, which could be, say, the mass of the proton m_p .

The remaining fundamental constants can now be expressed as dimensionless quantities. In the first place we have the ratio of the mass of every known particle to that of the proton, e.g. m_e/m_p . Then come the constants describing the interactions of physics. The strength of the electromagnetic force is proportional to the fine structure constant

$$\alpha = e^2/\hbar c = 1/137.03602(21)$$

Gravitation has a similar constant

$$Gm_p^2/\hbar c = 5.9050(27) \times 10^{-39}$$

The theory of nuclear forces is not yet known, but we have various dimensionless numbers describing the strong and the weak interactions. These numbers, and possibly others, determine the rate of nuclear reactions and the energies and widths of nuclear levels, although we do not usually know how. The strong forces have defied attempts to describe them by means of a perturbation theory. However, a way has been developed of handling first order perturbation theory of the weak interactions. In this approximation nuclear beta decay rates are proportional to the square of the weak coupling constant

$$\frac{gm_p^2c}{\hbar^3} = 1.012(3) \times 10^{-5}$$

To sum it up, we have three fundamental constants that are unique and set the scale to all physics. Their value in arbitrary units cannot be questioned. All other constants can be expressed as pure numbers, and one *can* ask why Nature chose to assign these constants their specific value. Most physicists would agree that our knowledge is incomplete until we are able to explain all these numbers in a unified theory.

Two points should be stressed. First, the choice of the three "scale setters" is arbitrary. Any three independent ones will do. Secondly, all constants are a consequence of a theory. But theories are good only as long as we are unable to refute them, and their experimental consequences keep on being confirmed. Thus the fundamental constants are constant only to the extent that we can establish that they do not vary. In particular they may appear to us to be constant over a time span of the order of one hundred years, but may really be changing on a time scale of the order of billions of years.

Several authors, such as Dirac,¹ Sambursky,² and Gamow,³ have attempted to introduce such slowly varying constants. But experiment has set more and more stringent limits on their variability. This review concerns itself with the present experimental limits.

The dimensionless quantities are easier to investigate. Most of them describe the strength of interactions. There is little a priori difficulty in

¹P. A. M. Dirac, *Nature*, 139 (1937), 323; *Proc. Roy. Soc. A* 165 (1938), 199.

²S. Sambursky, *Phys. Rev.*, 52 (1937), 335; S. Sambursky and M. Schiffer, *Phys. Rev.*, 53 (1938), 256.

³G. Gamow, *Phys. Rev. Lett.*, 19 (1967), 759.

admitting that they need not always have been at their present strengths. Furthermore, there are stricter experimental tests one can apply. We have very good upper limits on the variation of most of these constants.

Bahcall and Schmidt⁴ made a direct check of the constancy of the fine structure constant α . They compared the fine structure splitting of an oxygen OIII emission line on earth and in the spectra of five distant radio galaxies. The redshifts of the galaxies investigated were all about $z = 0.2$, indicating that their light was emitted approximately 2 billion years ago. That is a sizable fraction of the age of the universe. The ratio $\Delta\lambda/\lambda$ of the splitting of the line to its average wavelength is a pure number proportional to α . It is independent of redshift. Bahcall and Schmidt found this ratio to be equal in the spectra of the galaxies and in those of sources in the laboratory. They concluded that

$$\propto (z = 0.2) / \propto (\text{lab}) = 1.001 \pm 0.002.$$

Once the constancy of electromagnetism has been so well established, we can test other interactions versus it. Dyson⁵ considered the binding energy of heavy nuclei. These are small quantities which are the difference between two large ones, namely the strong nuclear binding and the electrostatic repulsion. A slight shift in one of them would change the binding energy by an appreciable amount. This would affect the rate of β decay, which is very sensitive to the energy difference Δ between the initial and final state, going at least as fast as $\Delta^{-2.835}$.

The possibility that the nuclear and electromagnetic forces vary in just the right way to keep the delicate balance is ruled out by the following argument. The electrostatic repulsion is linear in \propto to approximately 1%, but the nuclear forces are highly nonlinear. It is inconceivable that the balance could be maintained, except perhaps for a single nucleus.

Dyson actually presupposed the constancy of the nuclear forces, and tested the possible change in \propto . He concluded that, in order for Re^{187} and Os^{187} to have sufficiently long life times, so that they would not have by now been depleted, one had to set the limit:

$$-4 \times 10^{-13} \text{ y}^{-1} \leq (d\propto/dt)/\propto \leq 3 \times 10^{-13} \text{ y}^{-1}.$$

Dyson thus established the constancy of both electromagnetism, and

⁴J. N. Bahcall and M. Schmidt, *Phys. Rev. Lett.*, 19 (1967), 1294.

⁵F. J. Dyson, *Phys. Rev. Lett.*, 19 (1967), 1291.

the strong nuclear interactions. Peres⁶ gave an independent argument. He argued that if either of those forces had changed, the *stable* heavy elements must have had in the past a different ratio N/Z of neutrons to protons. This is because the deviation of N/Z from 1 is due to the electrostatic repulsion between the protons, and its relative strength to the nuclear binding. Thus different isotopes of the same *chemical* element would have different chemical elements as parents some time in the past, and there would be fluctuations in the isotopic compositions of elements coming from different locations on earth. These variations are known to be extremely small. An amusing difficulty arises with the possibility that electromagnetism was stronger in the past. A billion years ago Pb^{208} would have been Rn^{208} , which is a gas, and would therefore be uniformly distributed throughout the world. Lead ores are not.

Dyson's presupposed that the weak coupling g was constant. Actually even a large variation in g did not affect this argument, although it made his limits less stringent. The constancy of g can be demonstrated from the arguments of Gold⁷ and Chitre and Pal.⁸ These authors compared the dating of rocks by various methods. Summed together they show that four methods give the same age:

1. U^{238} spontaneous fission.
2. $\text{U}^{238}\text{-Pb}^{208}$ radioactive chain.
3. $\text{K}^{40}\text{-Ar}^{40}$ electron capture.
4. $\text{Rb}^{87}\text{-Sr}^{87}$ beta decay.

The first two rates depend on the strong and electromagnetic interactions, while the last two are weak decays. Had the weak interaction varied relatively to the other two, ages 3. and 4. would have been different. Gold's⁷ limit is a finer one for the weak coupling. It implies

$$|dg^2/dt|/g^2 \leq 2.8 \times 10^{-11} \text{ y}^{-1}$$

Here we also have a test of the constancy of m_e/m_p . The rate of electron capture relative to the competing β^+ decay is inversely proportional to the square of the electron mass. Thus we can set the limit:

$$|\frac{d}{dt} (m_e/m_p)| / (m_e/m_p) \leq 1.2 \times 10^{-10} \text{ y}^{-1}.$$

⁶A. Peres, Phys. Rev. Lett., 19 (1967), 1291.

⁷R. Gold, Phys. Rev. Lett., 20 (1968), 219.

⁸S. M. Chitre and Y. Pal, Phys. Rev. Lett., 20 (1968), 278.

A related quantity is the ratio of the magnetic moments of the proton and the electron $\mu_p/\mu_e = g_p m_e/2m_p$, where g_p is the gyromagnetic ratio of the proton. It can be determined by considering the hyperfine splitting of spectra. To check its constancy a method analogous to that of Bahcall and Schmidt⁴ has to be employed. However, wavelengths cannot be measured sufficiently accurately, if only because of the internal motions of the distant galaxies. In practice one measures the red shift of the 21 cm radio line and compares it with the red shift of the optical lines. Their equality indicates the constancy of the magnetic moments. Errors are large, however, and present limits⁹ are of the order of percents for $Z \leq 0.02$.

The suggestion that G may vary with time has aroused much interest. Dirac¹ suggested that it be inversely proportional to the age of the universe. Brans and Dicke¹⁰ developed a theory of gravitation different from Einstein's general relativity. In both theories G varies as a simple power of time

$$G = G_0 \left(\frac{T}{t} \right)^n$$

Here G_0 is the present value of G , T the age of the universe, and t time. In Dirac's theory $n = 1$. Brans-Dicke cosmology would have

$$n = 2/(4 + 3w)$$

where w is a parameter of their theory. Originally they suggested $w \sim 5$ which gives a very small $n \sim 0.1$.

Pochoda and Schwartzschild¹¹ checked the effect of such a varying G on the evolution of the Sun. If G were larger in the past so was the Sun's luminosity (approximately as the eighth power of G !). If 4.5 billion years ago (in our atomic units), when the Sun began to burn hydrogen, its luminosity was greater, because G was larger, it would by now have burned more hydrogen. If this larger amount of burned hydrogen exceeded a critical limit, the Sun would have gone off the main sequence, and would have turned into a red giant. It clearly has not yet done that.

⁹M. S. Roberts, IAU Symposium No. 44, Uppsala, Sweden, August 1970.

¹⁰C. Brans and R. H. Dicke, Phys. Rev., 124 (1961), 925;

R. H. Dicke, Rev. Mod. Phys. 34 (1962), 110.

¹¹P. Pochoda and M. Schwartzschild, Astrophys. J., 139 (1964), 587.

Pochoda and Schwartzschild concluded that $n = 1.0$ was permissible only if $T > 15 \times 10^9 y$, but that $n \leq 0.2$ was certainly compatible with present estimates of T . Attempts to devise more sensitive tests¹² have either failed or depended on details of theoretical models, which have not yet been verified.

Finally we arrived at the fundamental constants with which we have chosen to set the scales of physics m_p , c and h . We have no way of measuring them, since they set the scale of our measuring instruments. All we can do is to check their uniqueness at the present epoch. If any one of them changed uniformly all over the Universe, we would have no way of knowing this. The programs one can initiate are thus to measure these quantities with the same instruments for several different sources, and to check the constancy of the results.

According to special relativity mass, energy and momentum are related by

$$m^2c^4 = E^2 - p^2c^2.$$

We can measure the energies and momenta of many protons and ascertain that the difference $E^2 - p^2c^2$ is constant. This has been done for countless cosmic ray protons as well as laboratory ones. No deviation has been found.

Similarly the wavelength, frequency and speed of light are related by

$$c = \lambda\nu.$$

We can tune very accurately to a given radio frequency and then measure by interferometry the wavelength of the radiation coming from extra-terrestrial sources. One should be able to measure a 10m wavelength to an accuracy of $\sim 1\text{mm}$. This would verify the constancy of c to one part in ten thousand. As far as I know, nobody has attempted this experiment.

It should be mentioned that if we have two radiations with different c 's, the principle of superposition has to be given up. One cannot superpose simultaneously their electric and the magnetic fields, and still satisfy Maxwell's equations, unless the two radiations see different spacetimes.

Lastly we can measure the energy and the wavelength of visible light from far sources, and check the constancy of

$$hc = E\lambda$$

The wavelength is measured accurately by a diffraction grating. The energy is determined by one of the following:

1. The photoelectric effect.
2. The energy at which atmospheric absorption sets in.
3. The energy at which the sensitivity of a film falls off.
4. Diffraction by a prism, as compared with a grating.

Method (3) has already yielded the constancy of hc to about 10%, just by inspection of spectra photographed by numerous astronomers. A careful analysis of all methods could probably lower this to about 1% or even 0.1%. Very distant optical sources are, of course, available.

Here we also find a theoretical difficulty. A photon with h different from ours has its angular momentum quantized according to a different scale. If angular momentum is to be conserved in its interaction with one of our atoms, the quantization of angular momentum will break down. This argument does not apply to energy or momentum quantization, since one would argue that the frequency and wavelengths change. However, angular momentum is h , up to a numerical factor, and there is no escape.

The picture which emerges from all this analysis is that the fundamental constants have withstood our assault on them. We may in the future discover variations that are too small for us to notice today. On the other hand finer experiments may increase our trust in their constancy. Perhaps we should concentrate on attempting to understand the dimensionless numbers of physics. We might try to relate one to another.

¹²G. Shaviv and J. N. Bahcall, *Astrophys. J.*, **155** (1969), 135;
R. C. Roeder and P. R. Demarque, *Astrophys. J.*, **144** (1966), 1016;
D. Ezer and A. W. Cameron, *Can. J. Phys.*, **44** (1965), 593.

DISCUSSION

On papers by Y. NE'EMAN and A. YAHIL

ROSEN: Perhaps I should first explain why I should not be here. Like many other physicists, I know very little about philosophy. One is bound of course to use philosophy in physics but in my work I tend to do it more or less intuitively without any sophistication and for that reason I think that if I say anything connected with philosophy, the philosophers present will find what I say either naive or wrong. But since I am here, I will go ahead and do the best that I can.

First, I would like to make some remarks about Prof. Ne'eman's talk. Actually he said so many interesting things that were I to try to comment on the various points he made, it would take much too long. So instead of that I will refer to one aspect—the physicist as a human being—how much a physicist is influenced in his work by such things as his philosophy, his prejudices, his emotions and so on. Now, one point which perhaps might be mentioned is that human beings, for some reason or other, also have what you might call aesthetic feelings; they have a certain concept of beauty—even primitive people seem to have that—and physicists also bring it into their work very frequently. I hope Yuval will forgive me for saying this, but recently I was talking to somebody about elementary particles (about which incidentally I know very little). We happened to refer to Yuval's work on elementary particles—his Eightfold Way—and the man I was talking to said "Oh, that is a beautiful piece of work." I hear this expression "beautiful" in physics very often.

Einstein I think was very much influenced by such an idea of beauty, and he always judged theories from this point of view. Of course it is difficult to define what one means exactly by beauty. Certainly one aspect of it is simplicity. A physicist usually thinks of a simple theory as having beauty associated with it and in this connection, since Yuval mentioned the work of Dicke, I would like to refer to that for a moment.

You see the general theory of relativity itself is considered by everyone to be very beautiful—it has simplicity, it has very few elements, it has a certain elegance in its structure and so on. Now of course I don't mean to say that it is simple from the mathematical point of view—you have complicated differential equations—but it is simple from the stand-

point that it has very few independent elements. As it stands, then, it forms a beautiful whole, but here Dicke came along and tried to change it by adding another field—which he calls a scalar field—and many physicists feel that it somehow tends to destroy the beauty that was present previously. Of course it may be that whether we like it or not, we shall have to accept his point of view if it turns out to be necessary in order to agree with observation. But I think many of us at least hope that it won't be necessary, and here again emotion seems to come into physics.

This raises an interesting question when we try to bring such ideas as beauty into our thinking or into our evaluation of theories. One asks oneself, why should Nature be at all concerned with our ideas? Why should the fact that a certain theory is beautiful have any implication as to its connection with reality, with Nature? Isn't there something presumptuous in our trying to take our concepts, our standards such as that of beauty and imposing them on Nature? The only thought that comes to me here is this: that after all, human beings themselves are a part of Nature, and if we as human beings are aware of such concepts as beauty, then perhaps this awareness has something to do with Nature itself. It may not be too far-fetched to believe, therefore, that what we consider to be a beautiful theory may be more likely to have something to do with Nature than that which we consider to be an ugly theory. But that is of course only a speculation.

I would like now to go over to Dr. Yahil's paper and make a few remarks about it. Perhaps we could divide the paper into several parts. First, there is the section that deals with the idea that what we consider to be a constant of nature might be changing with time. If it were so, I think the physicist would naturally interpret it, not in the sense that the laws of Nature themselves are changing with time, but rather that certain conditions in the universe are changing with time and therefore affect certain quantities which we are trying to measure. Here the obvious change that one can think of is the expansion of the universe: its dimensions appear to be increasing and this could therefore conceivably affect certain 'constants.'

Now, in view of the fact that it looks as though such constants as the fine-structure constant and the ratio of the mass of the proton to the mass of the electron, and so on, do not seem to be changing with

time, there is the suggestion that there is no strong interaction between the micro-universe and the macro-universe. That is to say that the properties of elementary particles and small systems built out of these apparently are not greatly affected by, let us say, the curvature of the universe—which is something connected with the large scale phenomena. That is rather comforting, if it is true, because it makes the situation less complicated—we can isolate the small from the large and treat each one separately. Of course it may turn out eventually, when one carries out measurements to a greater degree of accuracy, that this is not so, but at least at present it looks this way and that is rather encouraging.

At another stage in his paper, Dr. Yahil discussed what perhaps one might call the question of consistency—that perhaps the velocity of light coming from one part of the universe might be different from that of light coming from a different part of the universe, when the two get to the same region of space, namely, where we are ourselves. If that were the case, that would really be a serious difficulty. We can imagine, as I said before, that the expansion of the universe could affect certain quantities and there would be no inconsistency here. But if we found that in our region of space, light from different sources behaved differently, this would certainly rule out the possibility of any field theory describing light, and would necessitate a tremendous revolution in physics. Up to now at any rate there is no reason to believe that we have such a difficulty, and I think every physicist would hope that such a difficulty will not arise in the future.

Dr. Yahil also raised the question—why are the constants of nature what they are? One hopes that some day better and more fundamental theories will be developed which will throw light on this question. Of course here the important constants to deal with are the dimensionless ones because they are the ones which would have an objective existence, so to speak, independent of our units and they are the ones which have to be explained. One can imagine that as physics develops, some explanation will be given in the sense that some of these constants perhaps will be found to be related to others and that will be a step in the right direction. But presumably there will still be some constants left over which need to be explained in themselves. That is to say, certain fundamental constants which are known to have certain values and which would have to be accounted for.

Now there may be various ways in which that could be done. Offhand one can think of two possibilities. One is that one has an equation which, when solved, will give an answer for the numerical value of a certain constant, for example, the fine structure constant. It could be perhaps the root of some algebraic equation. It could perhaps be the eigenvalue of some differential equation. At any rate we can imagine that a future theory, through its equations, will give definite values to certain constants.

Another possibility is that some of the constants that we know at present may turn out to be constants of integration of certain differential equations. In that case, the explanation as to why these constants have the particular values that they have is that at some time in the past, let us call it $t = 0$, they were given those values by whatever conditions were imposed on the universe, or existed in the universe, at that time. Perhaps that is a less satisfactory situation but it is hard to rule out such a possibility.

I mentioned before the fine structure constant as one which perhaps some theory will be able to account for numerically. On the other hand, one can't help wondering whether, for example, the larger number represented by the number of baryons in the universe, would ever come out as a definite number given by some theory—somehow one is more inclined to expect it to turn out to be a constant of integration determined by conditions that existed at the moment when the universe as we know it now came to exist.

Well these are the thoughts that came to me while I was listening to the previous speakers, and I think I would like to ask other people now to comment.

ROSENFELD: Well these have been very stimulating talks, including Rosen's comments, and I have only a few remarks to make about some of the questions that have been raised.

To start with, you mentioned this question of beauty which has even been systematized by Dirac. At one time he enunciated a "principle of beauty." This was at a time when everybody was in utter despair of ever understanding anything in field theory. Then he argued that one has not tried out all the beautiful theories of mathematics and that there was one theory that had been neglected, namely, the theory of functions of complex variables which he felt was a mine to be exploited. Well, they

are now used extensively, as you know, but I don't think that they are so fundamental as Dirac imagined.

Then you raised the question of what is beauty and suggested a criterion of simplicity. But then one may go one step further and ask, what is simplicity? You suggested that a criterion for simplicity may be the number of elements in a theory. But why should we be more satisfied by a theory with few elements than with another one? This is only a question of economy, which is of course a very strong motivation for all science, but this economy of thought must also be matched with the content that we want to put into a theory—the economy may be sometimes illusory. Think of axiomatization, which is popular with some people. What you do actually is to conceal with a few axioms the whole content, but—in order to get any information from this system of axioms—you have to work out this information by a process which is as long and painful as that used in order to conceal the information within the axioms. So where is the economy?

Then with regard to the question of the fundamental constants and their possible connections, I have the feeling that we can also think it is a beautiful thing to have a universal constant. Everybody was so admiring of Planck's discovery of a whole domain of nature which was dominated by one constant, the value of which was independent of the state of matter in interaction with radiation.

Yet, I feel that if one pushes this aesthetic approach too far there is an inherent danger. We have this feeling of beauty only relative to the state of our knowledge, and we must realize that at every stage in which physicists found themselves in the development of science, in their approach to a knowledge of the universe, they always had aesthetic feelings, though this knowledge differed vastly. For Plato the most beautiful figure was a circle, and that was fine for so long as the means of observation of celestial motions were so rough that a circle was a tolerable description for an orbit. I say tolerable, because even the ancients felt the need for complicating this. So this aesthetic feeling was actually misleading. There we come to Ne'eman's point that as soon as something becomes crystallized then it may cease to be a source of inspiration, but rather a source of inhibition. Thus we call Maxwell's theory beautiful because we know that it is a better approximation to electromagnetic phenomena than the theory which preceded it, but which Ampere himself

and Neumann and those people thought much more beautiful than this horrible system of Maxwell's.

I have the feeling that the relativity of our knowledge puts a very strong limitation upon any attempt to analyze our general views about the nature of physical theory. It is a case of increasing adaptation to a state of knowledge which is itself increasing owing to the growing complication—and here we come to the social aspects—of the social structure and the accompanying development of technology, which gives us a means of more detailed analysis of nature.

So to come to the point Yuval Ne'eman made about the symmetrization seen from various points of view. I was amused when he mentioned this inexhaustibility dogma that is found so beautiful by people who believe in it, but was actually felt to be a great conceptual obstacle by other people—Maxwell, for instance. He very acutely pointed out that a mechanistic explanation of the electromagnetic field by means of motions in an elastic medium was in danger of leading to a regressus ad infinitum, because ether had also to be conceived as a medium consisting of particles and one had then to introduce a sub-ether to explain how the ether particles react to each other. If one observes what certain contemporary physicists have made of this theory, one gets the impression that it was a very commodious device to escape from difficulties at a certain layer, to push them back into another that was more difficult of access.

Perhaps I may be allowed a last word, about the word "concrete" that is in your title but was not very prominent in your exposition. That is a very difficult word, because suppose we ask "what is a concrete picture?" It is a picture consisting of perceptions. What is a perception? It is a system of signals that are registered in our brain. And what is an abstraction? It is another system of signals of another group which is also in our brain.

TOULMIN: I have one difficulty in understanding the full force of Prof. Ne'eman's paper. It seemed to me that his central thesis depended upon a certain use of the word "philosophy" against which I feel obliged to protest. It is perfectly true that when one reads the newspapers, or the Saturday supplements, one is accustomed to seeing the word "philosophy" used to refer only to subjective eccentricities, expressions of political ideology and things of this kind. Of course, it may well be true that

there are circumstances in which one can speak of the propaganda use of a philosophically expressed ideology, in the course of disciplinary arguments, as an illustration of the influence of "philosophy" on "science"; but I should be very sorry if this was produced as typical of the manner in which philosophy proper had—either in the course of history, or at the present day—exerted an influence on science.

For, on the other hand, again and again one could demonstrate the creative value of a philosophically based methodology; and this does not drop out of sight at the moment when some adequate representation of natural phenomena is achieved, but maintains its life and significance within physics, even after some significant new representation of natural phenomena has been achieved. As against this, Prof. Ne'eman's attitude—as expressed for example in his remarks on the Nagoya Leninism—amounts to saying that in the last resort *only the abstract result counts*, while everything that went before drops out of sight.

Now the essential question here is: what counts as "counting"? By what standards do we decide what is to be regarded as "counting"? By what standards do we decide what is to be judged as a valid, genuinely and creatively new contribution, a proper extension of the scope of our theoretical representation of natural phenomena, and so on? And, when one looks at the historical development of our ideas about this matter, the creative influence of philosophical debate on the methods of theoretical physics is something which one cannot sweep under the rug, or dismiss as a sort of occasional piece of ideological prejudice which does more harm than good.

I have in mind, for instance, the kind of results that Alexander Koyré demonstrated, when he analyzed the influence of Descartes on Newton. In the tradition of Newtonian physics, as it was created from the late seventeenth century on, what counted as a valid, new extension of the theoretical representation of phenomena, was something which Newton had decided very much in the light of those general philosophical considerations which Koyré traced back to Descartes.

I don't want to say more here, but, if we are going to talk about the influence of philosophy in the actual doing of theoretical physics, let us look at this methodological level—at which there are substantive questions of theoretical methodology which retain philosophical roots—and not behave as if philosophy is involved in science only by way of ir-

relevant philosophical or ideological considerations which distort the argument.

LAKATOS: Perhaps I might add some remarks in amplification of Toulmin's comments. I am one of those orthodox philosophers who still insist on a *sharp demarcation between psychology of discovery and logic of discovery*. On the one hand we have the often tortuous psychological process by which a scientist arrives at a scientific result and on the other we have standards of appraisal, "logic of discovery," by which we *judge* this result as it is detached from the mind of the scientist and from the particular way he arrived at the result. What we judge is the result as it appears in books or periodicals. Professor Ne'eman showed us the role played by philosophical dogmas in the psychology of some scientists. I think—and I am sure Prof. Ne'eman will agree—scientists should have the liberty of being psychologically influenced even by the silliest philosophies: this is their private business. The scientific appraisal of their *achievements* (as opposed to their *inspirations*) is, however, a crucial problem, and I was disappointed that Prof. Ne'eman did not really discuss the relation of his topics to the problem of scientific standards. Beauty and simplicity are rightly regarded as too vague and subjective; and I would also argue that Prof. Yahil's falsificationist approach is unsatisfactory. However, I shall try to discuss precisely these problems in my own paper.

ELKANA: To my mind Ne'eman actually gave beautiful examples to show to what an extent the really interesting dialogues between scientists, the most creative dialogues, are in those parts which (as Toulmin commented) he is inclined to consider as not counting. It is also important from a normative point of view that if Ne'eman's choice of distinction is made between what counts and what does not count—and I accept all the reservations about the choice of the terms "concrete" and "abstract"—then in reality the most interesting, fertile things are going on in the domain of that which does not count. The importance comes out especially in respect to science teaching, which is based automatically on whatever in one's opinion counts. But what counts for Ne'eman is philosophically less interesting, for science teaching is less important and from a normative point of view an unwise choice. It is in the realm of science teaching that the interaction between philosophy and science could be the guidance for the future.

Just one remark on Prof. Rosen's "beauty" argument. I am not against this argument in principle, but as an historical remark, I don't know of a single case where a change occurred and the objection "the new approach is less beautiful than the old one" was not brought up. It is what always happens when suddenly the concrete gives way to the abstract, and then a new generation of scientists comes and turns the abstract again into something concrete—but they add something to it. It is not exactly the same thing as before, but they do again call it beautiful.

SAMBURSKY: About Ne'eman's comments—his idea that there is a certain stage where some abstract model becomes concrete actually has something to do with what he tends to belittle, namely—the role of philosophy. If you have certain data already or certain reasons to accept, say, the idea that three elementary particles form a higher particle, then these things may be strengthened by certain philosophical arguments. They might be arguments from simplicity, or other kinds of metaphysical or epistemological arguments. But in any case, philosophy plays a part in this concretization and therefore I think you were wrong to underrate it. But I do agree that at this stage a lot of things have to do with psychology.

Turning to Yahil's paper, there is one cosmic change we know about, namely the red shift of the galaxies. You can interpret it as expansion; you can interpret it as a change of hot stars. So you might want to take the basic fact of the red shift—a change on a cosmic scale—and relate it to a change of atomic constants, of the gravitational constant.

MENDELSON: I would like to introduce one point and in a way perhaps inadvertently support Prof. Ne'eman and argue against Prof. Toulmin. The point is this: that the ways in which scientists utilize or interact with philosophy are usually sloppy. The philosopher of science comes along at a later stage, and attempts to sort out what is happening, and is able to give form and substance to it. Now one of the reasons that I think this may be missed in the context of a discussion like ours is that certainly the uses of philosophy differ from field to field. This is what happens when you move away from a highly theoretical field, where you might expect a high standard of philosophical argument, to an observational field or discipline. Here, we know—and I am talking from my own background in the biological sciences (and my own paper will in-

volve this)—here we know for example that the philosophical baggage, despite having all the worst forms that Toulmin would point at, is enormously important in the way in which a scientist interacts with the things he is observing or with which he is experimenting.

TOULMIN: Everett Mendelsohn's statement is surely anachronistic. This distinction between "science" and "philosophy," as he is stating it, is rather a distinction between *scientists* and *philosophers*, but this has an application only to the twentieth century. If one talks about the role of philosophical considerations in theoretical physics *à propos* of any other century—well, Helmholtz was a philosopher as well as a physiologist.

MENDELSON: But at times a very sloppy one.

TOULMIN: At times a very good one too. But who were the professional philosophers then? Unless you care to call philosophers only those who were too uninformed to take part in the scientific debate.

MENDELSON: What you tell me now is that anyone who deals with problems of explaining and understanding natural phenomena is a philosopher.

TOULMIN: Indeed not. What I said was that Descartes was, by our standards, as much a physicist as a philosopher, just as Newton, just as Kant.

MENDELSON: Come back to the ones who did not turn out to be good philosophers—turn to the 17th century or 19th century figures who we look back on and say they were philosophical messes, will you tell me that they are also just as much philosophers as scientists? What you have done is choose the men who were good philosophers of Nature and you have said—yes they did science too and you can't separate the two activities. I agree with you that you can't separate them—what I am saying is that you can't only select as your philosophers of Nature those who turned out to have an enormous impact and be very successful.

JAMMER: When we were listening to Dr. Yahil I thought about a methodological question. How many of the constants mentioned have actually been measured directly? As far as I know, disregarding Millikan's famous oil drop experiment, only G can be measured straightforwardly whereas all other constants are interrelated somehow—or at least most of them are. That means that they suppose a certain theory in advance.

This brings me to a more philosophical remark. I just wonder whether

the idea has ever been elaborated of taking into consideration a possible fluctuational change in the very structure of the mathematical laws underlying those measurements, or most of them, of these constants?

NE'EMAN: I would like first to answer the protests we have heard from Professors Toulmin and Lakatos. It is true that seen from the viewpoint of the creative scientists, psychology and philosophy may somewhat overlap here. We try to devise "good" theories. We are thus in need of criteria of "goodness." Sometimes, of course, these criteria, whether right or wrong, dominate our thinking and become important psychologically. However, the issue itself is a philosophical one, because it deals with one aspect of "what counts" in a physical theory.

My distinction between "concrete" and "abstract" touches upon one of the crucial questions of the philosophy of science. I may have picked a bad adjective; however, take for instance the distinction between the "ideals of natural order" of Aristotle versus Newton, which is made in a beautiful book by Prof. Toulmin himself (*his Foresight and Understanding*). Put "model" for "ideals of natural order." Aristotle picked a "concrete" one, a "good physical picture" of a horse and cart, moving against a large resistance; Newton moved away to an "abstract" model which was very removed from his kind of everyday experience. Newton's was the more appropriate one. My thesis in all the examples I picked is just that: it is generally wrong to think that the more "physical" model or ideal is the more appropriate. This is not a sufficient or a necessary prescription for inventing good models, but it does away with one condition which is indeed commonly considered as necessary.

Now I have some direct remarks in connection with Dr. Yahil's lecture. There is a new result with respect to the velocity of light—it is constant and does not depend on the frequencies. This deals with the conjecture that maybe radio has a different velocity than light etc. There is an experiment which gives a very good limit—this is the discovery of the pulsars and especially the one in the Crab nebula. You see, the pulsar in the Crab nebula has been observed in the optical wavelengths and recently in X-rays and very, very recently even in hard X-rays. So you have pulses arriving in all wave lengths from the radio all the way to hard X-rays, some 10 orders of magnitude, and the pulse is entirely synchronous! It arrives at the same moment within about 10^{-7} of a second over 10 orders of magnitude in the frequency. Considering that the signals

travel some 6000 light years from the Crab, I think that sets a good limit on that part of c which would have depended on frequencies. This is a recent observation.

Dr. Yahil was complaining about the fact that we are not really trying to connect these constants together. I can only say that some of the best minds have been at work on this—Heisenberg has been trying to do it for the last 15 years, though I think the results are not impressive. I can say that we are all trying somehow.

There is a new constant which has not been mentioned here, which turns out to be an extremely important constant of nature. This is the Cabibbo angle, an angle of 15° which appears in the connection between the weak nuclear force and the strong nuclear force, and in the last three years there have been a variety of ideas to try and calculate this angle from fundamentals. You have to put in some idea—there was an idea of compensating electro-magnetic effects against weak effects. I can tell you that Gell-Mann and I spent the whole spring of 1969 trying to calculate this angle and we failed. So one is at least trying. Maybe the roots simply go deeper. It may be presumptuous to try and calculate some of these things at the present level. There is so much structure—not perhaps inexhaustible, but there might be a lot of structure underneath.

To return to the queries of Lakatos and Toulmin, I have been asked to assign my "lesson" to a well defined discipline. Was I dealing with the psychology of research? Or is it the influence of a philosophy which I was trying to trace? I think there is something of both. On the one hand, I have pointed out the irrelevance of the "good physical picture," in the context of contemporary scientific ideas, as a criterion of what will become "correct" science in the retrospective view. On the other hand, we have touched upon the psychology of creation, since this "good physical picture" may often be a guide to creative intuition. I think this has been a study of one aspect of what makes a scientific contribution "useful," or perhaps even more *what doesn't make it so*. That a "concrete" picture is not a necessary condition for a contribution to stay and become part of the edifice of physics. This may be obvious and well-known to the philosopher of science; I think I have made it clear that the scientists themselves are less aware of this point.

There is another issue, which I did not intend to bring up in the paper, but which may have attracted your attention through the choice

of my examples, which it permeates. When entering a new domain of experience, science has to go through a stage in which *phenomenology is the basis of a search for regularities*. After these regularities have been established, it moves to the explanation of these regularities by a basic mechanism.

Now, all my examples—and many others I can quote—relate to attempts to jump at a basic mechanism before the regularities have been established. This is the Sakata theme, this is Chew's way; it happened in the Mach-Einstein sequence. My theme is that when that happens, the "basic mechanism" is generally unscientific and *can only serve as a psychological trigger for the finding of regularities*. Take Prout's hypothesis in Chemistry: it was entirely worthless when made. If chlorine had had by chance $A = 35$ instead of 35.5, and the chemists had taken Prout seriously, it would have been the end of chemistry. If atoms are all made of hydrogen, they can be transmuted, and no experiment could have been regarded as the proof of the existence of a new element. One would never have reached the Mendeleev stage—and thus there would be no way to get to Rutherford and the supposed vindication of Prout's idea. Too vague an idea for a "basic mechanism" is useless, and sometimes dangerous.

Take the development of nuclear structure. In the thirties and forties, many workers sensed that the understanding of nuclear forces was so poor that one had to be patient and try to abstract a pattern from the regularities. However this was considered wrong and one had to wait for Jensen and Maria Goppert-Mayer in the late forties and early fifties. They threw away all "basic mechanisms" and discovered the shell-model. Now one can try and explain this pattern.

There is a certain similarity between the two theses: the "basic mechanism" can appear as one of these "good physical pictures." It may play at the general level of a discipline a role similar to that of the latter with an individual scientist's work. This is why I sometimes digress to discuss phases in the methodology of science—a subject which I have touched in some earlier publications, (see for example my "Hadron Matrix Mechanics" at the 3rd Coral Gables Conference on Symmetry Principles at High Energy, 1966) and which I hope to discuss at length elsewhere.

YAHIL: I have several comments to make. First of all I should say I

am not a philosopher and I have really never gone into philosophy. But I can summarize my approach to science as usually an approach of elimination, namely, any theory is good for me as long as it has not been refuted. Therefore if you come up with a nice idea, my approach is to ask what limitations can I put on it. These can come from either observational data or else from another theory that I have. If I find contradictory observational data then that shoots the idea down immediately. If I find contradicting theories then I have to make a decision between the two theories.

Now this is a very simple-minded approach and I have not yet found my difficulties with it. It is true that I have my own private prejudices—if you wish, call it metaphysics—in the sense that I have my own intuitive picture of what goes on. But I consider this my private affair, or at the most something that I can communicate privately with somebody else—but it would not be an argument that I would try to use to substantiate anything.

To come to details—Prof. Sambursky mentioned the specific example of the red shifts. We have red shifts coming from nebulae, and he says this clearly indicates that something is changing on the cosmic scale. That is fine—that certainly would be for me a stimulus to investigation. I would try to see what my intuition tells me about how to handle it. Now, there are several suggestions that have been made. One of them is Hubble's suggestion of an expanding universe. Another is the one which Prof. Sambursky has made, and there are several others. There have been all sorts of cosmological models. In trying to test them the first thing that I would ask is, how can I limit the theory? Not how can I substantiate it, but how can I limit it—what can I say against it? Only when I find that I have nothing to say against it, do I start to seek additional confirmation. Then I ask, what will it predict that I have not tested yet and that I should go on and test?

It has been suggested that the atomic distances are contracting by a change of h . Now this suggestion has been answered experimentally to about 10%, and the answer is—no. Even for light sources that are at a z of about 0.2 or 0.3, we can say definitely that h is not different from our own h to within 10% and that represents an age that is something like a tenth or a fifth of the age of the universe. So that we certainly don't have large scale variations of h and I think that if we really set

ourselves to make a definite experimental confirmation, we could lower it down to 1%.

I would now like to answer the criticism that was made by Prof. Jammer. He said, "how do you assume that it is the constant that is varying and not the physical laws?" A constant in physics is just as much a theory as a physical law and when you open Pandora's Box and you say, something is wrong, then it is very difficult to say it unless you can say *how* things go wrong. You can come and say that e has always been the same e that we measure today, but that the laws of electrodynamics were different in some way or another back in history. In that case you have to tell me in what way they might have changed. To me the most simple-minded thing to do is to go the other way around—assume that the law was the same back in history and suppose that certain constants, whose numerical value I *don't understand in the first place*, were the things that changed. And it is to this question that I try to address myself. If I change too many things at the same time, I will not be able to form any experimental test. All my theoretical framework, in which I can try to begin to think where to test, will have gone.

He also asked, are there numbers that we know directly and numbers that we don't know directly? Here he suggested that the Millikan experiment measures a number "directly," and perhaps little else does. Yet in order to know the electric charge by this supposedly direct method we have to have the electric charge on a small droplet of oil hanging in air and we have to have an electric field that works against gravity—we have to take into account viscosity, etc., etc. So we really have the fundamental laws. (We certainly have the laws of electricity. It is not true that we have here a test independent of these.) On the other hand, we can apply the same argument to a theory which has not been so well established, namely the theory of weak interactions. The weak interaction constant is measured "directly" in Prof. Jammer's sense by measuring the lifetime of the muon. Of course we only have a crude theory here, telling us how the muon decays into electron and neutrino pairs. Yet this does not affect the "directness" of the measurement, the only difference being that the theory of weak interactions is a new theory, and we have not yet accepted it as a God-given theory.

Finally I would like also to say something about Prof. Rosen's comment. He suggested that perhaps some of the numbers that we don't

understand today will turn out to be constants of integration. What he is saying is that they will be numbers that are just as arbitrary, but they will come from a different source. We will have an equation and that equation will have solutions. Since every differential equation has constants of integration, these constants of integration, of which we say as it were that God gave them to us, are the really arbitrary things. He (I mean Rosen) is however clearly substituting one arbitrary number for another arbitrary number. Whether I will like one approach better than the other I don't know, I would have to see his theory.

And finally, one kind of objection which is raised is of the opposite type of view—to say, well there is no experimental evidence or anything to suggest to us that things vary. I mean whoever saw any serious experimental suggestion that the electric charge was not constant? Whoever saw any experimental finding that would tend to suggest that the gravitational constant was not constant? Is there anything to tell us say that the speed of light is not constant, and that if it is not this will solve something that we don't know yet? The answer is "no." So why test it? Well, I don't think that the type of an approach which says that we don't have to test our hypotheses until we encounter difficulty is a fruitful one. Basically, my point of view is to try to shoot down any theory you can.

PHILOSOPHY AND THE ENTERPRISE OF SCIENCE
IN THE LATER MIDDLE AGES

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As you are all aware, the charge put to us when we were so kindly invited to speak here was to treat of some aspect of the interaction between or, more broadly put, of the relations of, philosophy and science. Setting aside the convenient fact that such a task is flexible enough in its restrictions to allow of an almost infinite variety in personal taste and idiosyncrasy, the relevance of these instructions to one who, like myself, looks primarily to pre-Galilean endeavors within the history of science is at least two-fold.

To begin with, relative to this particular place and this particular time, to speak historically of the relations of philosophy and science is singular in its appropriateness. For what has Shmuel Sambursky done in his work on science in antiquity if not treat precisely of just these relations? He has given us the privilege of witnessing not merely, as one of his most enthusiastic reviewers has said, a physicist looking at Greek science, but a physicist whose historical vision—to say nothing of its provocativeness—has always been extraordinarily sensitive to the significance of the philosophical in the scientific, to—as he has put it himself—“scientific thought, aiming at the formation of comprehensive theories and the philosophical foundation of a scientific world-picture.”¹ Furthermore, for me it is especially important that Shmuel Sambursky has exercised this vision most fully with respect to ancient science; for it makes it all the more fitting for me to pay tribute to him from the confines of my own bailiwick and to speak of the ties of philosophy and science within the era—if somewhat later, still firmly pre-modern—of medieval science.

The second reflection that issued, immediately if not simultaneously, from the charge of addressing myself to the relations of philosophy and science was that, if I were to set aside pure mathematics, formal logic, technical astronomy, the more practical side of medicine, and natural history (and I shall be doing so consistently in what follows), then with respect to my particular segment of the historical time scale, the relations I would be speaking of would almost always be ones of identity. Of course, everyone realizes that in the Middle Ages science *was* basically the same

¹S. Sambursky, *The Physical World of Late Antiquity*, London, 1962, p. ix.

as philosophy (the medievals realized this themselves to the extent that it would hardly have occurred to them to make any distinction in the first place). Now, although this much is willingly admitted, it is also continually set aside, if not forgotten. For it seems to me that most historians (including myself) have not sufficiently appreciated the significance of the medieval "science equals philosophy" (or, if one wishes more precision, "science equals natural philosophy"). This point is, in fact, the overall thesis of what I wish to urge upon you today. Put in other terms, my thesis is that inadequate attention to the effective identity of philosophy and science has at least had the disadvantage of yielding an incomplete picture of the enterprise of science in the later Middle Ages.

In attempting to persuade you, however partially, of the truth of this thesis, what I shall present is, at least in intent, designed to accomplish two things: (1) to give something of a more complete representation of what I see as being most characteristic of the "new" science in the fourteenth century, and (2) to set forth several suggested guidelines and caveats for a program for reexamination and new work in the science of this century. The latter will be urged by implication throughout much of what I shall say, although I shall in concluding attempt to bring some of this to a more explicit level through a series of specific historiographic remarks.

I have just said that my text is "designed to accomplish" these two things; perhaps, charitably viewed, it will. I should admit, however, that as I view it myself, it only begins to do so, since that of which I shall speak is very much part of work in progress. At the same time, I do not think it is in any sense fancy, for though I do not intend to flood you with examples and textual citations, a good deal of such are behind what I shall have to say and I believe I know where to go for literally mounds of further documentation. What I mean, rather, by "work in progress" is basically that I shall be presenting some of the results I have achieved in the continuing process of putting myself the rather thorny question of: What was it like to do science at Oxford, or at Paris, in the mid-fourteenth century? In spite of the fact that the very nature of this question will always make a final and definitive reply impossible, it is still one that is so important in determining the character of late medieval science that historians must, I think, never tire of approximating a likely answer.

FROM THE MEDIEVAL TO THE SCIENTIFIC REVOLUTION

Let me begin the substance of what might be considered my own first approximation toward an answer with an historiographic consideration. To see how medieval science has been, and is being, written will assist us in the appreciation of why the equation of philosophy and science in this period should always be kept well in mind. Pierre Duhem would be, of course, the first figure to examine in detail. But time legislates against the luxury of unravelling the rather fascinating story of how his philosophical (to say nothing of religious and nationalistic) predisposition insinuated itself into his interpretation of the key moves in late medieval physics. His case is, moreover, ironic as well as instructive since, in spite of the fact that he ends up evaluating medieval science from the vantage point of the seventeenth century and classical mechanics, he discusses—without truly appreciating—more medieval philosophy in his work than have almost all other historians of medieval science.² For present purposes, however, suffice it to say that Duhem was firmly convinced that, arising from the twin actions of the Church's condemnation in 1277 of parts of Aristotelianism and the consequent exercise of *science expérimentale*, fourteenth-century mechanics had within it, *en ce qu'ils ont d'essentiel*, the basic principles of the mechanics of Galileo and Descartes. The very heart of this fourteenth-century preparation was to be found, Duhem held, in the *positivism chrétien* in vogue at Paris, and in particular, as an example of this, in the *méthode inductive* of Jean Buridan.³

It does not, I think, seriously detract from the extremely great value Duhem's work in medieval science holds, to say that his particular thesis of how one gets from the fourteenth century to Galileo is no longer accepted. Why then, if his favorite symptoms of precursoritis are now rejected, need one mention his work at all? It is because that, even though the specific claims he made for medieval-modern continuity have been

²A most notable exception is Anneliese Maier, whose five volumes of *Studien zur Naturphilosophie der Spätscholastik* Rome, 1949 ff., are extremely valuable in forming a comprehensive picture of the late medieval scientific enterprise as well as in elucidating particular points and doctrines.

³This thesis appears throughout Duhem's work on medieval science, but rather explicit, and at the same time more general, statements of it can be found in his *Système du monde*, Paris, vol. 4 (1916), pp. 313–316; vol. 6 (1954), pp. 715–717, 729; and most concisely in his letter in *Rendiconti della R. Accad. dei Lincei* (Classe sci. fis. mat.) 22 (1913), 429–431.

muted almost to the point of non-existence, one still often views what is at the center of fourteenth-century mechanics as some kind of function or other of some of the things one finds in Galileo. One does not say that such is all there is to fourteenth-century mechanics, but the implication is that it is what is most important within it. And from the standpoint of Galileo it probably is.

Let me take the book on medieval mechanics as an instance of what I have in mind: Marshall Clagett's *The Science of Mechanics in the Middle Ages*. Clagett's view of the problem of medieval-modern continuity is that developments in the former "conditioned" concepts in the latter in many ways, and especially that "medieval mechanics . . . was continually modified to the point where it was seriously undermined, thus requiring a new mechanical system—and it was the Galilean-Newtonian system of the seventeenth century that fulfilled that requirement." All of this, we are most appropriately cautioned, occurred "within the basic framework of Aristotle's natural philosophy," but that, even so, the medieval treatment of certain critical problems contained elements "that were to prove useful in early modern times when the Aristotelian framework was abandoned."⁴ Now, I do not in any sense wish to deny that there were medieval notions and techniques that were used by Galileo and others. In fact I do not really wish to gainsay any of Clagett's conclusions; rather, I wish to add to them, and at the same time claim that what I think needs adding is in many ways that which is most characteristic of fourteenth-century mechanics, indeed of that which is new in fourteenth-century natural philosophy as a whole.

In order to begin spelling out what I have in mind, let me return for a moment to the scholastic modification and undermining of medieval—largely Aristotelian—mechanics that has been noted by Clagett.⁵ The first point I should like to make is that if one assumes that there did exist in the later Middle Ages a well-knit, well-defined body of knowledge that to a large extent paralleled at least part of the mechanics of the seventeenth century, then one can maintain that this medieval me-

⁴Marshall Clagett, *The Science of Mechanics in the Middle Ages*, Madison, 1959, pp. xix, xxix.

⁵Here, and in what follows, I am speaking of what Clagett calls medieval kinematics and dynamics as treated in Parts II and III of his book. I am not, and shall not, be referring to Part I: medieval statics. The latter I feel is quite separate from the subjects discussed in Parts II and III and, unlike them, does not form an intrinsic part of medieval natural philosophy. But I do not wish to argue this "separability" of medieval statics here.

chanics came to be undermined seriously enough to require a new mechanical system. Perhaps one could claim that Galileo himself viewed the situation in some such way. But although this may be true viewed from the modern end of things, within the confines of the fourteenth century itself, I think one can doubt the existence of such a *well-defined* body of knowledge called, by its resemblance with a later discipline, mechanics.

This is surely so with respect to Aristotle's works themselves, but it is also the case, I would argue, in those late medieval writings that re-worked, developed and modified these works. It is largely by re-arrangement and selection that we are able to discern *within* these scholastic writings what might, once so selected and so discerned, be called medieval mechanics in the usual sense of this term. What I should like to suggest then is that however much undermining and criticism may have been meted out in the later Middle Ages, what remained was still basically Aristotelian,⁶ and secondly, and of greater import, that to view what is said about "mechanics," or better about *motus*, in the fourteenth century with one eye fixed upon early modern science is apt to mislead. At minimum, such a view runs the risk of revealing less than the whole story of that at issue; it runs this risk because even if but one of the vanishing points of our perspective is anchored in the seventeenth century, this can urge the application of categories in the interpretation of fourteenth-century material which, if not entirely foreign to this material, are at least not those most naturally fitting to it. Thus, one can look upon the "new" late medieval *scientia de motu* as something other than a body of knowledge *essentially* concerned with (to cite several familiar examples) instantaneous velocities, the kinematics of uniformly difform motion, and the problem of the continuation of projectile motion or of the acceleration of heavy bodies in free-fall.⁷

⁶As is apparent from the above quotation, Clagett makes much the same point himself. Duhem, however, viewed the physics of the fourteenth century as so different from that of thirteenth century Aristotelianism that he would probably have denied, or at best only grudgingly admitted, this.

⁷Of course, Clagett's book contains a great deal more than a careful analysis and documentation of merely these factors. But it is significant to note that at the conclusion of the volume (pp. 676–682), in a tabulation of the "principal medieval mechanical concepts," the major portion (statics again being set aside) deal with aspects of just these issues and conceptions. Perhaps of greater importance for the dissemination of the historiography of medieval science is that in more general histories of science or physics, the distillation of medieval material usually results in an end product bearing on these issues and little more.

If one keeps in mind the fact that the science in question was a part of natural philosophy, then one would not, to begin with, separate (as Duhem did) what given sources have to say about local motion from what they maintain with respect to motions of alteration and augmentation.⁸ More specifically to take but an example—one might also refrain from isolating the scholastic treatments of projectile motion and acceleration in free-fall. For one can also consider the fact that these two problems (together with the theories of impetus or other notions used to resolve them) are but parts of a more general controversy, a controversy having to do with the possible relations of mover to mobile, with the problem of what is often regarded as the essential form of a mobile and with the notion or theory of *gravitas* as a whole.⁹ Nor is this merely a question of an alternative point of view without a difference. For, seen in this broader context, one has a more immediate and natural explanation of the medieval concern with other problems that formed a single cluster (especially in terms of the issues raised and conceptions employed) with the two questions of projectile motion and acceleration in natural motion.¹⁰

Similarly, medieval investigations of the relations between *potentia*, *resistentia* and *velocitas* (or *tarditas*) may receive less than their proper due if we translate these terms by their counterpart *F*, *R* and *V* within later mechanics. We should perhaps expand our dictionary and, in comparing *potentiae* and *resistentiae*, take *F* (as one medieval instructs us)¹¹ as "an active potency, where all senses of *posse agere* are taken into ac-

⁸Pierre Duhem, *Système du monde*, Paris, vol. 7 (1956), pp. 477, 531.

⁹If one discounts a good part of the interpretation, chapters 10 and 11 of volume 8 of Duhem's *Système du monde* gives some idea of the nature and magnitude of this more general controversy. See also Anneliese Maier, *An der Grenze von Scholastik : Naturwissenschaft (=Studien zur Naturphilosophie der Spätscholastik*, vol. 3), 2nd edition, Rome, 1952, pp. 143-254.

¹⁰In addition to the more general questions mentioned above, I have in mind such added particular problems as the possibility of a *quies media* in reflexive local motion and the imagined medial acceleration of projectiles. These employed conceptions in their revolution which were similar, if not identical, to those utilized in puzzling out the continuation of projectile motion and acceleration in free-fall. That the issues of a *quies media* and medial acceleration are initially of less interest to us is probably due to the fact that their formative influence upon seventeenth century mechanics was next to nil. This should not, however, obscure their importance in the fourteenth century.

¹¹The author is Marsilius of Inghen, cited by Anneliese Maier, *An der Grenze von Scholastik und Naturwissenschaft*, 2nd edition, Rome, 1952, p. 186.

count but come together and assist in acting, be they extensive, intensive, applicative, having to do with the density of matter or with the union of strength," and take *R* with a similar flexibility. And then, having done this, one might ask what difference this would make to what might be assessed as a part of medieval notions of dynamics.¹²

Alternatively, to cite one final example, the utilization of the modern pair of kinematics and dynamics to interpret the medieval distinction of treating *motus quoad effectum* and *quoad causam* might be questioned when one realizes that the distinction is applied more frequently to cases of motion of alteration than to local motion. For while the causes (the forces and resistances) in local motion are different in kind from the effects (space per time), in alterative motion this is not so (heat causes heat, cold cold).¹³

In suggesting that we should adopt a less modern, more flexible, orientation in formulating the categories and concepts we might wish to apply to medieval texts dealing with the problems of motion, I do not wish to imply that, without this, the previous history of these problems has been wrong-headed or without results of considerable value. On the contrary. For I do not wish to deny that the singling out of the particular notions and issues I have selected above for examination may well represent the response Galileo (and others in the early modern phase of mechanics) may have had to this material. It is important, of course, properly to delineate this response, whatever its extent or significance for seventeenth mechanics as a whole. However, I do wish to enter a plea to the effect that, for a reassessment of the medieval treatment of motion *qua* medieval, we should look again at the categories and assumptions we may bring to bear in our analysis.

THE FOURTEENTH CENTURY MEDIEVAL "REVOLUTION"

In any event, the possible correctives I have thus far suggested in

¹²This broader view might be helpful in explaining the inclusion of *calculations* dealing with *potentiae*, *resistentiae* and *velocitates* in late medieval *Sentence Commentaries* and even the attempt to fit such with the *omnipotentia Dei*. Cf. Anneliese Maier, *op. cit.* (above note 11), p. 261.

¹³I owe this observation to Edith Sylla, who has made the point in her unpublished doctoral dissertation (Harvard University, 1970); *The Oxford Calculators and the Mathematics of Motion, 1320-1350: Physics and Measurement by Latitudes*. This thesis in many ways embodies, incidentally, the type of approach to late medieval material that I am presently urging.

order to gain an adjusted vision of late medieval science are not nearly so significant, I believe, as the second major point I should like to make. This is that there is a late scholastic modification and undermining of *scientia de motu* which does substantially alter much in the character of medieval Aristotelian natural philosophy. Yet this change is *not* one basically, if unknowingly, directed toward Galileo or toward any other specific feature or figure of early modern mechanics. As a whole, what it leads to is rather more of the same in succeeding centuries and, on the negative side, to a variety of Humanist complaints. Restating my overall thesis, to understand this change is to understand that which is most characteristic of the new developments in late medieval natural philosophy and, at the same time, to see the relations of the new *scientia de motu* with philosophy (and even with some theology) as a whole. What is more, in order properly to ferret out the elements constituting this change, and in order to see them as bearers of the essential character of the enterprise of late medieval science, it is well that we disabuse ourselves, at least temporarily, of our preoccupations with the scientific revolution and, for that matter, with science itself.

This done, the initial factor one ought to consider in the change of which I am speaking is what I shall choose to characterize, in the interest of being both brief and comprehensive, as the development and embrace of new conceptual languages with which to treat the traditional problems of natural philosophy on the one hand, and with which to invent and solve new problems on the other. (And I speak of natural philosophy, even philosophy, as a whole and not just *de motu*.) Again in the interest of brevity, let me merely tabulate the most widely used of such "languages."

The first two are the most familiar, made known (if not exactly popular) by their inclusion in histories of science through their application in the so-called Mean Speed Theorem and in Thomas Bradwardine's "Dynamical Rule." I mean, of course, in the first instance the language of the *intension and remission of forms* and of the concomitant degrees and latitudes, be they uniform, uniformly difform, difformly difform, or uniformly difformly difform, no matter. This language is found literally everywhere within fourteenth century thought.¹⁴ Less universal, but

¹⁴Even the secondary literature is substantial. But a sufficient notion of what is involved can be gleaned from the following (and the additional literature men-

equally important, is the language or calculus of *proportiones*, most familiar to be sure from Bradwardine's application of it to the variables of local motion, but also used in all other manner of instances when one sought to compare the arithmetic increase in one set of things with the geometric increase in another.¹⁵

If these two new languages are mathematical, or at least quasi-mathematical, in nature, others were fundamentally logical. Thus, most extensive of all in its application was the *theory of supposition* of the medieval *logica moderna*. For not only did it furnish the scholastic philosopher with a theory of meaning and truth, but it placed in his hands an analytical tool with which to treat almost any problem, be it logical, epistemological, ontological, metaphysical, physical or just narrowly *de motu*.¹⁶ Hard on the heels of this new element in the medieval's conceptual apparatus came the closely related development of William of Ockham's *nominalism* (or better *particularism*). It too afforded a new means by which to analyze problems, traditional or freshly invented ones.¹⁷

On a level less that of a new language than that of a new "prescription for research," the ubiquitous invocation of the *potentia Dei absoluta* functioned as a way to push the examination of questions beyond the confines of the physical possibilities licit within Aristotelian natural phi-

tioned therein): Anneliese Maier, *Zwei Grundprobleme der scholastischen Naturphilosophie*, 3rd edition, Rome, 1968, part I; Marshall Clagett, *Nicole Oresme and the Medieval Geometry of Qualities and Motions*, Madison, 1968; John Murdoch, "Mathesis in philosophiam scholasticam introducta: The Rise and Development of the Application of Mathematics in Fourteenth Century Philosophy and Theology," *Arts libéraux et philosophie au moyen âge, Actes du quatrième congrès international de philosophie médiévale*, Montréal and Paris, 1969, pp. 233-246; André Combes, "L'intensité des formes d'après Jean de Ripa," *Archives d'histoire doctrinale et littéraire du moyen âge*, 37 (1970), 17-147.

¹⁵That is, put in a modern form that does not properly express what is involved, Bradwardine maintained that a change in velocity corresponded to a change in the determining force/resistance ratios amounting to: $(F_1/R_1) v_2/v_1 = F_2/R_2$. On this and its extension by others in the fourteenth century see Clagett, *Science of Mechanics in the Middle Ages*, ch. 7 and Murdoch, *op. cit.* above (note 14), pp. 225-233.

¹⁶Most treatments of the medieval theory of supposition do not press beyond its role in logic itself (see, for example, Ernst Moody, *Truth and Consequence in Mediaeval Logic*, Amsterdam, 1953), but some indication of its importance within natural philosophy can be had from Curtis Wilson, *William Heytesbury: Medieval Logic and the Rise of Mathematical Physics*, Madison, 1956, and J. Murdoch, *op. cit.* above (note 14), p. 220.

¹⁷See the references below in notes 22 and 23.

losophy into the broader field of what was logically permissible. At the same time, a similar extension was granted to consider possibilities beyond the limits of the revealed truths of faith.¹⁸

Finally, although we again do not in any proper sense have to do with a new language, a whole new constellation of concepts are seen operative in a newly found readiness to consider in detail not just the infinite as such, but *infinite values* in general everywhere: infinite weights, infinite forces, infinite durations, and infinite intensities of all sorts.¹⁹

All this, however, gives one but new languages or new conceptual tools. What, we must now ask, was done with them, especially with respect to what may be considered the new physics of the fourteenth century? What was accomplished was as new, if not newer, than the new languages themselves.

Let me eliminate specific consideration of the last two "languages" I have mentioned and, reversing the above order, say something of the more strictly logical side of things first. Here, in a way in a category somewhat by itself, the theory of supposition as ensconced in Ockham's particularism allowed one to rephrase and re-examine almost all of the traditional problems within natural philosophy. This recasting took the form, as it were, of no longer speaking about the things or entities involved in a given problem, but rather now about propositions whose terms stood for (*supponit pro*) such entities. Thus, elucidation of the kind of supposition terms were held to have within a given problematic proposition became a standard way of resolving the problem at issue, particularly so when a question of ordering, or prescribing limits for, continuous sequences was at stake (as was so frequently the case in a natural philosophy so much concerned with process, change and becoming).²⁰

What is more, this logic-dominated approach to questions was un-

¹⁸Although, appeal to the *potentia Dei absoluta* is everywhere present in late medieval philosophy and theology, the precise nature of the role it played still awaits adequate treatment. In the interim, some indication of its importance can be found in Paul Vignaux, *Nominalisme au XIV^e siècle*, Montréal, 1948.

¹⁹See J. Murdoch, *op. cit.* above (note 14), pp. 216-224, 238-246, and note 39 below.

²⁰To phrase an example in modern, rather than medieval, terms, the distinction between a limit as preceding all members of a given ordered series and a limit as preceding all members but immediately preceding no one of them, was expressed in terms of a difference of supposition for the term (*punctum*, *instans*, etc.) functioning as that limit. Cf. the references to Wilson and Murdoch in note 16.

usually congenial to anyone enamored of the Ockhamist view that any science consisted in but a set of propositions.²¹ Indeed, Ockhamism in general, like supposition theory, brought more than purely philosophical advance. For it often solved problems by removing what were hitherto seen as the problematic entities themselves. It did so not merely by the reduction of abstract, connotative terms (such as 'motion' or 'heat') to a function of absolute, concrete terms (such as 'this mobile,' 'this place,' 'this hot body,' etc.),²² but also through the elimination of such "fictions" as represented by the terms 'instant,' 'point,' or 'line,' an elimination most frequently affected by transferring the role such terms normally played to a series of propositions in which the problematic terms no longer occurred.²³ This Ockhamist based analysis of problems was not, of course, universally accepted, since it was bound to those of a particular philosophical persuasion. Still, parts of its approach persist, and find themselves admitted and applied by adherents to other philosophical doctrines as well.²⁴

²¹Of course, these propositions are in turn composed of terms or mental contents or concepts (*intentiones sive conceptiones animae*) which then in turn stand for (or have supposition for) individual corruptible and generable things. This view is most concisely put in the *Prologus* to Ockham's *Exposition of Aristotle's Physics* (as in Philotheus Boehner ed., *Ockham; Philosophical Writings*, Edinburgh, 1957, pp. 2-16, espec. p. 11).

²²It was by this kind of treatment of the term "motion," for example, that Ockham and Ockhamists made their contribution to the investigation of the standard medieval query over the nature of motion. For a brief history of this query see Anneliese Maier, *Die Vorläufer Galileis im 14. Jahrhundert* (= *Studien zur Naturphilosophie der Spätscholastik*, vol. 1), 2nd edition, Rome, 1966, pp. 9-25, 315-324. For Ockham's contribution in more detail see Philotheus Boehner ed., *The Tractatus de successivis Attributed to William Ockham*, St. Bonaventure, N.Y., 1944; and Herman Shapiro, *Motion, Time and Place According to William Ockham*, St. Bonaventure, N.Y., 1957.

²³For example, Ockham explains what is meant by continuous motion without appealing to some problematic phrase such as "does not rest for an instant" (problematic basically because of the introduction of the term 'instant') as follows: *Ad hoc quod aliquis moveatur localiter continue non tantum requiritur quod ubi non tantum coexistat simul cum affirmatione et negatione alterius ubi naturaliter (quia contradicatio est quod ubi iam acquisitum existat simul cum affirmatione et negatione alterius ubi), sed requiritur quod illud ubi non coexistat naturaliter cum aliis contradictoriis sibi continue succedentibus, puta cum istis, 'rex sedet,' 'rex non sedet,' vel cum istis: 'sortes est,' 'sortes non est'; si enim utrumque istorum potest verificari manente eodem ubi naturaliter, non est motus localis eius, sed necessario est quies intercepta* (In *Sent.* II, Q. IX, sign. J ed. Lyon, 1495). He offers a similar analysis of the problematic term 'instant' itself in *Sent.* II, Q. 12.

²⁴Ockhamist strictures concerning the "existence" of such entities as points, lines and planes were often accepted by others who did not follow his lead in other respects; Cf. V. Zoubov, "Jean Buridan et les concepts du point au quatorzième siècle," *Mediaeval and Renaissance Studies*, 5 (1961), 43-95.

When one adds to this the fact that the predilection for logical analysis that is represented by the application of supposition theory was all but universal among fourteenth century thinkers of no matter what stripe, it is evident that the penetration of medieval logic into all corners of natural philosophy was considerable. So considerable in fact that it insinuated itself into the second major field of application of the newly created conceptual languages we are examining.²⁵

This second field is, to us, more mathematical than logical; it is, in a few words, the field of measure. For at some time toward the end of the thirteenth century, or the beginning of the fourteenth, there arose a veritable furor to measure all things possible. Exactly when, or why, this came about is far from clear.²⁶ Yet if we cannot be informed about the origins of this suddenly appearing pervasive interest in measure, we can say a good deal about its nature and content. To begin with, we should note that we have to do with measure in a sense more general than we might take the term today. This is so not merely because the measure is taken of things we would regard as non-measurable, but rather also because, in spite of the fact that mathematical and quantitative factors are almost always at issue, from the medieval point of view the problem of measure is better seen (as Anneliese Maier has cautioned, but few have heeded) as a logical problem of denomination. Thus, the measurement involved in saying that a uniformly difformly hot body is as hot as a body uniformly hot in its mean degree is exactly parallel to the logical problem of denominating a body by (say) the predicate blue.²⁷ To see this is, once again, to realize the fundamental identity of philosophy and science.

Given this qualification, one should next stipulate what kinds of things the late medieval thinker set out to measure. Fundamentally, they were

²⁵In particular, one frequently finds the logical "mechanics" of supposition theory mixed with the mathematical analysis or treatment of a problem.

²⁶The suggestion of Gordon Leff (*Paris and Oxford Universities in the Thirteenth and Fourteenth Centuries*, New York, 1968, p. 244) that it somehow resulted from the rise of Ockhamism is at once too vague and general to be of much help. More profitable, if would seem, might be the pursuit of the suggestion referred to in the following note.

²⁷Anneliese Maier, *An der Grenze von Scholastik und Naturwissenschaft*, 2nd edition, Rome, 1952, pp. 279-280. The suggestion would be strengthened by further documentation and study. The example I have given in explanation is one of my own making, but I think it may express something of the medieval view of the matter.

three: (1) distances, not so frequently spatial ones, as temporal and essential distances (especially when one such distance was infinite when compared to another),²⁸ (2) static continua, where there occurs an ordering of elements over such continua (thus the points in a line, the instants in a time interval, or a quality that is uniformly varied over a given subject); (3) change, in all its forms: local motion, alteration, augmentation and even instantaneous substantial change when considered with respect to its temporal limits.

Such were, in outline, the kinds of things measured. How they were so measured was substantially the same in almost all cases. This was accomplished by the application, simply put, of a basic rule of measure or of a set of such basic rules.²⁹ Rules such as the following: (1) a uniformly difform quality is measured by its mean degree (the Mean Speed Theorem being but a particular instance of such mean degree measure); (2) the speed of a body undergoing uniform local motion is measured by its fastest moving point (rotational motion being the problematic case usually in mind); (3) an arithmetic increase in speed corresponds to a geometric increase in the ratio of its causes (or forces and resistances) which is Bradwardine's rule; (4) a variety of rules concerning the infinity of parts within a continuum—here the problem is how such parts are related the one to the other and what "measure" one can give for the infinity of parts within a part of a continuum relative to the infinity of parts within it as a whole; (5) a number of rules delimiting various kinds of active and passive powers in terms either of the maximum they can accomplish or the minimum they cannot; (6) a similar set of rules specifying the temporal limits of the existence of permanent and successive beings in terms of first and last instants of being or not-being; (7) all manner of suggestions concerning just which *scale* of measure should be utilized to measure, not just intensible and remissible forms,³⁰ but the

²⁸By an "essential distance" is meant the distance between two species, such as *homo* and *equus*, or, if you will, between their essences.

²⁹I have described the techniques involved in applying such basic rules in the article cited above (note 14) pp. 216-38. These same pages also explain what I am here tabulating as rules (1)-(4) in greater detail. For rules (5)-(6) see the book of Curtis Wilson cited above (note 16).

³⁰Richard Swineshead's deliberations over the proper scale for intension and remission are analyzed by Marshall Clagett in "Richard Swineshead and Late Medieval Physics," *Osiris*, 9 (1950), 131-161. Information on scales for measuring the perfections of species can be found in J. Murdoch, *op. cit.* above (note 14), pp. 238-246.

chains of being represented by the series of perfections of radically diverse species (how could one compare, it was asked, a man with an ass, or an angel with a fly).³¹

The formulation of such rules was, however, only the barest beginning of the late medieval furor to measure. Far more effort and ingenuity was spent in applying the rules. It has, for example, often been remarked that the notion of mean degree measure was used not only to treat scientifically respectable things like local motion, heat, light and color, but also to cover less tractable qualities such as love, charity, beauty, etc.³²

But this kind of extension of the basic rules of measure is far less significant in terms of what is characteristic of the "new science" of the fourteenth century than is another aspect of their application in the hands of the scholastic natural philosopher. This was the application, taken as a kind of testing, of any such rule to all conceivable variations of the given quality, power or other entity being measured. And the emphasis here should fall on the "all conceivable," since they invented variations with a vengeance, the more complex and the more outlandish the variation the better. They were operating to the very limits (to use a frequently appearing term) *secundum imaginationem*.³³

At times, the varied instances to be fed to a given rule were multiplied through a simple increase in the number of variables involved:

³¹At this point a rather extensive parenthesis seems appropriate in order to specify just which of the "new conceptual languages" noted above were utilized in the application of these basic rules. Naturally the language of the *intension and remission of forms* is paramount in rules (1) and (7), while that of *proportiones* is at the very heart of rule (3), and *infinite values* are everywhere present in the consideration of rules (4) and (7). Less obviously, the theory of *supposition* plays a large role in rules (5) and (6). But this is only the beginning. For not only does the *theory of supposition* rear its head wherever it pleases (it being, it seems, the most universal of the new languages), but the conceptual apparatus of the *intension and remission of forms* is applied to rule (3) when John Dumbleton "translates" Bradwardine into this language, and to rules (5) and (6) when, respectively, the powers or beings falling under these rules are taken as intensible or remissible ones. Interlacing things even further, the continua forming the subject of rule (4) can naturally appear as that being "measured" under any of the other rules, so the considerations relevant to rule (4) are often found "multiplied" into the factors pertinent to these other rules. In an abstract fashion, all of this begins to indicate the complexity involved in the late scholastic preoccupation with measure.

³²An excellent case of a treatment of the whole spectrum is now available in Marshall Clagett's recent book on Oresme cited above (note 14).

³³A more detailed description of many of the variant cases mentioned below can be found in J. Murdoch, *op. cit.* above (note 14), pp. 224-238.

one could move from considering merely the intensity of a single quality to the consideration of it in both intensity and extension, and then to the intensity of two qualities belonging to a single subject or to the extension and intensity of both these qualities, and so on.³⁴ At other times, the characteristic increase in the complexity of the instance being treated was accomplished by applying the rule to a well-known traditional puzzle. One of the most notable cases of such a ploy was Richard Swineshead's application of Bradwardine's rule relating speeds, forces and resistances to the question of whether or not, were a hole to be drilled through the earth and a long cylindrical body dropped through it, the center of that body or rod would ever reach the center of the earth. This was problematic since as any leading part of the rod passed the earth's center, that part would then function as a resistance to the rod's continued motion. (Swineshead proved, incidentally, that if we take it that the rod acts as a sum of its parts and accept Bradwardine's rule, then it will never reach its assigned goal.)³⁵

But these are rather special examples of how the most creative scholastics multiplied the grist for milling by their rules. More frequently the procedure was to hold constant the set of variables the rule was intended to measure, but then to strain every imaginative resource to set forth every possible kind of variations these variables could suffer. Thus, to cite Richard Swineshead once again, if one assumes with Bradwardine that speeds vary arithmetically while the corresponding ratios of force to resistance vary geometrically, then what can one say—or literally what further "sub-rules" can be developed—with respect to the different *kinds* of change in *F/R* relative to *V*? What happens, for example, if we hold *F* or *R* constant, but vary the other; or if we take pairs of *F/R* ratios, hold (say) *R* constant for each, but vary both *F*'s, one more than the other; alternatively, we can ask what follows if the changing *F* or *R* varies uniformly over time, or even interpret a variant *R* by the penetration of a uniformly difform medium by a constant *F*, once we are given a way to correlate this difformity in space with a change of resistance

³⁴It is just such a procedure, for example, that marks the differentiating points of the first four *Tractatus* of Richard Swineshead's *Liber calculationum*.

³⁵This particular text of Swineshead has been published, and analyzed, by M. A. Hoskin and A. G. Molland: "Swineshead on Falling Bodies: An Example of Fourteenth-Century Physics," *British Journal for the History of Science*, 3 (1966), 150-182. See also the appendix to Murdoch, *op. cit.* (above, note 14) pp. 250-254.

over time.³⁶ It is in this way that Swineshead establishes some 50-odd cases or sub-rules beginning from the basic equation of measure furnished by Bradwardine.³⁷ In excogitating this series of sub-rules he has, as it were, tested the mettle of the initial, basic rule, and, as a result, learned more of its effectiveness as a fundamental proposition of measure.

The importance of this cannot be exaggerated. For, whether this procedure be regarded as rule testing, as the implication of a set of corollary-rules, or merely as the multiplication of variant cases, the significant fact is that it is just this kind of activity that characterizes the major share of the sum total of literature constituting the "new" physics of the fourteenth century. I have used Swineshead's expansion of Bradwardine's measure for V 's, F 's and R 's as an example, but the same sort of extension is everywhere to be found practiced on every other basic rule of measure I have mentioned.³⁸ What is more, not only is this type of activity a major preoccupation of a great deal of late medieval nat-

³⁶The key to Swineshead's development of Bradwardine lay in his realization of how one can measure force changes when the resistance is held constant, or resistance changes when the force is held constant. To speak to only the first of these two cases, Swineshead stipulates that, if (say) F_1 acts on R and then increases to F_2 , and again acts on R , then we can relate the two force/resistance ratios in the following way (put in modern terms), $F_2/R = F_2/F_1 \cdot F_1/R$. (Indeed, this is the whole point of the first of the numerous rules he establishes in *Tractatus XIV* of his *Liber calculationum*: *Ubi cunque aliqua potentia crescit respectu resistentie non variae, tantam proportionem acquirere respectu illius resistentie per quantam ipsa fiet maior; vel sub aliis verbis eadem sententia remanente, tantam proportionem acquirere respectu sui ipsius quantum respectu sue resistentie* [ed. Venice, 1520, fol. 43va].) Now given that the one ratio F_1/R gives rise to a velocity (call it) V_1 and F_2/R gives rise to V_2 : this relation of the two ratios means that any difference in these velocities must be accounted for by the F_2/F_1 ratio. This is the key to all Swineshead accomplished. For, taking the above one step further, if we have another pair of F/R ratios related as $F_4/R = F_4/F_3 \cdot F_3/R$, then we can automatically see that the velocity difference determined by the second pair of ratios is either greater than, equal to, or less than the velocity difference determined by the first pair correspondingly as $F_4/F_3 \geq F_2/F_1$. With similar results established for resistance changes against a constant force, Swineshead had all he needed (save for some basic mathematics for comparing ratios) to do what he wished. As will be seen from the above, his moves are based solidly on the Bradwardinian base (as he himself admits) of an arithmetic change in V corresponding to a geometric change in F/R .

³⁷These rules, some of which I have described in the previous sentence, constitute *Tractatus XIV*, entitled *De motu locali*, of Swineshead's *Liber calculationum*.

³⁸This is not the place to document further examples, and reference to other literature is problematic since many of the relevant texts are unpublished and unstudied. Still, some information can be had from the following: For further de-

ural philosophy, but it is "newer," I feel, than such contemporaneous developments as impetus theory, newer because it marks a more radical break with the Aristotelian tradition in the way it approaches and treats problems.

If one grants, then, that this rule testing activity was one of the salient features of much of what fourteenth-century science was about, it would be well to probe further into its operations. The most evident fact is that mathematics was being applied, often so much so that the results were, if anything, *more* mathematical than much of what one finds in later, seventeenth century mechanics. In most cases, the mathematics involved belabors the obvious. In instances like Swineshead it does not. But a word of caution here. Whenever as with Swineshead, translation into modern terms of the mathematics involved inspires a sense of awe at the marvels accomplished, it is almost always the case that the translation has it wrong or misleads, and that there is a much simpler—albeit incredibly ingenious—way the results were reached.³⁹

developments of Bradwardine's function: Edward Grant ed., *Nicole Oresme. De proportionibus proportionum and Ad pauca rescripta*, Madison, 1966; for the extension of mean degree measure: Clagett's edition of Oresme's *De configurationibus* cited above (note 14) and Murdoch, *op. cit.* above (note 14), pp. 235–238 (see also note 39 below); on *maxima quod sic* and *minima quod non* and *de primo et ultimo instanti*: Wilson, *op. cit.* above (note 16); on the "fastest moving point" rule (2): Murdoch, *op. cit.*, above (note 14), pp. 233–234.

³⁹Evidence of mathematical ingenuity can be found scattered throughout much of the literature mentioned in the foregoing notes. But the dangers of a modern translation of much of the mathematics of measure might merit citing a case in point. Again I shall turn to Swineshead, *Tractatus II (De Diformibus)* of his *Liber calculationum*. He imagines that a given subject is hot in degree 1 over its first half, in degree 2 over its next quarter, in degree 3 over its next eighth, in degree 4 over its next sixteenth, and so on *in infinitum*. As a whole, he correctly concludes that this subject is (by an altered postulate of mean degree measure) hot in degree 2. Very well, in our terms he has correctly seen that the infinite series in question is convergent. (The relevant text has been edited and translated by Clagett in his book on Oresme cited above (note 14), pp. 59–61.) But Swineshead's real mathematical coup comes next. Take this same subject, do not alter the heat in any of its parts, but assume that every, and only every, 2ⁿth proportional part of it rarifies, but in such a way that each such part rarifies twice as slowly as its predecessor. Now, how hot is it? Answer: infinitely hot. (But note the paradoxical feature of it all: by no matter how small an amount, or how slowly, one rarifies the first 2ⁿth proportional part, the denomination of the whole jumps immediately from a mere 2 to the infinite; a feature most suited to the voracious appetite of the late scholastic for sophisms and incredible variant cases.) But to return. In our terms the corresponding infinite series is divergent. The general term of the series of rarefied parts is $(2^{2n-1} + 2^n)/2^{2n}$ and from this we can easily see the resulting divergence. This in the fourteenth century? Marvel of marvels! But no, Swineshead must not be looked to as some kind of fourteenth-cen-

On the other hand, the rule testing activity we have been examining can be viewed from a standpoint other than the mathematics it involved. For, whenever a new variant case is treated under a rule or a further supplementary rule is inferred, we are witnessing what is usually called by historians of science a *thought experiment*. If this is not a mis-application of a well-used, perhaps even over-used, historical category, then some scrutiny of the nature and function of the fourteenth-century thought experiment may well add to an evaluation of their total role in history. Let me begin by considering a recent suggestion concerning the role of thought experiments: that set forth by Thomas Kuhn.⁴⁰ Kuhn

tury grand master of infinite series. The truth is that he knew where he was going, as it were, before he even began to get there (something most frequently true when any infinite series arises in the Middle Ages). For, note well, he has chosen the series of rarefied parts so that, given the required *in duplo tardius* rarefaction between them, the sizes of the amounts added through rarefaction will follow the same series as the intensities for all rarefied parts. Therefore, any two added amounts will, taking their intensities into account, contribute the same to the measure of the whole. But this is but Swineshead's way of saying what we would translate by pointing out that the general term of the series representing the rarefied parts adds a constant $\frac{1}{2}$. In sum, what he accomplished mathematically was substantial, but it was not what our complicated infinite series translation implies. To see this is to see the kind of lesson I had in mind. (The relevant text in Swineshead is the following (*Lib. calc.*, ed. Venice, 1520, fol. 7ra): Sit a tale cuius prima pars proportionalis sit aliqualiter intensa, et secunda in duplo intensior et tripla in triplo intensior, et sic in infinitum [this much gives the initial convergent series mentioned above]. Tunc A est solum finite intensum, ut predicitur. Ponatur igitur quod secunda pars proportionalis A aliqualiter velociter rarefiet acquirendo quantitatem, et quarta pars proportionalis in duplo intensior secunda parte proportionali in duplo tardius acquirat de quantitate, et pars in quadruplo intensior illa secunda in quadruplo tardius acquiret quantitatem quam illa secunda, et sic in infinitum; et nunc incipiat huiusmodi rarefactio in illis partibus, stantibus omnibus aliis partibus non rarefactis nec condensatis. Et sequitur quod A solum finite velociter rarefiet seu maiorabit et secunda in duplo tardius et sic in infinitum. Et quod A subito in infinitum intendatur patet, quia ante quodcumque instans habebit A infinitas partes quarum quelibet tantum faciet ad intensionem totius, sicut est hoc certum datum, demonstrando illud quod faciet quantitas acquisita secunde parti proportionali; nam sicut aliqua pars acquisita alicui parti proportionali erit minor quam quantitas acquisita secunde parti proportionali, ita eadem pars erit intensior quam secunda pars proportionalis et quam quantitas sibi acquisita, ut ponit casus, addendo quod quelibet pars proportionalis in principio et continue sit uniformis. Sed hoc est generaliter verum: quod si proportionaliter sicut una quantitas seu pars alicuius est alia parte minor, ita eadem sit alia parte maiori intensior, equaliter facient omnes huiusmodi partes ad totius intensionem; ergo omnes iste quantitates acquisite infinite facient aliqualiter ad totius denominationem, id est, ante quodcumque instans, et per consequens cum erunt huiusmodi partes infinite, sequitur quod in infinitum facient ante quodcumque instans datum.)

⁴⁰Thomas S. Kuhn, "A Function for Thought Experiments," *Mélanges Alexandre Koyré*, Paris, 1964, vol. 2, pp. 307–334.

tentatively proposes, but then finally rejects, the view that thought experiments produce new understandings or clarifications of the scientist's "conceptual apparatus."⁴¹ If we interpret the basic rules of measure of which we have been speaking as at least part of such a conceptual apparatus, then surely late medieval rule testing falls tolerably close to performing such a function. To be more specific, one might add that each test, each new variant case, either (1) simultaneously confirms the basic rule as part of the relevant conceptual apparatus and adds to it in creating a new supplementary rule, or on the other hand (2) refutes an alternative basic rule that hitherto may have been considered part of another conceptual apparatus.⁴²

All of this, however, reveals little that is unexpected and surely nothing that could not be gained from the examination of other periods in early science. Another consideration is more proper, if not unique, to the natural philosophy we have been discussing. It relates to the claim in the same recent discussion to which I have referred that thought experiments arise not from the scientist's "mental equipment alone but from difficulties discovered in the attempt to fit that equipment to previously unassimilated experience. Nature rather than logic alone was responsible for the apparent confusion."⁴³ Here the thought experiments involved in fourteenth-century rule testing present contrary evidence; they have little, if anything, to do with experience in any proper sense of the term.

We instead here have to do with precisely what Kuhn wishes to deny, namely, with factors that are "applicable to any and every situation that might conceivably arise in any possible world."⁴⁴ *De potentia Dei absoluta* the medieval natural philosopher could licitly *secundum imaginationem* consider all logical possibilities. The only limitations were the law of non-contradiction, the restrictions implicit in the mathematical and logical techniques available to him, and the finite bounds of one's creativity as a thinker. Indeed, there are instances in which a particularly elaborate thought experiment is, as rule testing, carried out, and we

⁴¹Kuhn, *op. cit.*, pp. 308–309.

⁴²Parenthetically, I should add that, although medieval rule testing at times does serve this latter function, by far the greater share of such activity falls on the confirmatory side of things.

⁴³Kuhn, *op. cit.*, p. 329.

⁴⁴Kuhn, *op. cit.*, p. 322.

know on other grounds that the author in question did not even believe that this offered the proper resolution of the problem at hand (so, for example, Swineshead's complex treatment of the motion of a rod, acting as the sum of its parts, at the earth's center).⁴⁵ Furthermore, in many instances the result of rule testing is explicitly characterized as the generation of sophisms, a further indication of the imaginative, almost exercise-based, status these efforts were held to have in the fourteenth century. One must conclude, then, that advance occurred on the level of logical analysis and not at all, as Duhem would wish, on any inductivist plane. But this should not occasion surprise; it is even to be expected when science is so much a part of philosophy.

What I have tried to reveal of the new measure-dominated analysis of problems in natural philosophy is one of the keys, I believe, to the most important changes that occurred in the science of the fourteenth century. The other key—if anything even more predominant in late medieval material in spite of the fact that it has been necessary to devote less attention to it here—is what one finds in the new predisposition to submit every problem possible to a thorough-going dissection by the tools of the *logica moderna*.

Characteristic, I would claim, of new attitudes at this time, these two addictions allow us to see the real breaching of the limits in much within traditional Aristotelianism. How much more radical a departure it is to move from what Aristotle says (or implies) about the relations of V, F and R to Bradwardine and Swineshead than it is to run from antiperistasis to impetus theory. Or how much greater seems the distance between Aristotle's (and even Averroes') remarks on the proper category for motion and Ockham's analysis of motion as a term than between the Stagirite's consideration of Anaxagoras' infinitely small substances and late medieval deliberations about *minima naturalia*. Even in instances where Aristotle himself expended considerable time and effort treating a notion, the same contrast can be seen. Witness his careful and lengthy treatment of basically sensible infinites and compare it with the incredible number of new issues treated when one can view all of the logically possible ramifications of infinity, especially when these ramifications are subjected to repeated mathematical and logical (via supposition theory) analysis.

⁴⁵See Hoskin and Molland, *op. cit.* above (note 35).

Furthermore, what I have tried to emphasize in medieval *scientia de motu* also does much to resolve another issue that has bothered historians. Why, they have asked, was there no truly unified medieval mechanics?⁴⁶ Why did not Bradwardine—or any of his Mertonian colleagues for that matter—show interest in the problem of the continuation of projectile motion? Or why did no one save, by accident, Domingo de Soto in the sixteenth century, ever put uniformly difform motion and the Mean Speed Theorem together with acceleration in free fall?⁴⁷

They did not do so, I would suggest, because this kind of unification is congenial to the seventeenth century (from whence its notion is drawn) and not to the fourteenth.⁴⁸ But the late scholastics did have a kind of unification of their own. We find, for example, Bradwardine's rule and its calculus of ratios, and the conceptual language of *maxima quod sic* and *minima quod non*, both rephrased into the language of latitudes and the intension and remission of forms. Scales of measure are chosen in order to jibe with the rules of measure that will use them and even with apparently external, more general, philosophical factors. But of even greater significance, all of the conceptual languages and rules of which I have spoken were applied wherever possible—and sometimes even where not possible—in medieval thought. Not merely *scientia de motu*, but medicine, alchemy, all of philosophy and, especially theology, saw their mark. And why not? For science then was philosophy. In point of fact, I trust that much of what I have said in attempting to elucidate the character of the central development or change in fourteenth-century science has throughout urged, without explicit and continuous mention, its essential connection with natural philosophy.

THE FUTURE OF MEDIEVAL SCIENCE

I should now like to conclude by asking what suggestions all of this holds for the future historiography of this particular segment of medieval

⁴⁶One of the most informative treatments of this question is Ernest Moody, "Galileo and his Precursors," in Carlo L. Golino ed., *Galileo Reappraised*, Berkeley and Los Angeles, 1966, pp. 23–43.

⁴⁷See William A. Wallace, "The Enigma of Domingo De Soto: *Uniformiter difformis* and Falling Bodies in Late Medieval Physics," *Isis*, 59 (1968), pp. 384–401.

⁴⁸The case would be different, one would imagine, were there in the Latin Middle Ages (as I have suggested there is not) a well-defined body of knowledge constituting mechanics.

science. My prospectus is naturally not intended to be exhaustive in any way at all. Nor is it meant to furnish more than selected indicators with respect to methodology, presuppositions, point of view, and approach.

To begin with, we should recognize that in terms of the sheer quantity of source material extant, the kind of *scientia de motu* (together with all of its extensions) that I have been describing must have in some sense been a predominant concern. This is confirmed by its frequent appearance in what might best be called student notebooks.⁴⁹ Moreover, although such questions as the possibility of the rotation of the earth were in fact discussed, it is only our relating of this kind of question to Copernicus that would cause us to assess this as fundamental to a general statement of fourteenth-century concerns; one realizes this is true especially when one sees that treatments of the kind of problems I have discussed above outnumber discussions of the earth's rotation by something like 1,000 to 1.

Secondly, historians should make every effort to reveal patterns and connections that will assist us in explaining the apparently happy conjunctions of things very much part of natural philosophy with things that were clearly "bread and butter" issues in theology. Why should, for example, we find extensive use and discussion of Bradwardine's *De proportionibus motuum* within the confines of various fourteenth century Commentaries on the *Sentences*?⁵⁰ Or what history lies behind Thomas Buckingham's easy move to considerations of the intension, remission and latitudes of grace directly from an elaborate discussion of predestination and of God's prescience on the one hand, and of divine causality on the other?⁵¹ Again, continued attention to those factors I have urged as most

⁴⁹The most famous such "notebook" (Duhem had already seen its importance) is MS Paris, BN lat. 16621. Frequently referred to and utilized, it is still in need of analysis as a whole. Since we now know that this codex apparently belonged to a certain Jean de Falisca who possessed other, still extant, theological notebooks (Cf. P. Glorieux, "Jean de Falisca: La formation d'un maître en théologie au XIV^e siècle," *Archives d'histoire doctrinale et littéraire du moyen age* 33 (1966), 23–104), such an analysis will be likely to accomplish even more with respect to the place of the new trends in natural philosophy within fourteenth century intellectual history as a whole. I intend shortly to undertake this task; preliminary examination of several of the theological notebooks has already indicated a considerable penetration of the new conceptual languages I have been discussing.

⁵⁰Cf. Murdoch, *op. cit.* above (note 14), n. 57.

⁵¹Marie-Dominique Chenu, "Les 'Quaestiones' de Thomas de Buckingham," *Studia medievalia . . . in honorem R. J. Martin*, Bruges, 1948, pp. 229–241.

characteristic of fourteenth century natural philosophy will, I suspect, prove to be of help in answering such questions.

Further, in terms of the subject involved, the historian's search for accomplishments of significance should not be guided by the resemblance of the subject to some feature within modern science. Thus, I would submit that a good deal more of substance, of importance and of interest can be found, for example, in the medieval analysis of the motion of angels than in whatever astronomy occurs in Easter tables, or in the examination of the question of whether or not the infinite past time up to today is greater than the infinite past time up to yesterday than in the geometry of star polygons. One would discover more of importance because we would learn more of the whole tenor of late medieval thought.

Further, if my characterization of fourteenth-century science is correct, then, for the most part, this should tell us not to look for, or to expect, an application of results to nature (such as the relating of the Mean Speed Theorem to free-fall), nor to be critical because no such application was made. Thus, the analysis of unusual cases of change relative to speeds, force and resistances has, I think, no more to do with experience or nature than does the late medieval discussion of the truth of future contingent propositions (here Aristotle's sea battle has become '*Petrus est damnatus*'). Probably even less.

Further, proper realization of the importance and nature of the mathematical issues debated in fourteenth-century texts dealing with the measure of motion may well prevent ill-fitting evaluations of the significance of what was being done.⁵² Thus, when it is asked whether the natural motion of free-fall accelerates toward its end, in spite of the fact that temporal and spatial factors are mentioned, it is misleading to interpret that which is at stake as having to do with the alternative of velocity being proportional to space or velocity being proportional to time (the authors in question were probably not even aware of the distinction).⁵³ Rather, we can see that the major concern was merely whether *continue*

⁵²Cf. above, note 39.

⁵³That is, unless one carries matters all the way to $\Delta s/\Delta t$, it is perfectly consistent to maintain both proportionalities. For it is then quite plausible that one could maintain that the longer a falling body falls, the faster it falls, and the further it falls, the faster it falls.

velocitari should be interpreted as involving a divergent or a convergent increase.⁵⁴

Further, if the formulation of paradigms or the like is one's interest, then the equation of philosophy and science might suggest that for the Middle Ages we have been looking in the wrong places (namely, in modern science) for our guiding models. The history of philosophy as a whole might better be kept in mind. For a plurality of competing theories in philosophy is far more tolerable than in science; qualifications aside, Copernicus overthrows Ptolemy in a far more decisive fashion than Aristotle does Plato, or Ockham does Scotus.⁵⁵

Further, this same equation of philosophy and science allows one more adequately to judge the character of the medieval scientist and his activity. Thus, it seems to me that Swineshead fussing over mean degree measure or over Bradwardine's rule is far more like Peter Abelard worrying about universals and goatstags than he is like Copernicus or Galileo.

Finally, what I have tried to elicit as the basic character of fourteenth-century science is more consonant than some other interpretations with the views of observers who were far closer to the scene than we. Thus, we frequently find the complaint at the beginning of the sixteenth century, to cite a report of one student, that too much time is lost in discussing "situations that God could realize but which never happen, in treating the infinite, the intensity of forms in matter, and in examining whether the continuum is composed of points, etc."⁵⁶ This is but evidence of the legacy of fourteenth-century science at the beginning of the sixteenth. Students such as this one were, I believe, correctly assessing what this science was about; they were right, but in complaining, unappreciative, at least as historians.

⁵⁴Compare, on this point, the treatment of Clagett (*Science of Mechanics in the Middle Ages*, pp. 553–555, 565–569) and Anneliese Maier (*An der Grenze von Scholastik und Naturwissenschaft*, pp. 212–218).

⁵⁵In fourteenth century science, the latter kind of overthrow was more frequent, one of the results being the continued persistence of competing theories.

⁵⁶The student is Jean Forman; cited by Pierre Duhem, *Etudes sur Léonard de Vinci*, ser. 3, Paris, 1913, p. 162.

PHILOSOPHY, MATHEMATICS AND CONCEPTS OF SPACE IN THE MIDDLE AGES

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In one respect my thesis is identical with John Murdoch's. I consider, as he does, that philosophy and science are basically one in the Middle Ages. However, I shall lean more on the philosophical aspect and I shall not take as the principal theme of my paper the 14th century. A main subject will be Arabic science, or rather Arabic philosophy; that is to say, I shall speak about authors who, whatever their religion, wrote in Arabic.

Paul the Persian,¹ a Christian writer who wrote at the court of the Sassanid King Khosrau Anusherwan (that is to say before the Arab conquest), said, in characterizing the role of Aristotle, that before his time philosophy had been a series of dispersed sciences, just as ingredients of a medicament are sometimes dispersed and have to be brought together in order to form the medicament, in order that the medicament be efficacious. The dispersed sciences had been brought together by Aristotle in his *Corpus*; the latter formed (this being its great merit) a unified system of philosophy, which included natural science. Then the Arabic author who quotes Paul the Persian, proceeds to give a classification of the sciences.

The medieval classifications of sciences have a bearing on the theme of this paper. I shall refer to them briefly. First of all the main sciences in the Aristotelian corpus are 1) physics—that is to say the natural sciences which include also psychology, science of the soul; and 2) metaphysics—that is to say, a) the science of material mobile things and b) the science of immaterial substances.² And these sciences are supposed to give knowledge of the world as it really is.

¹See S. Pines, "Ahmad Miskawayh and Paul the Persian," *Iran-Shināsi*, II. 2 (1971), 121 ff.

²I.E. God and the so-called "separate" (that is to say immaterial) intellects.

Now, the world as it really is is a somewhat ambiguous conception and the view of the medieval Aristotelians has been sometimes misinterpreted in this connection. The world of the Aristotelians is much more of a construct than is often thought. That is to say, it is constructed by the senses and the intellect, because both the senses and the mind actualize things which exist only potentially in the world. In short, the world is to a certain extent constructed by man. Kant's Copernican revolution is in this sense much less revolutionary than it is sometimes supposed to be.

Metaphysics, as I said, is a science which deals with immaterial substances. However, in at least one case, the case of Al-Fārābī, a Moslem thinker seems to have questioned the assumption that man could have knowledge of metaphysics. In one phase of his thought he doubted the possibility of man being able to know the immaterial substances. The noteworthy point about this position, which casts a doubt on the status of the special science of metaphysics, is that it was expressed within an Aristotelian (and to some extent Platonic) framework. The Stoics and Epicureans denied the existence of immaterial substances. Al-Fārābī, on the other hand, affirmed that they existed, but doubted that man could ever obtain knowledge of them. And this might mean that intellectual perfection, which is man's supreme happiness, could not be reached. Accordingly the opinion was expressed by, or imputed to, Al-Fārābī that man could only obtain civic or political happiness.³ A corollary would be that his main, his supreme, activity should be political rather than intellectual. However, this is by no means characteristic of medieval philosophers. In most cases they believed in the possibility of metaphysics. While none of them doubted the possibility of physics.

Those were, as I said, the main sciences. The places of some other sciences were less strictly defined within the classification I have referred to. Of course there were disreputable sciences—for instance, alchemy and astrology—which the Aristotelians, I refer to the Arabic Aristotelians, did not believe in. But these sciences had adepts in the astrologers and the alchemists, and some of the latter toyed with the idea that science could give man ultimate power, in the first place ultimate political power. There were some legends that through alchemy certain dynasties had ob-

³This view may have influenced Marsilius of Padua. I hope to go into this question in another paper.

tained power; there were also the projects whereby men, and even prophets, might be created in the laboratory. As I have suggested these dreams and aspirations were rejected by the Arabic Aristotelians. Among other sciences, medicine and the science concerning the various properties of substances, minerals and others, may be mentioned.

Medicine was a practical science, and it had its place as such in the Aristotelian classification of sciences. However, it had to lean much more heavily than physics or metaphysics on experience, on experiments—the causes why medicaments worked were not known. One had to rely on experience and to see whether certain medicaments did procure cures or not, and to a great extent because of this an experimental method was worked out. Avicenna, the philosopher, was also a physician, and in his great medical work *al-Qanūn* he adumbrates the following method: if one believes that a medicament can procure a cure and wants to put this belief to the test, one should use that medicament varying all the other factors, so that in this way one can find out whether it really does work or not. This means that in this kind of research one does not seek the causes, which is the Aristotelian method—one only looks to the effect, trying to discover if it comes about under various conditions.⁴

The science of properties, i.e. of various properties—incomprehensible from the Aristotelian point of view—which were ascribed to minerals and other substances, has in this respect a certain similarity with medicine. Al-Rāzī, another physician, (who died in either 923 or 925 or 932), and the author of a treatise on the properties of minerals, adopts an apologetic tone in the beginning of this work. For he is aware that he will be blamed for treating of this subject. He admits that some of the phenomena with which he is dealing may be non-existent, but takes up the position that they should not be dismissed out of hand because they are incomprehensible from a scientific point of view, because no scientific explanation has been found for them. They should rather be put to the test of experience. I may mention that he cites as phenomena that have stood this test the following “facts”: 1) the attraction exercised by scammoning upon yellow bile; and 2) the loss of force incurred by a magnet rubbed with garlic and the recovery of this force through the magnet's being washed with vinegar. It may be noted that Francis Bacon

⁴See A. C. Crombie in G. M. Wickens ed., *Avicenna, Scientist and Philosopher: a millenary symposium*, London, 1952, p. 89 ff.

likewise lists the testing of beliefs of this sort among his desiderata,⁵ though he does not seem to give it a high priority.

Because of their favoring the experimental method, physicians were often looked down upon by the philosophers. Thus in spite of Al-Rāzī's proclaiming himself a disciple of Plato and having been influenced to some extent by the *Timaeus*, he was regarded as only a physician, that is to say, as a man who had no aptitude for philosophy. As Avicenna, being a physician who was also a more or less Aristotelian philosopher, put it, Al-Rāzī was only fit to examine urines and similar things.

However, it is not in medicine and not in the science of the properties of minerals that matters came to a head but in mathematics, because mathematics looked at from the Aristotelian point of view posed a grave problem. In a letter written in 1669 to Jacob Thomasius, Leibniz, who at that time was twenty-three years old, refers to and refutes a view, probably held by some contemporary thinkers, according to which Aristotle did not consider geometry as a science. Leibniz admits that some Aristotelian passages can, by dint of misinterpretation, be made to appear to support this view; many other passages can however be adduced in proof of the opposite opinion.

As Leibniz points out, Aristotle used in the *Analytics* geometrical demonstrations as a model for demonstrations in other sciences. It is in this context that Leibniz remarks that the scholastics tried hard to demote mathematics from its position as one of the perfect sciences. It seems to me that in this statement Leibniz was rather unfair to the schoolmen. The responsibility for the ambiguous status of mathematics in medieval philosophy should be imputed to the genuine Aristotelian conception of this science.

In the remainder of this paper, 1) some conceptions of the mathematical sciences and their subdivisions figuring in medieval (mainly Arabic) classifications of sciences will be mentioned; 2) the problem stemming from the clash between astronomical doctrine and Aristotelian physics will be referred to; 3) a brief survey will be made of the functions attributed to the intellect, the imagination and other psychic faculties in the conception of three-dimensional non-Aristotelian space and of geometrical objects. Both Arabic and scholastic authors will be

⁵See, for instance, Francis Bacon, *Parasceve ad Historium Naturalem et Experimentalem: Aphorismi De Conficienda Historia Prima*, § III.

referred to, and an aspect of the 17th century scientific revolution will be touched upon. 4) In the concluding remarks an apparent resemblance between some medieval views and certain facets of Leibniz and Kant's doctrines concerning one of the functions of the imaginative faculty will be noted.

Medieval philosophers, those who endeavored to be strict Aristotelians as well as many of those who in some points had no qualms about deviating from the peripatetic doctrine, speak of the abstract nature of mathematics and of mathematical objects. On this point they followed the teaching of Aristotle. The latter states his opinion inter alia in the following passages: ". . . mathematical objects are said (to exist) by abstraction, but physical objects by addition . . ." (thus he describes the relation of mathematical to physical objects, and vice versa, at *De Caelo*, III. 1, 299a 15 ff.); "This is like the mathematician's study of abstractions for in this study, he eliminates all sensibles as for instance weight and lightness, hardness and its contrary, heat too and cold, and all the other sensible contrarieties and leaves only the quantitative and continuous" (*Metaphysics* X.31, 1061a 28 ff.; following Heath's translation); this passage refers specifically to geometry.

The following passage which occurs in Averroes' *Great Commentary* on the *Metaphysics* (M. Bouyges ed., Beirut, 1938, vol. I. p. 299), and to which many parallels in Arabic philosophical writings can be found, sets forth a point of view similar to that of Aristotle: ". . . They (the mathematical sciences) take (as their objects) numbers and magnitudes (considered) as separate from that which exists (or: from the existent thing)." The fact that the mathematical sciences were thought to deal in abstractions meant inter alia that the medieval philosophers did not regard them as providing a correct model of the real world, such as was offered by the natural sciences, i.e. Aristotelian physics. For this reason mathematics was in general considered as inferior to physics; it was useful as an introduction to the latter (thus the Ikhwan al-Safa, Maimonides and many others). Avicenna's Platonizing classification (*Aqsām al-'Ulūm*, Tis' Rasā'il, Cairo, 1908, pp. 105 ff.), in which he lists physics as the lowest theoretical science and mathematics as an intermediate one (metaphysics being the highest), is an exception.

This conception of the methods and objects of the mathematical sciences, accounts for the statements of Arabic philosophers which contest

the legitimacy of the use of these sciences in the study of nature. Two passages out of many can be quoted here. In the treatise entitled *Fi'l-Falsafa al-Ulā*, (M. Abu Rida ed., *Rasā'il al-Kindī al-Falsafiyya*, Cairo, 1950, p. 111), al-Kindī makes the following observation: ". . . Everything that is natural (or physical) is endowed with matter. Hence it is impossible to use mathematical investigations in (dealing with) the existence (or: in discovering) natural things. For an (investigation of this kind) is (only) appropriate to that which is not endowed with matter." Avicenna in *K. al-Najāt* (ed. Cairo 1331 H, p. 339) remarks: "These mathematical sciences are not (qualified) to examine either the essences of the (material) substances (al-jawahir) or quantities in so far as they (subsist) in (these) substances."

In view of this conception of the objects of mathematics and the resulting contrast between this discipline and physics, the fact that the mathematical science—such as astronomy, optics, the science of music and the science of weights—aimed at providing a satisfactory model for some classes of phenomena belonging to the real physical world constituted a problem, for which a compromise solution was often proposed. Thus Ibn al-Haytham (Alhazen), for instance, describes⁶ optics as being both a mathematical and a physical science. And Averroes (*op. cit.*, vol. I, p. 51) propounds the generalization that mathematics treats of immaterial things or of those aspects of material things which are not affected by matter.

However, to many philosophers this compromise solution appeared to offer an inadequate answer to the difficulties posed by the relation between physics and astronomy, which in Aristotle's view (cf. *Metaphysics*, 1073 65 f. and Averroes, *op. cit.*, vol. III, pp. 1646 and 1654) was the mathematical science that had the closest connection with philosophy. The difficulties stemmed from the fact that the epicycles and eccentric centres of the Ptolemaic system were irreconcilable with Aristotelian physics, according to which the point at the centre of circular movements must be stationary.

In theory astronomy could of course be completely divorced from physics, and confined to the task of saving appearances (*sōzein ta phainomena*), i.e. elaborating mathematical theories—there could be several

⁶In his *Treatise on Light*, edited by J. Baermann, see *Zeitschrift der deutschen Morgenländischen Gesellschaft*, 36 (1882), 197.

of them—which would account for the observational data. This of course implies that they need not be regarded even by their partisans as giving a true description of the structure of the real world. This solution, envisaged by some Greeks, appears to have been suggested in Moslem times by Ibn al-Haytham (see Al-Bayhaqī, *Tatimmat Siwān al-Hikma*, M. Shafi ed., Lahore 1935, p. 79); it was a consistent but precarious position.

At this point it may be apposite to remark that the Ptolemaic system, as well as other hypotheses proposed as a substitute for it, did not provide an example of a complete divorce between astronomical theory and physical science. Thus Averroes, among others, remarks (*op. cit.*, vol. III, pp. 1655 f.) with regard to these hypotheses—or perhaps to the Ptolemaic system exclusively—that the *tekhnē* of astronomy has taken over from physics the principle that the heavenly bodies must have uniform motion, and that they cannot move in a straight line or execute a retrograde movement. The adoption of these physical principles can logically be regarded as signifying that "saving appearances" was not Ptolemy's sole aim, that to some extent, at least, his system was intended to refer to the real physical world. And this to my mind may legitimize the impugnment of this system by the Aristotelians.

This clash plays a considerable part in Maimonides' critique of some tenets of Aristotelian philosophy. In emphasizing the apparently insoluble conflict between the two sciences, and in expatiating on our uncertainty with regard to the nature and motions of the heavenly bodies, he seems to have in view an end which may be described in Kant's words: "Ich musste also das Wissen aufheben, um zum Glauben Platz zu bekommen" (Introduction to the 2nd edition of *Die Kritik der reinen Vernunft*). Maimonides, by painstakingly proving that human knowledge is limited, is able to contend that the truth of the Aristotelian teaching concerning the eternity of the world may, with a show of reason, be questioned. It may be argued that Maimonides' exposition of the limitations of human reason—exemplified inter alia by the conflict between physics and astronomy—was his most substantial contribution to non-Jewish philosophy; it certainly had a far-reaching effect. Given this fact, the question as to whether he really believed in these limitations (there are some grounds for thinking that he did not) could be set down as frivolous or unimportant.

We shall now go on to the last subject of the present paper, which is (as has already been mentioned) the role of the imagination in mathematics and in conceptions of space according to mediaeval philosophers, and some parallels to their teachings in the ideas of post-mediaeval philosophers, notably Kant. The subject is a vast one and of considerable importance for the history of philosophy, but, as is the case with regard to other subjects referred to in this paper, it can only be discussed here in a very sketchy way.

In fact the definition of the subject given above leaves out an essential part of the story, namely the views of the Greek philosophers on the role of the imagination in the creation of mathematics. To cite but one instance, the lack of an analysis of the elaborate epistemological doctrine of Proclus propounded in his *Commentary* on the 1st book of Euclid's *Elements*⁷ is a glaring omission. With regard to this doctrine I shall only mention the following points which seem to lead up to mediaeval teachings, that Proclus observes 1) that Plato considers that mathematics is the domain of *dianoia*⁸ 2) that in its mathematical (or geometrical) thinking *dianoia* is aided by, and cannot do without, imagination (*phantasia*⁹).

Now in Arabic philosophy, as we shall presently see, mathematical thinking is often considered as originating in a psychic faculty called *wahm*, a term which was rendered by the Latin schoolmen as the "estimative faculty." Now it so happens that in the translations from Greek into Arabic the term is not only used to render both *phantasia*¹⁰ and *dianoia*¹¹, it also serves to render a third Greek word, namely *doxa*. *Doxa*, as *wahm* (or other derivatives from the Arabic root *w.h.m.*) when used as an equivalent of the Greek term, differs from the imaginative faculty conceived as passively reproducing the sense data transmitted to it; the Greek as well as the Arabic term may be used to designate a faculty

⁷An attempt at such an analysis may be found in S. Breton, *Philosophie et Mathématique chez Proclus*, Paris, 1969.

⁸Proclus, G. Friedlein ed., *In Primum Euclides Elementorum Librum Commentarii*, 1873, pp. 3-4.

⁹Cf. for instance, *ibid.*, pp. 54-55, 185.

¹⁰Cf. Averroes' quotation of Aristotle's *Metaphysics*, 1024b 26, where *mā yata-sawwar bi' l-wahm* renders *phantasia*. See Averroes, M. Bouyges ed., *Tafsīr Mā Ba'd at-Tabi'at*, vol. II, p. 687, ll. 4-5.

¹¹To quote but one example, in a quotation occurring in Averroes, *op. cit.*, p. 921, l. 18, of *Metaphysics*, 1036b, *dianoia* is rendered by *wahm*. In this passage the abstractive function of *dianoia* is referred to.

one of whose functions consists in confronting the sense-data with what is considered as the true reality and judging them accordingly. It is in part in virtue of this faculty that men judge the sun to be bigger than it appears to be (see Aristotle, *De Anima*, III.3 428 a-b¹²). I shall follow the schoolmen in using for *wahm*, and other derivatives from the same root, the term "estimative faculty." It should however be borne in mind that it must have been difficult for the Arabic reader, who was aware of the other connotations of *wahm* in philosophical language and also of ordinary linguistic usage, to regard the word, in whatever context it was used, as other than a synonym of the various terms signifying "imagination."

Avicenna appears to consider mathematics as having a special relation to *wahm*, which meant in his linguistic usage a faculty combining imagination with judgment. This conception of mathematics may 1) be inferred from a statement in Avicenna's *K. al-Najāt* (Cairo 1331 H, p. 158) in which it is stated that every theoretical science¹³ has as its object either "existent" things or things "conceived by the estimative faculty" (*al-wahmiyyāt*¹⁴). By means of a process of elimination it can be shown that the latter cannot possibly be the object of any theoretical science other than mathematics. This conclusion is borne out by 2) a passage of the Metaphysics of Avicenna's *K. al-Shifā*,¹⁵ II, 1, referring to a science which deals with that which exists in (things) perceived by the senses, but is abstracted from these (things) by means of the estimative faculty (*al-tawakkum*) and by means of the (act of) definition (*al-tahdīd*). It is evident that the science which Avicenna has in mind in using this phrase is mathematics. In this connection the following statement made by the 13th century philosopher Nasīr al-Dīn al-Tūsī in his *Commentary* on Avicenna's *K. al-Ishārāt wa'l-Tanbihāt* may be quoted:¹⁶ "It is legitimate that the judgments of the estimative faculty (*al-wahm*) are recognized as true by the intellect. And because of the

¹²Cf. S. Pines, *Nouvelles Études sur Awhad al-Zamān Abu'l-Barakāt al-Bagh-dādī*, Paris, 1955, p. 47ff.

¹³*Sinā'ā nazariyya*. Literally "Theoretical art." In the context it is clear that in this passage Avicenna does not differentiate between "art" and "science."

¹⁴An alternative translation is "imaginary things."

¹⁵Father Anawati and S. Zaid eds., *K. al-Shifā': Ilāhayyāt*, Cairo, 1960, vol. I, p. 11.

¹⁶See S. Dunya's combined edition of *K. al-Ishārāt wa' l-Tanbihāt* and Tūsī *Commentary*, Cairo, 1960, vol. II, p. 403.

correspondence between them, there is hardly any difference of opinion with regard to (the kind of propositions that are found in) geometry (*al-handasiyyāt*).” In spite of the somewhat ambiguous wording of this assertion, it seems clear that in the opinion of Tūsī, who tried to offer a correct interpretation of Avicenna’s views, geometrical judgments originated in the estimative faculty, which, as we know, is or can be a kind of imagination.

Euclidean geometry appears to require a three-dimensional infinite space, whose subsistence—unlike the existence of the Aristotelian two-dimensional place, defined as “the limit of the surrounding body” (Physics IV.4, 218a 5-6)—is wholly independent of any relation it may have to bodies. Clearly such a three-dimensional space could only be admitted by faithful Aristotelians insofar as it was confined to the abstract sphere of mathematics and not regarded as a physical fact—as was done by such non-Aristotelian philosophers as Abū Bakr al-Rāzī and Abu'l-Barakāt al-Baghadādī and also by Ibn al-Haytham, who was not an outspoken opponent of Aristotle.

In a way somewhat similar to Newton’s, Abū Bakr al-Rāzī makes a distinction between absolute and relative space. Absolute space, which is three-dimensional and infinite, exists quite independently of the bodies contained in it.¹⁷ In the present paper we are concerned with his epistemological (rather than his purely physical) views on the matter. The very scanty information available as regards this point can be summarized as follows: 1) Al-Rāzī appears to suggest as a proof for the existence of three-dimensional space the fact that if one removes by means of the estimative faculty, or more probably the imagination (*al-wahm*), (all existing) bodies, absolute space will still remain in existence, i.e. it will still be imagined by that faculty (*wahm*).¹⁸ Elsewhere he adduces as a proof for the existence of empty space outside the world the fact that simple folk, whose soul has not lost its spontaneity, state that their reason tells them that such a space exists.¹⁹ In other words, Al-Rāzī considers that the fact that a physical conception or representation is ac-

¹⁷Al-Rāzī also postulates the existence of Absolute Time, which is a “flowing substance.” Unlike Aristotelian time its existence is not dependent on that of motion. It would continue to exist even if the world were to be annihilated.

¹⁸P. Kraus, *Abu Bakr Rhagensis (Razis) Opera Philosophica*, Cairo, 1939, p. 306.

¹⁹S. Pines, *Beiträge zur islamischen Atomenlehre*, Gräfenhainichen, 1936, p. 55. Al-Rāzī’s theory of matter, which, like his conception of space and time, is un-Aristotelian, is discussed in this study on pp. 40 ff.

cepted as certain 1) by reason, or 2) by the estimative faculty or the imagination, is a proof of its real existence.

According to Abu'l-Barakāt,²⁰ the notion of empty three-dimensional space filled with bodies is prior in the human mind to that of a *plenum* and is known through being innate (*maftūr*). This knowledge, as well as our conviction that beyond every limit there exists either empty space or space filled with bodies, may be attributed, according to him, either to reason or to the estimative faculty. This ascription is a matter of indifference—and in fact meaningless within the framework of his psychological theory. The essential point is that our conviction is true because it stems from *a priori* ('alā awwaliyatihā) knowledge. It may be noted that a similar opinion was discussed prior to Abu'l-Barakāt. Avicenna refers to and rejects the view that there exists a three-dimensional space recognized through innate (*maftūr*) knowledge. Apparently the opponents against whom he polemizes attributed this knowledge to an innate intellectual and estimative disposition of the mind, whereas he denies that a disposition of the intellect plays any part in the matter. According to him this conception is due to the estimative faculty, which, on this point, follows the faculty of imagination.

As far as we know, Ibn al-Haytham propounds his conception of space without explicitly referring to innate knowledge. In his *Risālat al-Makān* (published in Hyderabad 1357 H), he speaks of an imaginary vacuum (*khalā' mutakhayyal*), which is filled with bodies. This vacuum is clearly identical with three-dimensional space. The adjunction of the word “imaginary” (or if one translates literally: “imagined”) raises a question (which admits of no clear-cut answer) as to whether the “vacuum” of which Ibn al-Haytham speaks should be considered as a physical fact or as a mathematical notion.

The views on mathematics and infinite space current among the Aristotelian schoolmen of Christian Europe are for evident reasons similar to those held by the Arabic Aristotelians. Thus Aquinas considers that mathematics is a science that abstracts from matter perceived by the senses.²¹ He also holds²² that mathematical lines and figures are conceived by the imagination (*phantasia, imaginatio*).

²⁰S. Pines, *Nouvelles Études*, pp. 16 ff.

²¹See, for instance, his *Expositio* on the *Metaphysics*, II, XIV, 516.

²²*Op. cit.*, VII, X, 1494 f. At 1495, he remarks that imagination applied to mathematics may also be called *intelligentia*, because it deals with things (*res*) without the help of the senses.

Furthermore he observes²³ that mathematical magnitudes constituted by the imagination seem to be infinite, because whatever the magnitude one may imagine a greater one.²⁴ In a similar way it may seem that beyond (the limits of) heaven, there exists an infinite space, because one may imagine it.²⁵ In the 14th Century Thomas Bradwardine refers to the imaginary infinite space or vacuum which exists outside the world. God is necessarily present in this vacuum.²⁶ Many other passages using a similar terminology could be mentioned.²⁷

At this point, an attempt will be made to show, or to suggest—necessarily in very summary fashion—that some of the mediaeval conceptions which have been discussed, particularly the one dealing with the function of the imaginative faculty, have a certain relevance to physical theories which came up after the scientific revolution of the 17th Century.

From the Aristotelian point of view this revolution can, at least to some extent, be interpreted as aiming at, and resulting in, the replacement of the Aristotelian non-mathematical science of physics by "mathematical" sciences, for which within the mediaeval classification such traditional disciplines as astronomy, optics and the science of weights could be said to have provided models.²⁸

One of the consequences of this tendency to reduce physics to mathematics was that the existence of the Euclidean three-dimensional infinite space, which used to be a primary assumption of the geometers,

²³See his *Expositio on the Physics*, III, VII, 341.

²⁴He also remarks (*ibidem*) that number seems to be infinite, because the intellect can add a unit (*unitatem*) to whatever number is given.

²⁵According to him (*ibidem*) the assumption that there exists beyond the heaven an infinite space entails necessarily the conclusions: 1) that there exists an infinite body; 2) and/or that there exists an infinite number of worlds. In the beginning of the argument Aquinas appears to assert that *both* conclusions follow necessarily from the assumption; but further on he seems to imply that if one accepts one of them, the necessity for admitting the other is removed.

²⁶See A. Koyré, *Archives d'Histoire Doctrinale et Littéraire du Moyen Age*, XVII (1949), 82.

²⁷Three centuries after Bradwardine, Gassendi uses the term "imaginary spaces," these being spaces situated outside our world; God could create in them other worlds (*De Motu*, vol. III, ep. I, cap. XV, p. 494, quoted by A. Koyré, in *Etudes Newtoniennes*, Paris, 1968, p. 215).

²⁸A statement of Descartes may be used to illustrate this trend. In a letter to Mersenne, written in 1638, he asserts that "if he wishes to take a look at what I have written about salt, the rainbow, snow and so forth, he will know that all my physics is nothing but geometry" (Tannery-Adams eds., *Oeuvres*, vol. II, p. 268).

but rejected by the physicists, came to be regarded as a physical fact,²⁹ and as such thought to be one of the foundations of physical theory. This is notably the case in the Newtonian system of physics. It may be noted that, in common with Arabic non-Aristotelian philosophers, he considered³⁰ that the existence of an extension separate from, and independent of, bodies is proved by the fact that such an extension can be clearly conceived and that spaces beyond the limits of the world or empty of bodies can be imagined. This means *inter alia* that our clear conceptions and our imaginings provide valid evidence as to physical reality.

The Newtonian conception of space was attacked by Leibniz; his critique may be found in his well-known correspondence with Samuel Clarke. It would be out of place in the present paper, which is concerned with the problem of continuity in the history of ideas, to attempt to indicate the novelty and the revolutionary character of some of Leibniz mathematical and physical methods or conceptions. On the other hand it seems appropriate to note, very briefly, the fact that in dealing with the subject in which we are interested, perhaps to some extent quite consciously, he takes over and adapts Aristotelian and/or scholastic notions.³¹ This is notably true with regard to the role which he assigns to the imagination.

Like the scholastics, and perhaps in more radical fashion than is generally done by them, he holds that mathematics belongs to the domain of the imagination. It is, according to him "the logic of the imagination."³² In elaborating on this view, he was able to achieve a new con-

²⁹Newton and other natural philosophers believe that space is not only a physical, but also a theological fact.

³⁰See Koyré, *Études Newtoniennes*, p. 109.

³¹Leibniz' knowledge and utilization of the teachings of the schoolmen should be more thoroughly investigated than has yet been done. He mentions explicitly some scholastics; and in a letter to Basnage, written in 1706, he refers to his having formerly known the scholastics, better than he knows them now (see Leibnitz, C. I. Gerhardt ed., *Philosophische Schriften*, Hildesheim, 1960, vol. III, p. 143).

³²See L. Couturat, *La Logique de Leibniz*, Paris, 1901, pp. 290–291. Cf. the quotations from Leibniz occurring on p. 291, notes 1 and 2. a) "Mathesis universalis tradere debet Methodum aliquid exacte determinandi per ea quae sub imaginationem cadunt sive, ut ita dicam, Logicam imaginationis." b) "Mathesis est scientia rerum imaginabilium. Metaphysica est scientia rerum intellectualium." c) "Imaginatio generaliter circa duo versatur, Qualitatem et Quantitatem, sive magnitudinem et formam; secundum quae res dicuntur similes aut dissimiles, aequales aut inaequales."

ception, more comprehensive than the traditional one, of the functions of mathematics. On the other hand, the fact that (in his opinion) mathematics could to a large extent be defined as an activity of the imagination seems to have legitimized to his mind his distrust³³ of the aprioristic physical theories of such mathematicians as Newton.

In his 5th letter to Samuel Clarke (§ 5 and 6) he says in attacking the Newtonian concept of space: "Ainsi la fiction d'un univers matériel fini, qui se promène tout entier dans un espace vide infini, ne saurait être admise . . . ce sont des imaginations des philosophes à notions incomplètes, qui se font de l'espace une réalité absolue. Les simples mathématiciens, qui ne s'occupent que des jeux de l'imagination, sont capables de se forger de telles notions; mais elles sont détruites par des raisons supérieures." Further on in the same letter (in § 7) he says: "Puisque l'espace en soi est une chose idéale comme le temps, il faut bien que l'espace hors du monde soit imaginaire, comme les scolastiques même l'ont bien reconnu. Il est de même de l'espace vide dans le monde, que je crois encore être imaginaire par des raisons que j'ai produites."³⁴

In other words the imagination of the mathematicians (which is indispensable in the domain which is all its own, that of mathematics) is not equipped to deal with physical reality. Leibniz's view, that the conception of an infinite empty space, of a space beyond the limits of the world, of space as absolute reality, can be said to derive from the imagination and is for this reason (among others) invalid in physics, doubtless stems from the doctrines of the mediaeval Aristotelians.

The objectives and the final conclusions of Kant's *Critique of Pure Reason* are, insofar as they are related to the subject under discussion, different from, and indeed opposed to, those which may be found in Leibniz's writings. For Kant accepts the Newtonian physics and wishes to give it a philosophical validation. However Kant, just like Leibniz, is unable to accept the concepts of absolute space and absolute time, on which Newtonian physics is largely based, as part of objective physical reality.

³³His distrust of a recourse to the imaginative faculty in constructing a physical theory may be seen in a passage in a letter written in 1686 to the Landgrave Ernst von Hesse-Rheinfels. The passage which may contain a criticism of Descartes' physics reads: (Gerhardt ed., *Philosophische Schriften*, vol. II, p. 12): "Que les notions qui consistent dans l'étendue enferment quelquechose d'imaginaire et ne sauraient constituer la substance du corps."

³⁴See also Leibniz's 3rd letter.

Hence his predicament or, to be more precise, that part of it which concerns us. Through his resolution of the difficulty (which will be touched upon here only insofar as the concept of space is concerned) he preserves to some extent—and that seems to me a characteristic trait—a traditional mediaeval and Leibnizian conceptual framework, disguised and changed in such a way that it is made to legitimate the Newtonian physical theory.³⁵

The connection of Kant's conception of space with the traditional notions becomes clear if one calls to mind the role ascribed to the faculty of imagination in the various doctrines in question.

In a study named *Kant und das Problem der Metaphysik*,³⁶ M. Heidegger discusses the nature of the function ascribed to the ("productive" or "pure" or "transcendental") imagination (*Einbildungskraft*) in the *Critique of Pure Reason*.³⁷ Some at least of his interpretations seem to be philologically sound. To my mind he proves that within the framework of Kant's theory the "pure perceptions," i.e. space and time, are "rooted" in the pure imagination. This means inter alia that the designation in a passage of the *Critique of Pure Reason* of "pure space" and "pure time" as "imaginary beings" (*ens imaginarium*) should not be regarded as an inconsequential description or an isolated slip of the pen;³⁸ for the term accurately situates space and time within Kant's system. In so far as it relates to space, it is of course also reminiscent of, and undoubtedly derived from, the mediaeval Aristotelian or the Leibnizian references³⁹ to three-dimensional infinite space as an imaginary (or imagined) entity.

³⁵What was Kant's attitude towards the traditional (i.e. in the last analysis Aristotelian and scholastic) philosophical framework? There is some evidence to show that, on the whole, he was careful to preserve the structures of traditional philosophy. This tendency may have had some influence on the content of his teaching. A relevant example is given in my paper "Spinoza's *Tractatus Theologico-Politicus*, Maimonides and Kant" in *Scripta Hierosolymitana XX*, Jerusalem, 1968, pp. 49 ff.

³⁶Frankfurt a. Main, 1951.

³⁷The importance of this function is stated more explicitly in the first than in the second edition of this work.

³⁸The passage in question reads in the *Kritik der reinen Vernunft*, 2nd edition, p. 347, as follows: "Die blosse Form der Anschauung, ohne Substanz, ist an sich kein Gegenstand, sondern die bloss formale Bedingung desselben (als Erscheinung), wie der reine Raum und die reine Zeit, die zwar etwas sind, als Formen anzuschauen, aber selbst keine Gegenstände sind, die angeschaut werden (*ens imaginarium*)."

³⁹Some of which may be found above.

In his effort to preserve or restore the framework of traditional philosophy, while adapting it to the exigencies of Newtonian science⁴⁰ and jettisoning a considerable part of its doctrinal content, Kant discards Newton's doctrine that absolute, i.e. Euclidean, space forms a part of objective physical reality; he adopts the mediaeval view known to him through the intermediary of Leibniz, and no doubt through other channels, that such a space is a product of (transcendental) imagination, or, as he also puts it (the difference being a purely terminological one), a form of perception. However, whereas the Aristotelians and Leibniz believed that in this matter physical thinking should not follow the imagination,⁴¹ Kant was of the opinion that the subjective presupposition of the existence of the "imaginary" entities pure space and pure time is a precondition of physics.

Thus the Newtonian Absolute Space and Absolute Time are fitted into a traditional framework, within which they are regarded as they used to be in the Middle Ages as figments of the imagination, but at the same time, and because of their being products of the imagination, they are given at least as basic a role in the foundations and explication of physical theory as the one assigned to them by Newton.

⁴⁰Kant had, of course, also in mind the necessity to produce a philosophy capable of withstanding the onslaught of Hume's scepticism.

⁴¹Ibn al-Haytham and perhaps some other mediaeval philosophers form in this respect an exception; but their level of philosophical sophistication is incomparably lower than that of Kant.

DISCUSSION

On papers by J. MURDOCH and S. PINES

MENDELSON: The papers this morning both raised a series of points which I think are worth delving into in some detail.

Perhaps a good place to begin might be to point to what I found particularly in Prof. Murdoch's paper and perhaps less obviously in that of Prof. Pines, this was a strong plea for a belief in discontinuity. That is, that the mental activities which they were describing were really in large measure discontinuous with the activities described as science in the time of Galileo or the period thereafter. The implication being that this discontinuity is an important fact to recognize if we are to realize the context in which ideas emerge and what purpose they serve, where they came from and how they were used. If there is discontinuity I would really ask a sort of leading question: Are there other sources of this discontinuity which have to be examined, or do you have invention de novo of something called science in the 16th and 17th century which should go by an entirely different title in earlier periods? Can you really call what is being discussed in this 14th century period, which John Murdoch talked about, "science" and also call what Galileo deals with and Newton deals with in the 17th century—"science"?

TOULMIN: I must protest at this point. I did not hear John saying that there was a discontinuity, in this sense: to go further, the characterization that John gave of natural philosophy, as it was done by these Oxford and Paris people, is clearly continuous with important elements in the theoretical activity and methodology which I understand Galileo and his successors to have been employing.

Of course, we all know the bogus Baconian historiographical tradition, according to which Galileo was an empiricist, inductivist and all the rest; but this is as irrelevant to the facts about Galileo as Duhem's attempt to find scientific positivism in the 14th century is to the facts about the 14th century people. Indeed, what I found most illuminating about John's account of the inquiries these people were concerned with is the very continuity with the conceptual-analysis element in 17th century theoretical physics.

MENDELSON: The implication is in the ear of the hearer.

ROSENFELD: First of all I should like to express my great delight in Prof. Murdoch's talk in the sense that I see with great satisfaction that this plague you have called precursoritis is now effectively cured! About this question of continuity, I have the impression that a more adequate concept would be dialectic evolution, which implies both continuity and qualitative change.

I have a definite question to Prof. Murdoch, but it does not concern so much looking towards the future, towards the connection with Galileo, but looking backwards, to the connection with the Greeks. I think there is one. In Greek science one can observe the beginnings of rational thinking, which is a discovery for mankind just as it is a discovery for every child. The studies of child psychology show that until the age of about 12 the child does not think rationally—he thinks in concrete representations of sensory-motor schemes and develops a logic of his own based on manipulating those representations. I say sensory-motor with an accent on motor, because he thinks in terms of activity rather than of contemplation. The development of rational thinking in human societies is a late development which is only enforced upon society by the complexity of its organization. Now, if you look at Aristotelian physics, for instance, you find striking analogies with explanations that a child before the rational stage would give. In fact there is a case of a child giving spontaneously the anti-peristasis explanation for the motion of a projectile. On the other hand Aristotle has attempted the first analysis of logic, so I situate him in the transition period in which rational thinking was being discovered, so to speak, by the Greeks. Then there was a rapid evolution in the following Hellenistic period, which was very strongly rationalistic—with Archimedes, Euclid and also, as we have learned from Sambursky, the Stoics. But just this rationalistic trend in Greek science was, I think, practically lost in the Occident during the Middle Ages. I know there were translations of Archimedes and Euclid, but if I am not mistaken those translations were made by people who did not really understand what they were translating. Well, this is my question to you—had these translations any influence, or would you agree that the scholastics were resuming the transitional stage in which we find Aristotle?

I may give an example that struck me in Oresme—you will surely find other examples with your great knowledge of the period. Oresme

developed graphical conceptions which he obviously regarded as a great novelty, since he felt the need to defend them. There is a passage in which he tells the student that if somebody objects that it is not allowed to mix up qualities and to compare a line with a number or with some other quality, then he may reply that it was Aristotle who did so in such and such circumstances. You may remember better that passage which strikes me as evidence that this was a great novelty for him. I tried to find out whether this graphical conception arises in the spontaneous mental development of children: this is really not the case—when a child is confronted with a problem of graphical representation, it is a very difficult problem for him and he actually only solves it when he has reached the stage of rational thinking. So it seems that this graphical conception shows Oresme in the transitional stage towards rational thinking. Likewise, the insistence of the scholastics on investigating all the subtleties of logic, points in the same direction—they may have exaggerated, but that is natural when playing with a new tool: the main thing is that there was a new tool, that they were developing further than Aristotle.

MURDOCH: Let me first say something in answer to Professor Rosenfeld's remarks. Looking backward, as he suggests, there obviously is a continuity—forget for the moment Ptolemy, Archimedes and other figures, and look merely at Aristotle. Here there is appreciable continuity, particularly in the sense that no matter what "novelty" a medieval thinker may introduce, he always operates within an Aristotelian framework. Of course, there are many instances in which not only the framework but most all of the substance is Aristotelian as well. So, for example, in many of the *expositiones* or commentaries *ad litteram* of Aristotle's texts, where in spite of much interpretation of what Aristotle meant, there is a great amount of simple recasting of his arguments into a different form, of inventing new examples making the same point, and so on. On the other hand, one could argue that what I have spoken of this morning, while equally Aristotelian when it comes to framework, is less so on the substance level. If so, then perhaps we should accept Professor Rosenfeld's suggested emendation and here speak of "dialectic evolution, which implies both continuity and qualitative change." In fact, I would think that any interesting case of "continuity" in the history of thought would be of such a nature.

On reflection, one can convincingly argue this for many of the 14th

century developments in Aristotelian natural philosophy that have been the focus of my paper today and point up just what the "qualitative changes" were in the bargain. If, as an example, we go back to speeds, forces and resistances, we can, to begin with, say that, when Aristotle *implies* laws about such things or about relations from establishing a mathematical relation between *V*'s, *F*'s and *R*'s, then it is not primarily this problem that concerns him. The relations he does suggest are either being used *ad hominem* against others who might believe in an infinite universe or in infinite values within it, or who believe in the existence of the void. Alternatively, to cite the most significant occurrence of such implied "mathematical formulae"—that in Book VII of the *Physics*—what Aristotle is really concerned with (because we know that Book VII, though incomplete, was but a first draft of what he accomplishes more fully in Book VIII) is examining strengths of things that can move, strengths of agents and, in particular, of the one agent that holds center stage in the final act of the *Physics*—the Prime Mover. This is what he is worried about, and not primarily what relation of proportionality obtains between *V*, *F* and *R*.

However, soon thereafter in the Greek commentators, it is quite clear that one begins to witness the pulling out of just such a relation from Aristotle as if it were one of the major issues troubling him. These commentators begin a systemization of Aristotle that renders him much duller, much less lively, than he really is, and they begin talking in general about what one can work out relative to velocities, forces and resistances. Even merely this move constitutes, I would think, a considerable qualitative change. Without having the Greek commentators on the *Physics* (save in fragmentary form indirectly through Averroes) the medievals indulged themselves in the same change. But they went even further. Thus, when we come to Bradwardine, we have someone who focuses a whole work on the question of the proper mathematical proportionalities for velocities, forces and resistances. True, Bradwardine and his "exponential function" are still cast in an Aristotelian framework insofar as he believes himself to be working out an issue of grave concern to Aristotle and even to be revealing what he may have meant in the first place. But the fact that he buttresses all of this with much more of the mathematics of ratios and proportions, and far fewer considerations drawn from Aristotle's deliberations, marks a further qualita-

tive change. After Bradwardine there are, to be sure, those who bring his "function" to bear in their *questions* on Book VII of the *Physics*; but successors such as Swineshead edge the change one step more: the mathematical manipulation of the "function" is now the primary concern and not that of any possible relevance to Aristotle's intention or meaning. Looking backward, then, we do here have to do with an instance of Professor Rosenfeld's continuity with qualitative change.

Turning to a second question, that of the influence of the translation of Greek scientific works, I should reply, first of all, that they were not at all incompetent. As a matter of fact, those Latin translations made directly from the Greek are often the most accurate we have, far more reliable, for example, than modern translations into German, English or French, in that they frequently come close to matching the Greek word for word. Moreover, not only were these translations accurate, but there is much evidence to indicate that those who made them well understood the substance of their sources as well as the language involved.

Secondly, as far as the use to which these translations were put, two points might be made. On the one hand, both Euclid and Archimedes were appreciated and employed in the literature of medieval Latin mathematics, optics and statics. Further, the existence of this literature also means that what Professor Rosenfeld has called the "rationalistic trend in Greek science" was not lost in the Latin West, though admittedly its flourishing may have been less robust here than in Islam. (Parenthetically, I would think that the term "rationalistic" is too broad; it would, I feel, fit Aristotle as well as Hellenistic Greek science). On the other hand, Euclid and Archimedes (especially the former) were also utilized within medieval natural philosophy itself, and often by authors who had little of mathematical ingenuity about them. Here, Euclid was looked into basically for things of philosophical interest or application. Although this may have meant that Euclid was being appreciated in a manner quite unlike that of a Pappus or a Newton, it was still putting him to use, howsoever scholastic and medieval it may have been.

Thus, the translations did have an influence. Yet I would not agree that the scholastics were, as Professor Rosenfeld suggests, "resuming the transitional stage" of Aristotle. I say this not at all because the translations did have an influence, but rather because, in the first place, I think it misleading to view Aristotle as somehow transitional to Euclid

and Archimedes. Secondly, and more importantly, my agreement with Professor Rosenfeld that the scholastics I have been speaking of represent an Aristotelianism that has undergone qualitative change would suggest, I think, that they had moved beyond their mentor. They were at least frequently drastically different from him.

Finally, one other point about why I feel this "transition stage" characterization to be inadequate can be made by speaking to Professor Rosenfeld's example of Oresme. To begin with, Oresme was not nearly so much a novelty as suggested. He was part and parcel, I believe, of the whole "measurement preoccupied tradition" I have treated this morning and, in chronological terms, came after Oxford-based activity in this tradition. Thus, I think any impression of novelty he may give can be misleading; in this regard, his defense of what he is doing can best be explained not in terms of his having begun something crucially new, but rather in terms of there being competing theories in the 14th century relative to what rules and techniques one should apply within the measurement tradition. One can, for example, find the same defensiveness within debates about the intension and remission of forms which have nothing to do with Oresme. Finally, however novel Oresme may or may not be, I feel that it would be erroneous to regard him, or the whole measurement tradition in the 14th century, as *fundamentally* some kind of "transitional stage toward rational thinking," that is, I take it, toward the science of (say) Galileo and Descartes. True, there are isolated elements within this tradition that have relevance, even application, within early modern science, but one of the points I have tried to urge today is that, whatever partial continuities there may have been in this direction, the whole tenor of the measurement tradition in the late Middle Ages is quite different from that found in Galileo and his successors, so different, in fact, that it would be wrong-headed to regard the former as basically transitional to the latter.

This leads me naturally, I suppose, to Stephen Toulmin's remarks about continuity vs. discontinuity in the forward now, rather than backward direction. Well, perhaps Stephen is correct in claiming that I did not explicitly clamor for discontinuity between the medieval and the early modern. Nonetheless, if I am forced to give my view with regard to this problem and am not allowed the comfort of qualifications, then I would opt for discontinuity. This follows, I think, from a quite dif-

ferent overall impression one derives from reading the relevant 14th century texts and, for example, Galileo and Descartes; I would generalize, that is, what I have said of the distinct tenor of the late medieval measurement tradition to cover 14th century natural philosophy as a whole. Still, allowed to stand alone, this is a terrible over-simplification of the facts and not an especially informative one at that. It seems to me that qualifications must be allowed if one is to reach an even half-adequate answer to the issue. This permitted, then one's answer varies, it seems to me, with the specific continuity-discontinuity question being put. In a way, some of this is, I think, implicit in my paper. But I had better render things more explicit, even extend them.

Thus, if one approaches the problem by trying to answer the question: What would Galileo most likely have seen in the medieval tracts *de motu?*, then some continuity does have to be allowed. Continuity in the sense that at least some notions found in these tracts were used (in a variant form, no matter) by Galileo. This is essentially Claggett's point. He would claim—in fact, has claimed in reacting to a draft of my paper—that "vertical history" still has its place, and that this is one instance of it. My stance on the matter is, to repeat myself, not that continuity does not exist in this sense, but that it is wrong to leave things at that. Medieval science needs (indeed, is more in need of, I would say) some "horizontal" history as well.

Secondly, then. If one looks to this "horizontal" history (or at least to the fragment of it I have tried to sketch in my paper), then one should come down on the side of discontinuity. For, to apply a modern version of the language often used (wrongly, of course) in the Renaissance to disparage scholastics, the logic-chopping, hair-splitting, measure-mania or what have you, that you find in Heytesbury, Swineshead and Oresme (and there are many other minor figures who are similar) is not, I think, continuous in any proper sense with the scientific, or even philosophical, activity of the 17th century. In other words, I don't believe that reading (say) Descartes will lead you to a belief to the effect that: yes, he is extending the same kind of thing Swineshead was doing. No, I don't think so. I think Stephen would agree with this discontinuity.

On the other hand, Stephen feels that what I have said does imply a continuity between the Oxford and Paris people and something in the 17th century. I don't feel that the kind of specific continuity that Claggett

has brought out is what he is thinking of. There is, however, another kind of continuity that might be seen as latent (far too latent, I fear) in what I have said this morning. It is there in spite of the kind of discontinuity I have just urged; it is, I think, even quite consistent with it. The precise nature of this other continuity is in need of a great deal more work in order to be delineated properly, but from what Stephen has said of the "conceptual-analysis element" in the 17th century, I suspect that it may be this that he has in mind. It is, first of all, a continuity on the methodological, philosophical, or perhaps better, epistemological and logical levels. By this I do not mean a continuity based on some common thread of a developing experimental method (as Crombie, Randall and others have stressed). It consists, rather, in all of those conceptions and methods that made 14th century philosophy as a whole (not natural philosophy alone) break with the philosophical attitudes dominant in the 13th century and, at the same time, establish a form, a framework, in which philosophizing in the 17th and 18th centuries was to be cast. To put it in terms used by Ernest Moody to describe this phenomenon: Philosophical activity in the 13th century was (following much in its Greek and, especially, Islamic forebears) fundamentally cosmological and speculative; in the 14th century it became essentially critical and analytic. Moody had in mind in particular the late medieval logical and epistemological criticism of metaphysics, but I believe that the same transformation holds for natural philosophy (or science) as well. In any event, this new critical, analytic attitude, together with the new kinds of questions it gave rise to, are factors that point in the direction of early modern philosophy and science. Perhaps this is what was on the edge of Stephen's mind.

Of course, all of these considerations in no way begin to exhaust the variables in the continuity-discontinuity issue. I have not, for example, made even the slightest pry into matters of external, non-literary factors (which would be, I think, to switch our deliberations to a quite different level of cause and effect). Nor have I paid special heed to the problem of the continuity of particular trends or doctrines. Here an incredible amount of further excavating and examining of the sources remains to be done. Consider again the whole tradition of Aristotelianism. If it is true that you move from Aristotle's Aristotle to a systematized Aristotle in late antiquity, so it also seems true that this systematization is altered

in Islam and again (more than once) in the medieval West. But there is perhaps even another Aristotle: that of Galileo's dialogues. And no one has really begun to distinguish any of these Aristotles. Yet until we do so, how are we going to resolve even this particular, single track problem of continuity-discontinuity?

ROSENFELD: The point that I tried to make is that while Aristotle in the Middle Ages is not the same as the real Aristotle, he still has corresponding features. That is what I call a dialectic evolution, in which you reconcile a continuous transmission of elements with the fact that they nevertheless appear transformed at a higher level, after certain features have been rejected.

MURDOCH: All right, so they are rejecting certain things in Aristotle. Which means, it seems to me, that their Aristotle has a different approach, a different attitude, to problems than does Aristotle's Aristotle.

PINES: It happened to every philosopher who has been systematized.

ELKANA: I think we should discuss discontinuity versus continuity somewhat more, and I would like to suggest a formulation and ask the medievalists if it would fit the picture they are describing. With all the changes in conceptions of science there is something common no doubt to Bradwardine and Planck—this in a sense is realized. It seems to me that what makes a discontinuous impression on us is the following: any scholar who is interested at any level in any period in the physical world is also interested in philosophical problems, but the kind of philosophical problems with which he is dealing changes from period to period. That is, the "scientist" in the Middle Ages was interested in rational theological problems and coupled them with his questions about the physical world. The typical "scientist" in the 19th century coupled his interest in science mostly with epistemological problems. And we might be moving nowadays towards that kind of scientist who typically and self-evidently combines ethical problems with his questions about the world. At present the typical brilliant scientists claim to be anti-philosophical. If it lasts for long I don't think it will be very fruitful for the development of science—but this too is a philosophical view. In other words, my question is: would it suit your picture of the Middle Ages to say that what is discontinuous with the 16th century and 17th century is really the kind of philosophical problems with which their interest in the physical world around us is combined?

MURDOCH: Your suggestion is most attractive, but not one that will allow an especially comfortable fit, I think, with the Middle Ages, at least not with the 14th century developments I have been concentrating on. First, it won't do to pass off the medieval "scientist" as being interested simply in rational theological problems. He was just as preoccupied with epistemological and logical issues. So I don't think that we can base any particularly striking discontinuity on these grounds alone. But more to the crux of the matter, your analysis separates "science" from philosophical interests in a way that will not wash for the kind of toiler in *scientia de motu* I have been talking about. To put it another way, I don't think it would be very meaningful to ask Richard Swineshead: What kind of philosophical problems are you coupling your interest in nature with? It would be better should we merely say that he, and others like him, were just working continuously within the faculty of arts tradition which, in the 14th century, placed primary emphasis upon logical and epistemological matters, and that out of this somehow—I do not know precisely how—there suddenly arose his and others' interest in the measurement tradition.

ELKANA: It certainly does not proceed like that—yet in natural philosophy, there is less difference between the *V*'s and *R*'s of the 14th century, and the masses, forces, etc. of the 17th, 18th and 19th century than there is between their views on nature, God and perception and those of the post-17th century. It is always one natural philosophy with some elements which you cannot disentangle easily, but in some of these elements there is much less change than in the others.

MURDOCH: No, I think not. For what they meant by these *V*'s, *F*'s, and *R*'s was really quite different, quite a great deal broader, than their meanings in the 17th century. They were just as willing to have them involve the variables of qualitative and quantitative change as those of local motion, just at home with allowing them to represent the relative powers within, and effects of, a given medicine, and so on. It is, then, we who make these conceptions in the 14th century more or less continuous with them in the 17th by a kind of picking and choosing that is directed by our 17th century view of things. This is but a particular instance of the point I have been trying to make.

TOULMIN: Surely the problem here is—to talk about coupling is to make the mistake that John was criticizing—that we can make this

distinction between "philosophy" and "science" clearly, in the way in which Yehuda is applying it, only *after* there has been a certain differentiation.

MENDELSON: Stop using the word "coupling" and use the word "content." What is the content of the philosophy at different periods and I think this is what Yehuda really means.

TOULMIN: The discovery that there are sub-classes of content which can be dealt with in some particular isolated way—this is a discovery which *has to be made*. One might argue that this is one thing which was done in the 17th century: that people discovered specific areas where particular methods of analysis could fruitfully be pushed much further.

PINES: In Islam there were many mathematicians or astronomers who were only mathematicians and astronomers, who as far as we know were not interested at all in philosophical problems—there is a big literature which only deals with astronomy and mathematics. I think this may be different in medieval Europe.

MURDOCH: Though not as extensive, there is the same kind of mathematical and astronomical literature in the West. But I have all along, both in my paper and now, excluded this from the equation of philosophy and science that I have urged. What is more, the problem of continuity vs. discontinuity that I am now talking about—and about which Duhem, Clagett and others have spoken for that matter—is one relative to Galileo, or better relative to what is usually regarded as mechanics in Galileo. This is what I am really addressing myself to.

Now, is there anyone in the 14th century who does what we would call kinematics or dynamics—forget about statics—who does not automatically couple? Or better, who does not even have to bother to couple since he is doing philosophy in the very doing of his so-called kinematics and dynamics? Very few, I would maintain.

AGASSI: The question of putting continuity and discontinuity and novelty in context I think is worth pursuing. The theory of discontinuity is the traditional philosophy of science introduced by Bacon. According to it the work of Galileo was declared to be a novelty, not because he did not know about his predecessors, or think about them, but because he did not depend on them—his results, we are told, came all from experience. And this goes even further—not only in science but in philosophy generally. When Husserl in his *Cartesian Meditations* criticizes

Descartes, he says Descartes was influenced by the scholastics and expresses the view that he himself has no predecessor.

The continuity theory says, we *are* all indebted to our predecessors for *almost* all of our views. Thus, Rupert Hall has a footnote somewhere where he says that according to Duhem the 14th century is a dress rehearsal for the 17th century. The situation that Duhem wanted to present was just that—not merely to show that there were precursors, but that there cannot be an independence of one generation of another. Prof. Murdoch claimed that according to Duhem in essence the 14th century led the 17th century; but Duhem was against essences, and merely tried to show that views progress by constant tinkering. But Duhem overdid his case. He had to convince people that influences were important so he understandably overstated his case—even though he rejected the theory of essences. Contrary to it he declared that in science a small change here, a small change there makes it impossible to say where our essence resides. This is why continuity is essential, because what every scientist does is take a corpus of science as it is and make a slight change, with the accent on slight. That is why the continuity theory is that which declares that no great change occurs at any period.

I would say a bit more. What theory we have about continuity or discontinuity is, as you know, something which is open to rational debate, but you John started your paper with a very strong remark which impressed me greatly—you said we must, as historians, show empathy to those people and see the world as they see it, not from the viewpoint of their successors, and especially you later said we should not be critical of them. Now I agree with the suggestion that we should empathize with them and should not be critical of them, in the sense that we know they are mistaken and this is really trivial. It is trivial to be critical of even 17th century thinkers and surely of 14th century thinkers. Likewise, just to show where they are right—to show sympathy with them in this manner—is ridiculous and not very interesting. But when you say we must show sympathy, it is a bit constraining. I still retain my status as amateur so the “must” does not hold for me, but I still think it is not a matter of obligation. There is a matter of obligation here when the historian forgets to say that a topic, let us say the possible rotation of the Earth, occupies in the 14th century a very minor position and he blows it up deceiving his readers—that is a must, he should not do that, at least he should not

knowingly deceive his readers. But it is not a matter otherwise of having to follow this idea or that.

A historian has his reading of his material. Now, if he gives us the rules which govern his reading, his criteria of historiography, and he presents the 14th century in his own way, then he has been as frank as he should be. What you (Murdoch) resent is that people conceal their historiography and give a false impression of what the original literature looks like to people like me who don't look at the original literature because of their human limitations.

So, I really just wanted to plead for more tolerance in this matter.

JAMMER: I am afraid that any discussion on continuity versus discontinuity would be doomed to failure as long as we don't apply strict criteria to these conceptions, and being afflicted with precursoritis I should mention those famous discussions between Anneliese Maier, Clagett and other scholars concerning the continuity between impetus and momentum or early medieval formulations of the Law of Inertia and Descartes, Newton etc.

Now in mathematics we have a strict exact definition of what continuity is—I hope we have it in spite of all those problems behind the idea of the continuum. In the history of science we don't have—in fact we have two extremes. Somebody, I don't recall who, once defined a historian of science as a person who tries to prove that nothing new ever happened because he always could find some precursor. At the other extreme you have historians of science who see in every scientist a revolutionary.

Now the criteria, I think, we have to apply in order to clarify this issue might be taken either from the conceptual context, or on documentary evidence. For instance, in the conceptual background of those formulae—Bradwardine, Swineshead, the “calculatores,” etc. etc.—they all still keep to the Aristotelian dictum that you can't compare or establish proportions or ratios between heterogeneous physical quantities—they have to be in modern terms of the same physical dimensions. With Galileo it changes, but they still keep to that. And I think that is a point which can be used as a definition of this continuity. While one might suggest other criteria.

Now coming to the documentary evidence, I think the issue of continuity versus discontinuity should not be looked at only from our point

of view as we see it, but one should really find whether some statements of those medieval scholars had a personal effect on a later scholar or scientist, and even if these statements are not emphasized particularly in the context of their writing—an isolated statement sometimes had such a personal effect. Because then I would say that on a psychological basis there is continuity, and I might even say conceptually too, but this perhaps conceptually not so much as psychologically.

Now a very short remark on another issue which has been brought up here—philosophy versus science. I might be wrong but I think that this issue should also be dealt with on the same basis of finding exact criteria—as long as you don't have any criteria you can always say this is science and that is philosophy or vice versa, etc.

I would very much appreciate hearing any remarks on that simple criterion which I might propose: as long as any idea (whether it belongs strictly to science is quite irrelevant at present—let me say for instance Mach's principle), has not found a strict mathematical formulation, it belongs to philosophy. Any statement which has not been quantified is philosophy. As soon as the statement has found some quantification, so to say, in a very general sense, it belongs to science.

Well I leave it open—I just want to inject a point for discussion.

ROSENFELD: I still cannot defend myself from the impression that Oresme regarded the graphical method as a great novelty and I imagine that you will tell me that whoever was the first—some obscure Italian—did also, but it is possible that he did not, because the first inventor may not always realize the importance of an invention. I was very struck by this particular passage of Oresme in which he insists on the fact that he is doing something which one might think is impermissible, as Prof. Jammer says—that it was a sin against the homogeneity of quantities. But then he had found some passage in Aristotle—one can find almost everything when one looks carefully enough—in which Aristotle did the same thing, and in fact Euclid himself compares areas with lines and so on. That was one point.

But now I wanted to raise another point: John Murdoch suggests that he does not know why they were so fond of considering measurements and measuring things. I think there is a good reason for that, namely, that at that time the problem of measurement became an acute social problem owing to the expansion of trade. And again you find evi-

dence for that, documentary evidence, in Oresme—in the preambles to his translations of various books. For instance in one of these preambles he explains why he gives a French translation of this book instead of a Latin one. He says there are many people, intelligent and ingenious, who do not know Latin: obviously he is thinking of the merchants—he was helping the king in the difficult task of reconstructing the French economy that was completely wrecked by the Hundred Years' War.

So I think that when you insist, quite rightly, on considering the medieval activity on its own, without looking at the future which they obviously could not know, nevertheless this insistence on philosophical considerations should not be the only one, especially for the 14th century: there were other practical considerations which are also enumerated by Oresme. I quite agree with you these practical motives were so weak that the main emphasis was indeed of a methodological character—what I call playing with a new tool, namely this development of logical thinking in continuation of Aristotle's endeavor. But one must not neglect on that account the influence of the social environment from which the scholastics were not so secluded at that time as later, in the 15th century. And so I would then raise the problem which I think is an important historical problem: Why was this hopeful start of science as we know it abortive? Why did Oresme's graphical method, for instance, degenerate into the extravagances of the "calculatores" of the 15th century? And why did not modern science start from there? Now this is a challenge!

MURDOCH: To begin with, your suggestion that the late medieval interest in measurement is somehow related to socio-economic factors—I must say that I am sceptical of that, sceptical that it is related in any way at all to such factors. The suggestion that I made in my paper, namely that the preoccupation with measurement is in some way an outgrowth of concern with logical problems and logical techniques, is infinitely more reasonable, I think; the connections are there in the sense that the same individuals are involved in both phenomena, and it has the added advantage of fitting well with a host of other 14th century attitudes within philosophy.

Your second question returns us to the whole issue of continuity vs. discontinuity. To ask why the "hopeful start" of science in the 14th century (meaning by this the measurement tradition) was abortive, is to

ignore the discontinuity that I have claimed to exist between this tradition as a whole and Galileo. Swineshead's or Oresme's preoccupation with measure properly understood, for example, does not even point in the direction of Galileo; that is, no one could have remained faithful to the essentials of the measurement tradition and at the same time have developed it into anything like the *Two New Sciences*. Of course, one could say that a few elements within that tradition were, shorn of the tradition itself, used by Galileo. That is Clagett's point about continuity. But this speaks, I think, not to anything in the tradition functioning as a "start" of modern science in the sense that it was in itself somehow "logically productive" of Galilean results; it is more about what a particular individual named Galileo could, given other conceptions and attitudes of his own, make of, or do with, several elements in that tradition.

HIEBERT: I have difficulties with some of the historiographic suppositions here. Perhaps my own feelings would be closer to what I know of Prof. Sambursky's works, and also the paper of Prof. Pines.

It seems to me that a kind of myopic concentration on an attempt to reveal the whole story, as you put it, is probably not the right way to look at these things and it leads you to the kind of statements that you made in saying that you should disabuse yourself of science, for example. I don't know why you should say that. Is the failure to disabuse yourself of science any more dangerous than the failure to disabuse yourself of philosophy, or mathematics, or logic, or psychology, or theology? It seems to me that by singling out science as something which we should disabuse ourselves of, you are somehow saying that there is a continuity somewhere within the philosophical tradition—I don't know where. But the approach that I would like to see is for people who have scientific competence, who have philosophical competence and who have knowledge about visual thinking and about experiments on children and about technology, and psychology of religion and so forth, to ask interesting questions about these things and hope that the illumination that comes from asking interesting questions, even from a modern bias like in our own physicists, will shed some light upon these things.

So I felt very strongly that to disabuse yourself of even the most modern conceptions in physics is not giving yourself a fair chance of raising significant issues, philosophically significant issues that may somehow shed light upon what you want to show. Your intention is to show a whole story somehow.

MURDOCH: My suggestion that we disabuse ourselves of science (and the scientific revolution), in this particular instance of examining 14th century physics or what have you, was conditioned by the recent historiography of the subject. Science and the scientific revolution *have* been used to "shed some light" (to use your phrase) upon the 14th century. I am now suggesting that we adopt another tack and shed a different light, indeed much more light, on the subject. Is that so objectionable?

As far as your second point, your plea for using a variety of tools and approaches, is concerned, you have my total support. By all means, not only use science, but anything you wish in looking at history. If you want to, for instance, you might axiomatize Aristotle, even attempt to translate parts of him into the language of Whitehead and Russell's *Principia Mathematica*; but remember what you have done, that you have axiomatized him, that you have so "translated" him and then ask yourself: Well now, what have I changed in Aristotle, if anything? (Parenthetically, the moral to be drawn from this rather outlandish example is that in such a case one would undoubtedly be aware of the necessity of asking this question; but if it is merely some part of modern science that one is using in one's "translation," then in most instances one does not, indeed one has not, bothered to ask how Aristotle, or whoever, might have been changed. But the question should be asked in such a case no less than in cases of more fanciful translations.)

But to return to the value of allowing the variety of approaches and "translations" you, and I, would urge, and in the bargain to the value of using philosophy of science in general; I think that one of the major points that needs to be made is that employing such a battery of tools might lead historians of science to see things that they would not have otherwise. To return to the same example, if one axiomatizes parts of Aristotle's *Posterior Analytics* (as some historians have done), then one should follow this (as the same historians have seldom done with enough care) with the further observation: Aha! Now Aristotle really does not have a postulate of reference or a postulate of verifiability in precisely *our* sense of these terms—but he does have something very similar, let us see what it is. And you might not have noted this point unless you had used your modern tool.

To conclude, however, I should make one final point. Although I am at one with you, Erwin, in pleading for the use and value of this kit-bag full of varied historical tools, it does not follow from this that

all, or any particular one, of them must be applied by any one historian or applied to any one topic. Thus, the "science tool" might be permitted to be absent, or better to be reduced to a far less dominant role, in the particular topic I have had in mind.

MENDELSON: If I could just push a question right here because I think the exchange has brought out a problem that continually comes up in the history of philosophy and science. Erwin Hiebert suggested this broad use of a whole armory of tools some of which may be more or less useful than others and every one around the table will have his choice. The question which comes to my mind each time something like this is put out, is "toward what end"? Where are you going with it?

HIEBERT: That's what bothers me—I don't know that I want to know the whole story. It reminds me of the preface of Carl Sandburg's biography of Lincoln in which he says if the whole story were to be told it would take longer to tell it than it took to enact it in history, and if I look at my own field of the 19th and 20th century where my interests lie, you know to tell the whole story is just nonsense—you choose interesting problems and you try to illuminate those problems but to tell what really happened—somehow I sort of lose interest in that kind of thing.

MURDOCH: But look, Erwin, obviously we are never going to be able to tell the whole story in the sense you have in mind of exhausting absolutely all extant sources. Still, I do not think that you would wish to tell a partial story that was, in your view, incomplete in an important way, even apt to mislead. In any event, you would welcome supplementing such a story. That is what I am urging: A more complete story in the sense of being more representative of what doing *scientia de motu* was all about in the 14th century.

MENDELSON: How do you introduce a calculus of judgment then? Where is your point of choice as to which story to examine and which to ignore? I mean this is really what John was after this morning, was it not?

LOWE: While welcoming the general approach to medieval mechanics urged by Prof. Murdoch, I would like to raise a query about the term "rule testing" used by him. It seems that in some of the instances that he cited nothing was being tested, and in other instances one needs to make clear exactly what is being tested. To explain what I mean: con-

sider a geometry textbook. In it one will find firstly "axioms" and "definitions," and then things called "theorems" which are generally derivations of special results from the given axioms and definitions. Here and there amongst the theorems one finds what are called "problems," which have the form "to find the area of this" or "to find such-and-such a point" or something of the kind.

Now, in the case of deriving theorems it does not seem to me that anything is being tested at all—except perhaps the *adequacy* of the given axiom system for the derivation of geometrical assertions whose truth is already recognized. In the case of "problems" the adequacy of the axioms may also be under test; but normally the problems are not presented in that spirit—they are presented as things for the pupil to try out his wits on, to see if he can use the results that have been already proved for him, so that what is being tested, if anything, is the ingenuity of the pupil. In either case—theorems or problems—if anything is being tested it is certainly not the truth of the axioms (which are traditionally supposed to be self-evident), or the truth of the theorems and solutions to the problems (which are supposed to follow from the initial assumptions with logical rigor).

To come to what Prof. Murdoch cites from Bradwardine and other people, it seems to me that what they are doing is quite comparable. First of all there are a lot of special rules that they derive: how you combine quantities of different kinds and different varieties and so on, to get special results, and this seems to be analogous to deriving theorems in geometry, namely nothing in particular is being tested—one is simply looking for further consequences and special applications of principles one has already assumed. Or if anything is being tested, it is simply the adequacy of the axioms to cover the given field of investigation.

Then they introduce these curiosities such as dropping things down a hole through the centre of the earth, where quite clearly there is no question of empirical testing for truth being involved, and here I think in some cases they may have been trying to test the adequacy—though not the truth—of their assumptions, namely could they give an answer at all to such a problem. But on the other hand, I think in many cases they were simply assuming that their proposals about the nature of motion were sufficient to solve these problems, and seeing whether their ingenuity was in fact up to finding an answer using principles which

they were fairly sure were sufficient. These, therefore, correspond to the "problems" of the geometry textbook.

It is mistaken to describe this activity as a whole as "rule testing," because at least some of the time there seems to be no intention to test anything, but rather simply to derive more and more results even in rather boring profusion. Where testing is involved, it is misleading to describe it as "rule testing" without qualification. To a modern audience, talk of scientific "rule testing" without qualification suggests "testing for truth" or even "testing experimentally for truth." However, it seems clear that when Bradwardine and his contemporaries are doing any testing, they are not testing for truth, and certainly not experimentally, but they are either testing assumptions for adequacy, or they are testing their own ingenuity—when one cannot use the term "rule testing" at all, since it is not the rules but their *authors* that are under test. So as I said initially, sometimes nothing is being tested, and elsewhere one needs to make clear what (or even who) is being tested and in what respect.

Moreover, this activity does not seem to be either something of a new kind, or even very un-Aristotelian. To the extent that Euclid's geometry exhibits an approach advocated in the *Posterior Analytics*, so also do these medieval treatments of motion. It has of course often been remarked that Aristotle hardly ever follows the method of the *Posterior Analytics* himself. To this it has also often been answered that Aristotle has two methodologies of science: that of the *Posterior Analytics* and that of the *Topics*. Moreover, he says explicitly that only the approach of the *Topics* can be used when one is searching for the first principles of a science, whereas the deductive method applies when one already has the first principles and is seeking to develop the science from them. Now, in his own treatment of motion it is generally the first principles alone that Aristotle is looking for—he is trying to find definitions of motion, place, etc. which are what correspond in a treatment of motion to the definitions of the point, straight line, etc. which Euclid places at the beginning of his geometry. But if Aristotle had found suitable axioms to add to his definitions of basic concepts involved in motion, he would presumably have wanted to develop the science in the manner of the *Posterior Analytics*, in other words after the fashion of a geometry textbook, which is more or less what the medieval writers on motion appear to have done—quite in the Aristotelian spirit.

MURDOCH: To be sure, you don't *have to* regard it as rule testing—indeed, somewhere in the paper I have allowed the description of this activity as the implication of corollary-rules or the multiplication of variant cases as well. Secondly, your description of what I have called "rule testing" as essentially the derivation of theorems from a given set of assumptions or axioms of course accounts for the *logic* of what they were about in each case, but it omits a number of features that are important, I think, if one is to gain an idea of the intent and character of the activity in question. What I mean to say is that it is extremely instructive to note, for example, that the added cases or "theorems" that were derived were often what would be considered, from the mathematical point of view, minor variations of a single proposition and, more significantly, that what was derived was frequently posed in the guise of resolving a sophism. Similarly, to describe the activity as a kind of "rule testing" is also, I feel, revealing of their attitude and approach. To begin with, your analogy with a geometrical textbook can be misleading. For the scholastic would also have regarded the axioms in such a text as self-evident or *per se nota*. But the basic rules of measure that assume the logical role of "axioms" in late medieval *scientia de motu* were not that; for there was frequently a choice made with respect to which basic rule they "developed." Thus, for example, one could either decide to measure the speed of bodies undergoing rotary motion by their fastest moving point (the Mertonian choice) or by the speed of a point midway between the center and the outermost extremity. Whichever alternative was chosen, derivation of the "consequences" can properly be seen as testing the mettle, as I have put it, of the basic rule so chosen. If you so wish, you can interpret this as "testing the adequacy," as you would prefer, but of the single basic rule at issue and not of something like an axiom system; for they devoted their attention only to the basic rule and not to the other various assumptions they made in deriving the added variant cases, or what have you.

There is, however, another reason, perhaps an even more important one, why I still believe "rule testing" (shorn, to be sure, of any connotation of an empirical test) is an appropriate characterization of what they were doing. It all has to do with the parallel I drew between Swineshead and Abelard. In drawing the parallel I had in mind the fact that Swineshead bears a greater resemblance to Abelard than he does to Copernicus

or Galileo, but I would expand this and say that, in doing whatever he did with Bradwardine's basic rule, Swineshead was also closer to Abelard than he was to (say) Euclid. Swineshead's acceptance of Bradwardine's rule corresponds, for example, to Abelard's formulating a theory of universals and then saying, well now, this is how one appropriately explains the universal term "rose," but now let us see if my theory can also take into account the situation represented by the (assumed to be true) proposition: "There are no roses." This latter move by Abelard is, I would urge, parallel to Swineshead's activity in deriving his fifty-odd rules. And just as Abelard can be interpreted (appropriately, I would claim) as testing the applicability of his theory of universals in making his move, to Swineshead can be seen as testing the power of Bradwardine's rule. From a slightly different point of view, confirming the equation of science and natural philosophy that I have taken as a theme, we can say that Swineshead's activity was (structurally, not in terms of content) not new, not because it bears and his scholastic commentators on the *Posterior Analytics*, but rather because he was doing what was traditionally done with philosophical theories or *positiones*: establishing or testing their applicability. The fact that he uses a good deal of mathematics, while most philosophers did not, and could not, should not mislead us.

LOWE: It seems that a lot of the things that you are treating can't be described in this way. When you say they go to great lengths to show how you can combine several different qualities of different kinds of uniformity and deformity and so on—it seems to me this is simply deriving further consequences.

MURDOCH: I think not. Once again, to take Swineshead as an example of this kind of "combining," the very structure of the treatises in question in his *Liber calculationum* would, I feel, lend credence to my description. For in each instance, as he moves from measuring one quality to more than one and from consideration of simple intensity to those of combined intensity and extension, he begins his discussion by setting down a plurality of *opiniones* as to how one should measure the combination in question. He then, *opinio* by *opinio*, by running through a battery of variant cases, shows what will obtain in each instance, and ends by showing that one among these *opiniones* is preferable. It is, in a few words, preferable because it accounts for or explains the same variant cases (and often a few more to boot) with greater efficiency or

adequacy than do the other *opiniones*. To describe all of this *merely* as the deriving of further consequences would, I think, leave a good deal undescribed.

TOULMIN: I want very quickly to deflect the discussion in Prof. Pines' direction. The discussion he gave of the analogies between the traditional Aristotelian criticism of the Platonic doctrines about space, time and motion, and Kant's new attempt to produce a reconciliation between Newton and Leibniz was extremely illuminating; but what it shows up here is the fact that, even after you have made these comparisons between one issue in one epoch and another issue in another epoch, what appeared as a defect at one stage may at a later stage be turned to advantage and made a merit. If I understood Prof. Pines correctly, he is saying that what traditional Aristotelians regarded as a terrible defect in the Platonic approach to the concepts of space, time and motion is—perhaps unknowingly—embraced, made a merit of, and then used as a principle, namely the new conception of the function of physical theory as the product of the "critical imagination." The word "imagination," in the sense of "imaginary," had always previously been dyslogistic: but now it becomes eulogistic. As we now say, the mind creates theories, and space and time are the framework or scaffolding within which we build our physical theories.

This of course raises the important question, how can an argument which is regarded as a fundamental criticism in one epoch be turned to advantage and made the constructive basis for philosophy in a new vein. Is this a point?

PINES: Yes, I think this is one point I want to make. In other words, it comes together with Kant's critique of metaphysics—the traditional conception of metaphysics which was, according to Kant, impossible, and also with his conception of the role of mind—that is to say the objective science as science based on the work and structure of man's mind which includes imagination and several other factors. He insists on the role of imagination in the first edition of the Critique of Pure Reason, but he rather plays it down or cuts it out in the second edition.

But still, as you said, it is an attempt at reconciling Leibniz and Newton and what I find striking about it is that it does produce a Resar torian framework—what is negative is positive and what is positive is negative.

NEWTON'S 1672 OPTICAL CONTROVERSIES:
A STUDY IN THE GRAMMAR OF SCIENTIFIC DISSENT

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"Evidence that the premises from which demonstration proceeds are necessary may be found in the fact that the way in which we raise objections against a professed demonstration is by saying 'it is not *necessary*' " . . .

Aristotle, *Posterior Analytics*, 74 b 18

0. THE INTERPRETATION OF NEWTON'S OPTICAL CONTROVERSIES

The customary interpretation of Newton's controversies following his 1672 paper, which involved Hooke, Pardies and Huygens, usually centers on problems of optical theory. Thus, it was believed that at the root of these heated correspondences were such irreconcilable conceptions as particle vs. wave and original colors vs. color as modification. (The first pair in references [18], [19] and [25] appended to this paper, the second in [24] and in most standard histories.)

Even when broader issues were seen to be involved, such as mechanistic vs. Aristotelian modes of explanation, this was firmly connected and tied with some technical problem of optical theory and mechanical philosophy, (as, for example, in [22], [23]).

It is the purpose of the present paper to propose an interpretation of Newton's controversies in 1672 that concentrates on the problem of legitimate modes of writing science. I suggest that this was a problem that played no small part in initially giving rise to the controversies and in helping to maintain them to their unsatisfactory end. A neglect of this side of the controversies by historians has given rise to some false assertions and curious misrepresentations concerning the intellectual and scientific potentials of such geniuses as Hooke and Huygens. Moreover, the present suggestion will help to connect these scientific debates with

a major intellectual trend which spread over Europe and was one of the ideological foundations of the Royal Society—namely, the trend of scientific skepticism.

Yet, my main object will not be so much to prove the existence of a new factor in the dispute that ensued, as to point out the structure, almost the linguistic structure, into which the criticism of Newton's paper was moulded by the critics. Thus, I am far from asserting that this structure really reveals the *true motives* of the critics. Such true motives are no doubt important for the historian, but I wish to suggest that a study of the form into which critical arguments were put, or the logical gambits used in propounding them, may be of an immense use in throwing some new light on old views, as well as to suggest new clusters of fruitful problems.

1. THE DOGMATIC EXPRESSION OF NEWTON'S REVOLUTION

The first paragraph of Newton's 1672 optical paper announced the need for a revolution in optics. Three pages later, the revolution was performed, again in a single short paragraph. The rest of the paper carefully explained that all was over: The revolution was complete.

The whole thing had the air of a juggler's act. Its formal and logical structure so closely and perfectly resembled those of a scientific revolution. The principles of accepted optics, Newton pointed out, predicted a certain experimental result, but the actual result was entirely different. Hence, something was wrong with those principles, and they had to be replaced. Moreover, he pointed out exactly which assumption was wrong and replaced it by a new one, which yielded true predictions. Though the logical structure of his argument was classical for a scientific revolution, still the situation had none of the social earmarks of a revolution; for nobody else was aware that anything was wrong in optics; no need for a reconstruction was ever felt. The phenomenon which Newton pointed out was a commonplace phenomenon, which never aroused any curiosity since it was believed to be in accord with the laws of optics. Newton's first paragraph already contained an undisguised criticism of contemporary authorities on optics:

"It was at first a very pleasing diversion to view the vivid and intense colors produced thereby; but after a while applying myself to consider them more circumspectly, I became

surprised to see them in an *oblong* form; which according to the received laws of refraction, I expected should have been *circular*." ([15], p. 92)

This was bad form. One doesn't just walk in, announce a fundamental inconsistency in accepted scientific beliefs, declare the need for a revolution, perform it, and walk out. Things are simply not done this way.

And to think that it all hinged on one simple observation—hardly an experiment—and one even simpler argument: Pass a white ray from a circular point source through a prism, and put it at a position of minimum deviation. The experiment is over. Now the argument: If every part of the incident ray is equally refrangible, then in this position the refracted image must be geometrically similar to the shape of the source, that is, it must be circular. But the experiment shows the image to be non-circular. Hence, not every part of the incident white ray is equally refrangible. Since customary optics operates under the assumption of equal refrangibility, customary optics is false and must be replaced by a new optics, which assumes that white light does not consist of equally refrangible rays.

The actual argument was far more complicated than the one which Newton chose to make public and which looked so innocent, but it was no less rigorous. Every single point was taken care of and separately proved: that the stationary position which the refracted beam reaches indicates minimum deviation; that in this position the mean refraction is equal to the mean incidence; that in this position the alternate incidence and refraction angles of the two extreme rays defining the pencil are equal; that, therefore, the angles of divergence of this pencil at incidence and refraction are equal, and, as a consequence, that the refracted image must be geometrically similar to the form of the light-source.

All this was proved, and even prepared for publication, in Newton's *Lectiones Opticae*. But none of the worthies who read the paper knew of this massive mathematical basis, and Newton hardly even hinted at its existence. As a consequence, though the general argument was eventually understood, the most important thing in the paper came to be looked upon as a curiosity. It was the vision Newton had of his paper as realization of a new aim in natural philosophy, namely mathematization of scientific argumentation.

Nothing very explicit could be read in the paper concerning this vision, since Oldenburg saw to it that the relevant passage be omitted on publication. The scaffoldings removed, the mathematical ones by Newton and the ideological ones by Oldenburg, what remained had a clean, indisputable and finished look. As a result, it was formulated in a way which no sober scientist of the time could bear: It applied "certainty-predicates" where actual, in contradistinction from programmatic, natural philosophy was concerned. This in itself was bad enough, but on top of it this way of expression was used without the least visible justification, the impressive mathematical superstructure being generally unknown. It was easy to judge the work as emotionally unbalanced. For example, concluding the description of his Experimentum Crucis, Newton says:

"And so the true cause of the length of that image was detected to be no other than that light consists of rays differently refrangible . . ." ([15], p. 95)

And summing up the thirteen propositions wherein the doctrine of the origin of colors was expounded, again he uses a similar formulation:

"13. I might add more instances of this nature but I shall conclude with this general one, that the colors of all natural bodies have no other origin than this, . . ." ([15], p. 99)

And then in rapid succession the following declarations:

"And that this is the entire and adequate cause of their colors, is manifest . . ."

"That these things being so, it can be no longer disputed, whether there be colors in the dark, nor whether they be the qualities of the objects we see, no nor perhaps, whether light be a body."

"But to determine more absolutely what light is, after what manner refracted, is not so easy. And I shall not mingle conjectures with certainties." ([15], p. 100)

This was all that could be read on this point by Pardies and Huygens in the Philosophical Transactions. But Hooke, who received "for review" the original paper before it went to print, read a crucial passage which was not printed afterwards when the paper was published:

"A naturalist would scarce expect to see the science of those (colors) become mathematical, and yet I dare affirm that there

is much certainty in it as in any other part of optics. For what I shall tell concerning them is not an hypothesis but most rigid consequence, not conjectured by barely inferring 'tis thus because not otherwise, or because it satisfies all phenomena (the philosophers universal topic) but evinced by the mediation of experiments concluding directly and without any suspicion of doubt." ([15], p. 96 and see note 19 *ibid.*)

Oldenburg, like the cautious and calculating person that he was, thought it prudent to omit this passage for publication in the Transactions. But Hooke had read it and rejected it as a curious piece of dogmatism; the harm was done. Thus started an unpublished correspondence between him and Newton, as Oldenburg would time and again omit from print the crucial and relevant passages. But though the full extent of Newton's dogmatism was known only to those who heard of or saw the pre-edited paper, the intellectuals of the age were trained, almost conditioned, to spot signs of dogmatism and to react in distrust, and what remained in print was more than enough for Pardies and Huygens. The situation, therefore, was ripe for an "eyebrow-raising" reaction: a revolutionary theory, where no revolution was felt needed; a theory destructive of all previous ones; an unclear mathematical groundwork which is not even hinted at; and all this in a dogmatic style implying the final truth of the theory.

All these were very easy to spot, were actually spotted and were made the core of the dispute that followed. Everyone was fully aware that this was the main issue to settle. Everyone, that is, except Newton, who, truly perplexed, and after four months of meditation on Hooke's reaction, at last said:

"But I must confess at the first receipt of these considerations I was a little troubled to find a person so much concerned for a hypothesis, from whom in particular I expected an unconcerned and indifferent examination of what I propounded." ([15], p. 171)

He was aware that Hooke's main point was Newton's use of certainty expressions, but attributed this to Hooke's misdirected interest in saving his own hypothesis. But, Newton urged, a proper criticism of the paper should concentrate on the certainties in it, which were plentiful,

and to which no such sort of criticism was applicable. Being certainties, no blemish could be found in using certainty-predicates in reference to them. And thus the next sentence reads:

"But yet I doubt not but we have one common designe, a sincere endeavor after knowledge, without valuing uncertain speculations for their subtleties, or despising certainties for their plainness."

Thus he sidestepped the whole of Hooke's attack, and the dispute was now at skew directions. For, as I shall presently try to argue, though Hooke was truly "concerned for" his hypothesis, and naturally so, it was not his purpose to validate or refute *any* hypothesis. What caused him "concern" was Newton's unmistakably dogmatic mode of announcing a theoretic result with an air of finality. Newton would never be able to comprehend this sort of criticism, for above his "mathematical way" and his vision of the mathematization of argumentation, or at the base of it, was his utter incapacity to grasp the "fine structure" of the difference between fact, or phenomena, and theory in those delicate situations where they seemed to fade and blend into each other. These categories were hopelessly mingled in his mind, which fusion, though a handicap in his disputes, was an integral part of his genius, as it was of other geniuses such as Harvey, Galileo, Dalton and Faraday. This trait of Newton, which I believe to be a fundamental source of his difficulties with his public in 1672 and on many occasions afterwards, is mentioned here only for the sake of completeness. (A detailed study of this subject will be published separately). Newton's next instalment of the unpublished correspondence showed more clearly that the exact locus wherein his dogmatism lay eluded him once again:

"In the last place I should take notice of a casual expression which intimates a greater certainty in these things than I ever promised; viz: The certainty of mathematical demonstration. I said indeed that the science of colors was mathematical and as certain as any other part of optics; but who knows not that optics and many other mathematical sciences depend as well on physical principles as on mathematical demonstrations: And the absolute certainty of a science cannot exceed the certainty of its principles. Now the evidence by which I asserted the propositions of colors is in the next words expressed to be

from experiments, and so but physical: whence the propositions themselves can be esteemed no more than physical principles of a science. And if those principles be such that on them a mathematician may determine all the phenomena of colors that can be caused by refraction and that by computing or demonstrating after what manner and how much those refractions do separate or mingle the rays in which several colors are originally inherent; I suppose the science of colors will be granted mathematical and as certain as any part of optics. And that this may be done I have good reason to believe because ever since I became first acquainted with these principles, I have with constant success in the events made use of them for this purpose." ([15], p. 187, and see note 18, *ibid.*)

This was a typical example of Newton's repeated failure to grasp the exact import of the dispute. For when Hooke had claimed that he couldn't regard Newton's theory as necessary, his reason was not its being based on experimental data which are intrinsically uncertain. This was completely a side issue, and to turn it into a main point was either to evade the issue or to misunderstand what was at issue. Hooke's point was rather that any theory was probable in principle since it was a theory and not a description of facts. The certainty or accuracy of the facts described or assumed was irrelevant. Thus it was not Newton's previous assertion about "the science of colors becom(ing) mathematical" which was of importance in the dispute. It was the rest of Newton's little manifesto that mattered, in which his blind spot is most forcefully expressed. What gave offense was his stout affirmation that his theory is not a hypothesis but "*a most rigid consequence*" of "*experiments concluding directly and without any suspicion of doubt*." But Newton could never realize that such declarations were inappropriate in a sober scientific tract, and that they gave the whole work a slightly absurd air. And he therefore never even tried to defend it.

2. HOOKE AGAINST NEWTON'S CLAIM OF NECESSITY

From the first letter on, Hooke declared that he did not doubt either the truth of the experiments or the high plausibility of Newton's interpretation of them. These were never points he wished to dispute. It is true

that he never accepted Newton's theory, but this is very different from the assertion that he rejected it as false. This is a most important point to realize, since it is seldom clearly pointed out, and quite frequently ignored or misrepresented. Thus A. R. Hall writes:

"The reactions of the critics, Hooke and Huygens amongst them, were interesting. When Newton's first paper appeared in the Philosophical Transactions, they judged initially that he was merely speculating, and tried to answer with irrelevant arguments. Then they denied that the experiments gave the results described by Newton, or *maintained that if the experiments were correct the conclusions drawn from were false.*

Neglecting the minor critics, it may be doubted whether either Hooke or Huygens . . . ever succeeded entirely in adjusting their thinking in accordance with the evidence of Newton's experiments." ([8], p. 254)

And in the same vein, C. C. Gillispie writes:

"The incomprehension which greeted his theory of colors was the more frustrating that it raised objections among inferior minds who truly could not understand what he meant, so deep and novel was his insight, so new and different his conception of science." ([6], p. 125)

Even such an authoritative work as A. I. Sabra's *Theories of Light* does not quite do justice to Newton's adversaries, though its views are very close to the views presented here. Thus he writes:

"In general, Hooke's attitude to Newton's theory was the following: he was willing to grant the experimental results reported by Newton, but was unable to accept the 'hypothesis' which the latter had proposed to explain them." ([20], p. 251)

Now, I quite agree that Hooke did not, as a matter of fact, "accept" Newton's explanation. But his "inability" to accept it was far from, nor did it ensue from, his rejecting it as false, and this is nowhere even suggested in Sabra's work.

To do justice to Hooke it must be shown that (1) he never rejected Newton's hypothesis, (2) he regarded it as highly "ingenious" and (3) he was quite equal to the task of understanding it thoroughly.

That Hooke did not regard Newton's theory as false will appear

from realizing that, on the one hand, he regarded it as being as well suited for explaining the experiments as his own and that, on the other hand, he carefully declared his skeptical attitude equally towards both. The two theories he regarded as of equal explanatory power, as equally devoid of absolute proof, but also as equally exempt from falsificatory experiments. They were, for him, equivalent theories. Thus, he could not reject Newton's theory as false without doing the same to his own. Referring to the Experimentum Crucis, he says:

" . . . for the same phenomenon will be salved by my hypothesis as well as by his without any manner of difficulty or straining." ([15], p. 111)

Both theories lacked any proof of their being a true description of reality:

"In short I will assure him I do as little acquiesce in that (Hooke's theory) for a reality as I do in his." ([15], p. 200)

And to sum this up, the following words should be conclusive:

"Nor would I be understood to have said all this against his theory as it is an hypothesis, for I do most readily agree with him in every part thereof, and esteem it very subtle and ingenious, and capable of salving all the phenomena of colors"; ([15], p. 113)

As to his understanding of Newton's theory, it must be emphasized that the "depth" of this theory—to use Gillispie's expression—did not go beyond the "mixture-idea" of the constitution of white light and the constant one-one relationship that obtains between color and index of refraction. To assert that such high powered intellects as Hooke and Huygens could fail to grasp this "depth" is absurd. Hooke's letters leave no doubt that he fully and completely mastered these principles, for otherwise he would not have been able to dissect these principles into their experimental part—which he accepted—and their theoretic or explanatory part—of which he could not see the *necessity of acceptance*:

"First then Mr. Newton alledges, that as the rays of light differ in refrangibility, so they differ in their disposition to exhibit this or that color; with which I do in the main agree . . ."

"Upon this account I cannot assent to the latter part of the

proposition, that colours are not qualifications of light derived from refractions or reflections of natural bodys but original and connate propertys etc." ([15], p. 112)

And afterwards, in the second letter he suggested that Newton's formulation of Hooke's doubts was in fact inadequate. For Newton's formulation of one main doubt was "whether whiteness be a mixture of all colors," ([15], p. 178); while Hooke insisted that formulation of the true difficulty would ensue by adding: "or colors (be) in light before it be refracted." ([15], p. 202).

I suggest that Hooke intended this addition to be an answer to Newton's promise in the relevant passage to *explicate* the phenomena without using any hypothesis. Hooke points out here that Newton will not be able to explicate Hooke's proposed addition since this is his very explicating hypothesis.

Again, Hall's contention that "it may be doubted whether either Hooke or Huygens . . . ever succeeded entirely in adjusting their thinking in accordance with the evidence of Newton's experiments" is no less misguided. The evidence of the experiments was fully accepted by Hooke as we saw, but his acuteness in distinguishing between this and the explanation Newton gave of it enabled him to treat them separately.

I believe that what caused the misinterpretations of the Newtonian controversy prevalent among modern historians was Newton's own reaction and interpretation of it. He was amazed to see his critics so much "concerned for a hypothesis," whereas what was at stake were simple facts. What he could not see was that no one disputed the truth of the facts or the *adequacy* of the hypotheses. Modern historians too are slow to see this, tending to view the situation through the eyes of Newton, the victor of history.

Now, since according to my suggestions, it was far from Hooke's intention to refute Newton's theory of mixture, the question arises as to what did he intend to do in his criticism. I will try now to show that it could be maintained that all he probably wished to convey was the idea that there was no strict certainty in Newton's theory, contrary to what Newton himself implicitly wrote in the published paper, and explicitly in the unpublished correspondence. Showing this may also corroborate the contention, that Hooke never tried to refute Newton's theory. It will be noticed too, that neither did Hooke argue that the

theory lacked the best experimental basis available.¹ Both refutation and validation of the theory were quite beside the issue at hand, which was simply this: to show that Newton's theory was not necessary. This purpose is clearly stated in the second sentence of his letter:

"But though I wholly agree with him as to the truth and curiosit of those (observations) he hath alleged yet as to his hypothesis of salving the phaenomena of colours thereby, I confess I cannot yet see any undeniable arguments to convince me of the certainty thereof."

And thus the main argument stated here refers neither to the truth nor even to the plausibility of Newton's theory. The main problem to be treated is this theory's lack of the absolute and mathematical certainty which Newton alleged it had. Hooke's whole position is summed up by him thus:

"Nor would I be understood to have said all this against his theory, as it is an hypothesis; for I do most readily agree with them in every part thereof, and esteem it very subtil and ingenious, and capable of salving all the phaenomena of colours; but I cannot think it to be the only hypothesis; not soe certain as mathematical demonstrations." ([15], p. 113)

Hooke's technique of proving his contention is a direct answer and response to Newton's unpleasant reference to "the philosopher's universal topic," namely their procedure of the validation of a theory by "barely inferring 'tis thus because not otherwise." ([15], p. 96)

Hooke tried now to demonstrate that exactly because Newton's theory could not satisfy this condition, that is, because it was not unique in being well suited for "salving the phaenomena," it followed that it was not necessary, and therefore not certain, since certainty pertains only to necessary assertions. Needless to say, Newton was right, but so was Hooke, for just as a theory may not be validated by an elimination pro-

¹Kuhn, who had discerned and stated this view in [13], p. 37, refused nevertheless to accept it as Hooke's whole *theoretical* justification, and added that another reason for Hooke's criticism was the small number of experiments in Newton's paper, contrary to the usage as exemplified in Hooke's *Micrographia*. Though it is a plausible suggestion, it should be noted that Hooke never mentioned the matter.

cedure, even so it may well be shown to lack certainty by confronting it with an equivalent theory.

So Hooke set out to demonstrate the lack of absolute necessity of Newton's mixture hypothesis, and for this he used his own wave hypothesis as an equivalent theory. That this was his only intention in bringing in his theory, as distinct from any attempt at a refutation of Newton's theory, he declared several times, both in his first and in his second letters, and I can hardly see a reason for not taking his word for it:

"I agree with the observations of the ninth, tenth, and eleventh, though not with his theory, as finding it not absolutely necessary, they being as easily and naturally explained and salved by my hypothesis. The reason of the phaenomena of my Expt which he alledges is as easily salvable by my hypothesis as by his; as are also those which are mentioned in the 13th. I do not therefore see any absolute necessity to believe his theory demonstrated, since I can assure Mr. Newton I can not only salve the phaenomena of light and colours by the hypothesis I have formerly printed . . . but by two or three other very different from it, and from his, . . ." ([15], p. 113)

More explicitly Hooke expressed the idea in his second letter:

"And whereas Mr. Newton believes that I am averse from his by a presuppossession of my own, I do assure him I urged mine and might have urged any other, only to show that this or that hypothesis was not absolutely necessary . . ." ([15], p. 198)

And again,

". . . I see no absolutely necessity thereof from any thing he has yet said, since the thing is capable of being explained without making use of that supposition as intelligibly as by supposing it. And Mr. Newton is much mistaken if he thinks that I opposed his hypothesis for the sake of asserting my own. . . . But it was not to establish this or that hypothesis but to show Mr. Newton's corpuscular hypothesis of light and colours not absolutely necessary." ([15], p. 200)

This lack of necessity was also one of the reasons which determined Hooke's attitude towards Newton's *Experimentum Crucis*. This was not

the evidence, said Hooke, which could crucially decide between two hypotheses such as his own and Newton's, and thus establish the necessity of Newton's theory:

". . . for the same phenomenon will be solved by my hypothesis, as well as by his, without any manner of difficulty or straining; nay, I will undertake to show another hypothesis, differing from both his and mine, that shall do the same thing."

He was right, for the *Experimentum Crucis* treated of light after being refracted, whereas the mixture theory described its constitution before the refraction. And of this point, which is the main hypothesis of Newton, Hooke was fully aware² as the following two passages, from the first and second letters, respectively, amply show:

"But why there is necessity, that all these motions, or whatever it be that makes colours, should be originally in the simple rays of light I do not yet understand the necessity." ([15], p. 111)

"To his reasoning about the first of these I have only this to say that he doth not bring any argument to prove that all colours were actually in every ray of light before it has suffered a refraction, nor does his *exp. crucis* as he calls it prove those properties of coloured rays . . . were not generated by the said refraction."

And his first letter ends with a twin denial of the necessity of a mixture supposition and of the absoluteness of the demonstration of Newton's theory, while at the same time allowing for the possibility of both:

"Tis true I can in my supposition conceive the white or uniform motion of light to be compounded of thousands of com-

²Sabra argues that opposition to Newton came mainly from the recognition of the atomistic presuppositions which he made in the interpretation of his *Experimentum crucis*. This may well be true but was not used by either Pardies or Huygens. Nor did Hooke, who alone pointed out these atomistic commitments of Newton, use them as the basis of his criticism of the *Experimentum crucis*. His criticism, cited in the following two passages, is formulated so as to be independent of any atomistic aspect. We have here a case of a sharp insight into the logical structure of Newton's theory. Sabra, who adopts Hooke's view ([20], p. 295), never mentions his debt to him.

pounded motions, . . . but I see no necessity of it. If Mr. Newton hath any argument, that he supposeth an absolute demonstration of his theory, I should be very glad to be convinced by it." ([15], p. 114)

To sum up: I suggest that one of the main purposes of the criticism which was leveled against Newton was to point out the fact that his theory did not have the necessity which he claimed for it. In other words, his adversaries admitted that the theory could be true, that it was doubtlessly adequate, but refused to accept Newton's claim about its unique status as the *only* theory capable of explaining the phenomena. On the other hand, Newton took his extreme stand for two reasons, the one being the mathematical basis of his refutation of conventional optics, and the second was the concrete character which his theory assumed in his mind, (to use Prof. Ne'eman's terminology). This resulted in his inability to isolate theoretical from observational and factual elements in his suggestion.

3. THE EXCHANGE WITH PARDIES AND HUYGENS

3.1 The dialogue between Pardies and Newton followed the same general pattern. Pardies acknowledged the ingenuity of the theory and its high explanatory powers, when, referring to the mixture hypothesis, he said:

"The author's notion of colours follows very well from the preceding hypothesis. . . . Indeed, nothing can be more ingenious and proper, than what he says about Mr. Hooke's experiment . . ." ([3], p. 89)

However, though the theory was quite well suited to explain the phenomena, it was equally by no means necessary. In Pardies' case, however, this was suggested because Newton's presentation of the first experiment failed to bring out the fact of minimum deviation and its theoretical consequences. This failure obliterated the significance of the data that Newton gave about the angles of incidence and refraction in his first experiment. Pardies received the information about Newton's letter from a second source, (as appears from his second letter), and as the significance of Newton's data was probably lost on this source, Pardies

ignored them completely. Thus, he pointed out that a difference of 30° between rays whose angles of incidence are in the vicinity of 30°, produced the elongated image of the experiment, and this on the old assumption of a single refraction index. A similar misunderstanding produced a similar suggestion concerning an orthodox interpretation of the Experimentum Crucis. In short, Newton's theory was ingenious but unnecessary. The question of truth was not touched:

"Since then, here is an evident cause of that oblong, figure of the rays, and that cause such as arises from the very nature of refraction; it seems needless to have recourse to another hypothesis, or to admit of that diverse refrangibility of the rays." ([3], p. 89)

Though Pardies produced two experimental facts which seemed to contradict Newton's theory—namely, that an actual mixture of all available colors did not produce white, and that not every pair of Hooke's wedges stops the whole transmitted light—yet these were not made the basis of the criticism.

Newton's answer cleared Pardies' technical misunderstandings, and Pardies was satisfied now that no difficulties could be seen "on that head." But on another "head," namely the question of the necessity of Newton's theory, he refused to yield:

" . . . on that head I find no difficulty. I say on that head; for the greater length of the image may be otherwise accounted for, than by the different refrangibility of the rays." ([3], p. 104)

And he proceeds to an account of some available theories which are adequate alternatives, being equivalent to Newton's theory, such as Grimaldi's, Hooke's and Pardies' own theories.

Newton in his answer expanded on methodological problems, producing some of his classical formulations of the "best and safest way of philosophizing." In it he made more explicit his view of his own theory. It was not a mere possibility, but a certainty since it concerned only some "properties" of light. Therefore, this whole quibbling about possible alternatives is completely of no use in deciding the worth of his "Doctrine":

"For if the possibility of hypotheses is to be the test of the truth and reality of things, I see not how certainty can be obtained in any science; since numerous hypotheses may be devised, which shall seem to overcome new difficulties." ([3], p. 106)

Pardies, in his answer, never mentioned this problem again. It is quite probable that Pardies realized Newton's blindness to the "necessity-problem" and simply despaired of enlightening him on this "head."

3.2 Though Huygens was more insistent, the same thing happened to him. His first comment on the theory was couched in typically cautious words:

"J'avouë, que la nouvelle théorie des couleurs, avancée par M. Newton, me paroit jusques ici très-vraisemblable; et *L'Experimentum crucis . . . la confirme beaucoup.*" ([15], p. 207)

Whatever was implicit here was spelled out in no equivocal terms after Huygens read Newton's second answer to Pardies with its rules about the best road to certainty in science:

"Ce que vous avez mis de M. Newton dans un de vos derniers journaux confirme encore beaucoup sa doctrine de couleurs. Toutefois la chose pourroit bien estre autrement, et il me semble qu'il se doibt contenter que ce qu'il a avancé passe pour une hypothèse fort vraisemblable." ([15], p. 235)

This was the essence of what Hooke, Pardies and Huygens tried to argue: That things could be otherwise than described by Newton's mixture hypothesis, which, therefore, was neither necessary nor certain. True, none of their suggestions and examples was even moderately adequate for the task, as Newton's answers always showed. But this was not the important thing in the issue. It was important, however, for Newton, for it strengthened his conviction that things really could not be otherwise than his theory described. While his adversaries talked about theories in general, he talked about a single theory. They talked about the proper way of talking, but he talked about truth and facts. And Huygens' last words reflected that he suddenly realized the uncrossable chasm that lay between them, which made futile any attempt at a true two-way communication of ideas:

"Pour ce qui est des Solutions de Monsieur Newton aux doutes que j'avois proposé touchant sa theorie des Couleurs, il y auroit de quoy y respondre et former encore de nouvelles difficultés; mais voyant qu'il soutient sa doctrine avec quelque chaleur, je ne veux pas disputer." ([15], p. 285)

4. FALLIBILISM PRIOR TO NEWTON AND ITS DOCTRINE OF STYLE

4.1 The philosophical movement of the century started as a two-fold reaction against Dogmatism as exemplified in Aristotelian—scholastic philosophy and against skepticism. These two trends were not separate but intermingled in most of the arguments against the new scientific movement. A standard argument consisted in pointing out some insurmountable difficulties on human minds' road to knowledge, and then concluding that the only certain knowledge available is the one that survived through the ages, its survival being the proof of its certainty.

The two most important philosophical movements of the 17th century sprang out of an effort to meet this general sort of argumentation: Both Bacon's and Descartes' thought is based on, and starts with, a full development of the first stage of the argument—Bacon with his systematization of the sources of error in his theory of idols, and Descartes with his taking skeptical technique to extremes by his device of the *malin génie*.

Now, though they employed the skeptical technique with the aim of disproving skepticism itself and, thereby, the dogmatic conclusion of the dogmatists' argument, the popularity of their systems ensued in a new and wide-spread realization of the force of skeptical arguments.³

In England a "Bacon-faced generation"⁴ inherited from him many of the elements which were at the root of Newton's controversy. Only, the sides in the dispute put different emphasis on the same elements. While

³For this aspect of the influence of Descartes' doctrine on continental thought in the decades that preceded the foundation of the Royal Society, see [17], ch. X, entitled "Descartes: Sceptique malgré lui."

⁴Henry Stubbe's phrase, see [11], p. 258. Jones' fundamental work establishes the immense influence of Bacon on the scientific movement in England in the 60's and 70's of the century. That the Royal Society abandoned Bacon's ideal of science is argued in [14], but Leeuwen's work leaves much to be desired, since it ignores the role of the concept of theory in Bacon's thought, which cripples his whole thesis.

Newton endorsed an extreme rejection of all theory, unless it was mathematically deduced from the purely experimental basis, his opponents were primarily sensitive to dogmatic modes of speech, and to the imperfection and weakness of human intellect when trying to pierce nature's secrets. For Bacon's doctrine contained a strong case for basic skepticism, while its optimism was carefully limited. This side of Bacon's doctrine is very little stressed by him, so little indeed that his philosophy has the outer appearance of a silly and thoughtless extreme optimism. As a matter of fact, however, it contains "mitigating-suffixes," which probably did not escape the attention of the members of the Royal Society. To give only one example, after enumerating the kinds of idols, he says:

"And as the first two kinds of idols are hard to eradicate, so idols of this last kind cannot be eradicated at all. All that can be done is to point them out, so that this insidious action of the mind may be marked and reprobated." ([1], vol. III, p. 45)

The effect of the effort of the scientific movement to steer clear of both skepticism and dogmatism, joined with the claim that a method—both intellectual and experimental—of arriving at well-founded knowledge really does exist, resulted in a sort of guarded, mitigated, and sober brand of skepticism, whose main aim was to stay on a safe footing from both dogmatic and skeptic attacks. Skepticism in its extreme and destructive sense was sidestepped by using Bacon's slogan about the aim of science being the achievement of higher and higher degrees of certainty. Both dogmatism itself and the charge of having created a new kind of dogmatism, were met by a careful employment of language in the description of both the aims and the achievement of the new science. "Certainty-predicates" were in general banned and "probability-predicates" were used whenever possible.

I will now produce some evidence indicating that one typical element of the new scientific fallibilism was an emphasized sensitivity to the style of the language in which the theories were presented. This I will try to show through passages taken from works of the relevant circle of people, but written prior to, and independently of, the controversy about Newton's optics.

4.2 In an attack on extreme Epicurean atomism, Boyle wrote against the dogmatic mode of asserting that matter and motion are the

only principles which underlie phenomena. Using what since then has become a standard argument, he points out that the same effects may be the outcome of different causes:

"And it will often times be very difficult, if not impossible, for our dim reasons to discern surely which of these several ways . . . she has really made use of to exhibit them . . . and that it is a very easy mistake for men to conclude that because an effect may be produced by such determinate causes, it must be so, or actually is so." ([2], vol. II, p. 45)

This was the general statement of the fallibilistic nature of scientific argument. It had its immediate roots both in Descartes (for example [5], vol. I, pp. 300-302) and in Bacon, and was by now used for checking enthusiastic science-makers. Boyle also employed the Baconian and Cartesian doctrine about the various degrees of certainty which pertain to different kinds of proofs: metaphysical proofs have the highest certainty, for what they demonstrate cannot be otherwise; physical proofs are of inferior kind, since they are based on hypotheses; everyday decisions are based on moral certainty. (Moral certainty, be it noted, was what Descartes argued for his theories about the physical world). But physical proofs attain, as a matter of fact, only very seldom the high degree of certainty allotted to them here, and generally they do not surpass moral certainty, the reason being the great difficulty in executing all the needed experiments with all the necessary rigour

"that is requisite for the building of an undoubted theory upon them. And there are I know not how many things in Physicks, that men presume they believe upon physical and cogent arguments, wherein they really have but a moral assurance." ([2], vol. IV, p. 42)

The cogency of this view is better brought out through the skeptic's technique, which Boyle employs in his "The Sceptical Chymist." His description of it may be taken as the general scheme of what was about to happen in Newton's controversy, and the plausibility of Hooke's contention about his real aim in that dispute may be seen to be based on his belief that Newton was well-acquainted with this procedure. Boyle says that in case some of the arguments of the skeptic Carneades, who is Boyle's spokesman,

"shall not be thought of the most cogent sort that may be, he hopes it will be considered, that it ought not to be expected, that they should be so. For his part being chiefly but to propose doubts and scruples, he does enough if he shews, that his adversaries arguments are not strongly concluding, though his own be not so neither . . . it is not necessary, that all the things a sceptic proposes should be consonant; since it being his work to suggest doubts against the opinion he questions, it is allowable for him to propose two or more several hypotheses about the same thing. . . . Because it is enough for him, if either of the proposed hypotheses be but as probable as that he calls in question . . . ([2], vol. I, p. 460)

All this found its expression in the style Boyle used in the presentation of his theories, which was the easiest and natural way to make manifest his abhorrence of either dogmatism or complete skepticism:

Perhaps you will wonder . . . that in almost every one of the following essays I should speak so doubtfully, and so often, perhaps, it seems, it is not impossible, and such other expressions, as argue a diffidence of the truth of the opinions I incline to, and that I should be so shy of laying down principles, and sometimes of so much as venturing at explications. But I must freely confess . . . that having met with many things, of which I could give myself no one probable cause, and some things, of which several causes may be assigned so differing . . . I have often found such difficulties in searching into the cause and manner of things, and I am so sensible of my own disability to surmount these difficulties, that I dare speak confidently and positively of very few things, except of matters of fact. And when I venture to deliver any thing, by way of opinion, I should, if it were not for mere shame, speak yet more diffidently than I have been wont to do." ([27], vol. I, p. 305)⁵

⁵What Newton derided as the "Philosopher's universal topic," was probably the method which Boyle had in mind when he set about to "prove" the plausibility of mechanical philosophy. ([2], vol. I, p. 335) This point is not always appreciated, as for example in a recent history of atomism by Kargon, in which he argues that though Boyle attempted to validate mechanical philosophy, all he in fact succeeded in doing was to falsify Aristotelian theory. What is not taken into account here, apart from the illegitimate confusion of Kargon's modern confirmation ideas with Boyle's 17th century concept of "proof," is that this refutation of Aris-

4.3 The same pattern of careful balance between dogmatism and skepticism may be found in Glanvill. Again, Baconian and Cartesian theories about the validity of skepticism are made the foundation. Glanvill added, however, his insight about the theoretic, and therefore hypothetical, nature of every causal explanation. Now, since "All knowledge of causes is deductive: for we know none by simple intuition," it follows that "we cannot infallibly assure ourselves of the truth of the causes that most obviously occur; and therefore the foundation of scientifical procedure, is too weak for so magnificent a superstructure." ([7], pp. 166-7)

The idea of factual or logical impossibility, which lies at the bottom of every dogmatism, is thereby seen to lack any validity:

"We hold no demonstration in the notion of the dogmatist, but where the contrary is impossible; for necessary is that, which cannot be otherwise. Now, whether the acquisitions of any on this side perfection, can make good the pretensions to so high strain'd an infallibility, will be worth a reflexion . . ." ([7], p. 168)

"For the best principles, excepting divine, and mathematical, are but hypotheses; within the circle of which, we may indeed conclude many things, with security from error: But yet the greatest certainty, advanc'd from supposal, is still but hypothetical. So that we may affirm, that things are thus and thus, according to the principles we have espoused: But we strangely forget ourselves, when we plead a necessity of their being so in Nature, and an impossibility of their being otherwise." ([7], p. 170)

Though most of his book is directed against the Aristotelian brand of dogmatism, the last part of it is aimed at the "Virtuosi" of the Royal Society. It contains six arguments which aim to convince those virtuosi of the worthlessness of "opinionative confidence":

totelian theory of qualities probably constituted for Boyle a strong case for the truth of the alternative theories then extant, namely the mechanistic theories, according to the logic of "'tis thus because not otherwise." It is highly instructive to notice that even here the conclusion about the falsification of Aristotelian theory is formulated by Boyle in 1675 in terms of lack of absolute necessity:

"My purpose in these notes was rather to shew, it was not necessary to betake ourselves to the scholastic or chemical doctrine about qualities . . ." ([2], vol. IV, p. 236 quoted in [11], p. 101)

- (1) It is the "effect of ignorance," for "True knowledge is modest and wary; 'tis ignorance that is so bold and presuming." ([7], p. 195)
- (2) It is caused by passion and not by reason, and "He's but a novice in the art of autocracy, that cannot castigate his passions in reference to those presumptions, and will come as far short of wisdom as science." ([7], p. 196)
- (3) It is "the great disturber both of ourselves and the world without us: for while we wed an opinion, we resolvedly engage against every one that opposeth it." ([7], p. 197)
- (4) "To be confident in opinions is ill manners and immodesty; and while we are peremptory in our persuasions, we accuse all of ignorance and error, that subscribe not our assertions. The dogmatist gives the lie to all dissenting apprehenders, and proclaims his judgment fittest, to be the intellectual standard. . . . And he that affirms that things must needs be as he apprehends them, implies that none can be right till they submit to his opinions, and take him for his director." ([7], p. 200)
- (5) "Obstinacy in opinions hold the dogmatist in the chains of error, without hope of emancipation."
- (6) "It betrays a poverty and narrowness of spirit." In his last argument which also closes the book, Glanvill refers to the style which is appropriate to sober scientific discourse. He already said that:

"Did we but compare the miserable scantness of our capacities, with the vast profoundity of things; both truth and modesty would teach us a more wary and becoming language." ([7], p. 169)

And now again he connects the proper frame of mind with a proper mode of speech:

"the determinations of the nobler mind, are but temporary, and he holds them, but till better evidence repeal his former apprehensions. . . . The modesty of his expression renders him infallible; and while he only saith he thinks so, he cannot be deceived, or ever assert a falsehood." ([7], p. 202)

Thus, this "modest" mode of speech is turned into a means of achieving certainty in science: The more assertively is a theory expressed the less chance has it of being able to validate its claims.

4.4 Glanvill's book was published twice within five years. First in

1661, and then in 1665 after he was elected to the Royal Society. In the same year Hooke published his *Micrographia*. In the preface he goes over the well-trodden road of Baconian and Cartesian doubt. The manifest fallibility of "sense and of memory" makes "all the succeeding works which we build upon them, of arguing, concluding, defining, judging, and all the other degrees of reason . . . liable to the same imperfection, being, at best, either vain or uncertain." Even those propositions "which we think to be the most solid definitions, are rather expressions of our own misguided apprehensions than of the true nature of the things themselves."

"The effects of these imperfections" are "ignorance and stupidity" as well as a "presumptuous imposing on other men's opinions and a confident dogmatizing on matters, whereof there is no assurance to be given." Hooke's fear of dogmatizing is expressed in the following passage:

"If therefore the reader expects from me any infallible deductions, or certainty of axioms, I am to say for myself, that those stronger works of wit and imagination are above my weak abilities; or if they had not been so, I would not have made use of them in this present subject before me: Wherever he finds that I have ventur'd at any small conjectures, as the causes of the things that I have observed, I beseech him to look upon them only as doubtful problems, and uncertain guesses, and not as unquestionable conclusions, or matters of unconfutableness; I have produced nothing here, with intent to bind his understanding to an implicit consent; I am so far from that, that I desire him, not absolutely to rely upon these observations of my eyes, if he finds them contradicted by the future ocular experiments of sober and impartial discoverers." ([9], fifth page of the Preface)

This same fear is expressed in his words to the Royal Society. Here, however, he is mainly concerned about his "expressions." After having praised the "rules" which the Royal Society prescribed for best securing the promotion of science he praises

"particularly that of avoiding dogmatizing, and the espousal of any hypothesis not sufficiently grounded and confirm'd by experiments. This way seems the most excellent, and may pre-

serve both philosophy and natural history from its former corruptions. In saying which, I may seem to condemn my own course in this treatise; in which there may perhaps be some expressions, which may seem more positive than YOUR prescriptions will permit: And though I desire to have them understood only as conjectures and quaerries (which YOUR method does not altogether disallow) yet if even in those I have exceeded, 'tis fit that I should declare, that it was not done by YOUR directions."

4.5 Two years later, in 1667, Thomas Sprat published his *History of the Royal Society*, whose purpose was to develop a well designed answer and refutation of the dogmatic and skeptic adversaries of the new scientific movement. But the immense importance which attached at the time to the book stemmed mainly from the fact that it was both the first and the definitive exposition of the philosophy of scientific research held at the time by the members of the Royal Society.⁶

One of the most recurrent themes of this apology is the non-assertive character a true philosopher of nature must have, and which, by implication, the members of the Royal Society do in fact possess. In contradistinction from the dogmatic philosopher, who "love(s) not a long and a tedious doubting," a true philosopher "should be well practiced in all the modest, humble, friendly virtues: should be willing to be taught, and to give way to the judgment of others," since "such men, whose minds are so soft, so yielding, so complying, so large, are in a far better way, then the bold and haughty assertors." ([21], p. 32-34)

Closely connected with this conception of "the doubtful, the scrupulous, the diligent *Observer of Nature*" ([21], p. 367) as the ideal of the new scientist, is the insistent defense of toleration of opinions, religious and otherwise. Motivated both by fearful memories of recent religious wars, and by the urgent need to clear the Royal Society from charges of being menaces to existing social, academic and religious order, Sprat points out the anti-revolutionary character of the Society which ensues from their specific occupation which is "to assemble about some calm and indifferent things especially experiments." Indeed, this occupation

⁶Concerning the circumstances of its publication see [4]. The philosophical issues are described in [11], pp. 222-236. Jones also documents the official status of the work on p. 333, n. 85 and n. 87.

is suggested as "the most effectual remedy to be used" against civil and religious upheavals, for in such experiments "there can be no cause of mutual exasperations: In them they may agree or dissent without faction, or fierceness" ([17], p. 426) and, by this means, come to a peaceful life of spiritual tolerance. The contemplation of Nature "never separates us into mortal factions; . . . (it) gives us room to differ, without animosity; and permits us to raise contrary imaginations upon it, without any danger of a *Civil War*." ([21], p. 56).

Whereas religious and social toleration are suggested as the beneficiary probable outcomes of the method of toleration adopted in the Royal Society, this method is a most crucial factor for preserving the new science from old corruptions, and primarily from "peremptory addiction to others tenets, (which) sowers and perverts the understanding." For, "that man who is thoroughly acquainted with all sorts of opinions, is very much more unlikely, to adhere obstinately to any one particular. . ." ([21], p. 97) Moreover, the members of the Royal Society have, "by their indifferent hearing of all conjectures, that may be made from the tenets of any sect of philosophy . . . allowed a sufficient time, to ripen whatever is debated." ([21], p. 104-5)

For a theoretic reason for this democracy of "tenets," and "this fluctuation, this slowness of concluding," (p. 104) typical of the new scientist, Sprat resorts to the standard gambit used in skeptic apologies such as Descartes' and Boyle's:

"They could not be much exasperated one against another in their disagreements, because they acknowledge, that there may be several methods of Nature, in producing the same thing, and all equally good (. . .)." ([21], p. 92)

However, even though "the Royal Society may perhaps be suspected, to be a little too much inclined" towards "sceptical doubting" "because they always professed to be so backward from settling of principles, or fixing upon doctrines," yet "It must also be confessed, that sometimes after a full inspection, they have ventured to give the advantage of probability to one opinion, or cause, above another." ([21], p. 107)

This "confession" is followed by reasons why by thus "venturing" the Royal Society have not, however, "run any manner of hazard," one of the reasons being that

"whatever they have resolved upon; they have not reported as unalterable demonstrations, but as present appearances . . ." ([21], pp. 107-08)

and this independently of, and in addition to, the cautious manner of the commitment expressed by "giving the advantage of probability."

5. SUMMARY

My purpose in this paper was to point out the presence of a stylistic and linguistic mannerism among Newton's opponents, to connect this

Huygens published his theory of light in 1678, and so it seems that what he wrote there concerning methodological problems may not be relevant for illuminating his response to Newton's theory in 1672. As a matter of fact, his methodology in 1678 may have been constructed as a result of the 1672 controversy. However, some confirmation of the thesis here defended may be found in the mode of speech that Huygens adopted in his treatise, and it may be presented for this purpose.

Huygens' theory of scientific proof is succinctly presented in his preface. The only difference between the certainty attainable in geometry and in physics is that in the former it stems from the certainty of the axioms, while in the latter—from the certainty of the conclusions as confirmed by experiment. But nevertheless a very high degree of certainty may be attained in physics. Huygens' formulation of this is in the best Cartesian style: "It is always possible to attain thereby to a degree of probability which very often is scarcely less than complete proof." ([10], p. VI)

However, in the whole Treatise, only once does he use a truly assertive expression. Generally, he prefers to express himself in terms of confirmation and probability. So for instance the phenomena of double refraction, far from "overturning" his explanation, in fact "strongly confirm" it. [10], p. 52) After describing his hypothesis concerning the particles which build the Iceland crystal, he says: "Touching which particles, and their form and disposition, I shall at the end of this treatise propound my conjectures and some experiments which confirm them. . . . The double emission of waves of light, which I had imagined became more probable to me after I had observed a certain phenomenon . . ." ([10], p. 62) Typically cautious is the following summary:

"I have investigated thus, in minute detail, the properties of the irregular refraction of this crystal, in order to see whether each phenomenon that is deduced from our hypothesis accords with that which is observed in fact. And this being so it affords no slight proof of the truth of our suppositions and principles. But what I am going to add here confirms them again marvelously." ([10], p. 88)

In order to explain the behavior of the regular and irregular rays after splitting, he says,

"It seems that it will be necessary to make still further suppositions besides those which I have made; but these will not for all that cease to keep their probability after having been confirmed by so many tests." ([10], p. 92)

More extreme expressions are found only once in the whole treatise:

"All this proves then that the composition of the crystal is such as we have stated." ([10], p. 98)

with the general trend towards a sterilization of scientific discourse, and to suggest this mannerism as one important root of Newton's optical controversies in 1672. Of the philosophical background I mentioned the emergence of "constructive skepticism," with which I suggested connecting the linguistic mannerism. However, any attempt at a fuller reconstruction of the philosophical currents and undercurrents behind this linguistic ceremonialism, the caution and the general distrust of reason at the 60's and 70's would have to take into account a formidable array of intellectual currents, a task which obviously is beyond my present purpose.

Suffice it, therefore, to outline the general view which emerges concerning the role which Newton's first paper played in changing the philosophy of science of the next decades of the 17th century. The paper, and the exchange which took place afterwards, were an announcement of the end of the first stage in the Baconian scheme and of the first period of Royal Society, in which the aim was expressly limited only to the accumulation of a solid ground of experiments and observations with no expenditure of work towards the discovery of the true nature of things. The distrust of reason and the belief (again, only partial) in the senses, was concomitant with this aim, both as an ideological basis and as a logical consequence.

Newton's paper announced the beginning of a period, in which general laws may be "derived" with certainty from the accumulated mass of empirical facts. The distrust of reason is to be replaced by a mathematical scheme of "deriving" laws from a given set of facts, just as theorems are derived from a given set of axioms. That certainty may be attained in this new stage is justified by denying that any hypothetical element is presupposed in the reasoning, thus declaring the whole chain to be concerned only with the purely phenomenal layer of reality. Dogmatism is also thereby denied, since it pertains only to some mode of holding *opinions* (Glanvill's term) or *conjectures* (Sprat's term). Since only facts, phenomena, and logical or mathematical derivations from these, are allegedly used by Newton, naturally he cannot see the point of the accusations. A new period begins—the period of the blind spot.

REFERENCES

- [1] F. Bacon: Spedding, Ellis and Heath eds., *The Works of F. Bacon*, London, 1861.
- [2] R. Boyle: Birch ed., *The Works of Honourable Robert Boyle*, London, 1772.
- [3] B. Cohen, *Isaac Newton's papers and letters on Natural Philosophy*, Cambridge, Massachusetts, 1958.
- [4] J. I. Cope, Introduction in [21].
- [5] Descartes: Haldane and Ross trs., *The Philosophical Works of Descartes*, Dover Publications, 1955.
- [6] C. C. Gillispie, *The Edge of Objectivity*, Princeton, 1960.
- [7] J. Glanvill: Y. Owen ed., *Scepsis Scientifica*, London, 1885.
- [8] A. R. Hall, *The Scientific Revolution, 1500–1800*, 2nd edition, Longmans, 1962.
- [9] R. Hooke, *Micrographia*, Dover Publications, 1961 (facsimile of 1st edition, 1665).
- [10] Huygens: Sylvanus P. Thompson tr., *Treatise on Light*, Dover Publications, 1962.
- [11] R. E. Jones, *Ancients and Moderns*, 2nd edition, Berkeley and Los Angeles, 1965.
- [12] R. H. Kargon, *Atomism in England from Hartot to Newton*, Oxford, 1966.
- [13] T. Kuhn, "Newton's optical papers," in [3], pp. 27–45.
- [14] H. G. Van Leeuwen, *The Problem of Certainty in English Thought 1630—1690*, The Hague, 1963.
- [15] I. Newton: H. W. Turnbull ed., *Correspondence*, Vol. I, Cambridge, 1950.
- [16] I. Newton, *Opticks*, Dover Publications, New York, 1952.
- [17] R. Popkin, *The History of Skepticism from Erasmus to Descartes*, New York, 1964.
- [18] L. Rosenfeld, "La théorie des couleurs de Newton et ses adversaires," *Isis* 9 (1927), 44–65.
- [19] L. Rosenfeld, "Le premier conflit entre la théorie ondulatoire et la théorie corpusculaire de la lumière," *Isis*, 11 (1928), 111–122.
- [20] A. I. Sabra, *Theories of Light from Descartes to Newton*, London, 1967.
- [21] T. Sprat, *History of the Royal Society* (edited with critical apparatus b. J. I. Cope and H. W. Jones), St. Louis, Missouri, 1958.
- [22] R. S. Westfall, "Newton's Reply to Hooke and the Theory of Colours," *Isis*, 54 (1963), 82–96.
- [23] R. S. Westfall, "Newton and his Critics on the Nature of Colours."
- [24] R. S. Westfall, "The Development of Newton's Theory of Colours," *Isis*, 53 (1962), 330–358.
- [25] Whittaker, Introduction to [16].

HEGEL'S PHILOSOPHY OF NATURE

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In a symposium on the interaction between science and philosophy a short account of Hegel's philosophy of nature is probably not out of place, all the more as the bicentenary of Hegel's birth was celebrated a few months ago. Seen in retrospective, the results of this interaction between the philosophers of nature, especially Schelling and Hegel, and the scientists, have on the whole been of a more negative nature, for they had, in the 19th century, increased the rift between science and philosophy. Scientists, in their reaction to Hegel's metaphysics, a prioristic approach, and dialectical method, were driven into the other extreme of a complete rejection of metaphysics and a strictly positivistic attitude, which even now, in the second half of the 20th century, is, if not dominating, still a strong element of the rather ambivalent attitude of scientists towards philosophy.

However, I believe that Hegel's image and the image of his philosophy of nature has been grossly distorted by scientists, and the purpose of my talk is to make some amends for this injustice done to him and to emphasize a few of his positive achievements.

It is important to note that Hegel's philosophy of nature was preceded by that of Schelling, which was published in 1799, eighteen years before Hegel's *Encyclopaedie der philosophischen Wissenschaften* (1817).* Schelling's ideas were a pure product of romanticism, and Hegel, in the 3 years of his association with Schelling in Jena (1800–1803), was profoundly influenced by this romantic trend.

Two essential features of the romantic idea determined Schelling's philosophy of nature.

*All references (§) are quoted from G.W.F. Hegel, *Encyclopaedie der philosophischen Wissenschaften*, ed. by G. Lasson, Leipzig 1911. The English quotations from Part I of the *Encyclopaedia* are taken from A. V. Miller's translation, *Hegel's Philosophy of Nature*, Oxford 1970, with the permission of the Clarendon Press. All other quotations have been translated by the author.

- 1) The *organismic*, anti-mechanical image of nature. The contemplating Ego, the subject, and nature, the object of contemplation, are both organic entities, and it is this identity of subject and object which makes possible a real understanding of nature. For Schelling nature was an organic structure composed of polarities and contradictions, but unified into a harmonic whole (similar to the ideas of Heraclitus and the Stoics).
- 2) The concept of *wholeness* applies also to the *cognition* of nature. Only a concrete, direct, intuitive approach can uncover the secrets of nature, whereas an abstract reflection, an analysis based on artificial dissections, and making the subject also part of the contemplated object, will block a real comprehension, or may lead the philosopher to a mixing up of his constructions or reflexions with the organic reality.

It is easy to see that Schelling's ideas were in conformity with a certain romantic phase of the French revolution, its organic and anti-mathematical trend, especially its rejection of modern mathematical analysis and its abstract method.

Characteristic of romanticism was also the emphasis on the role of the *genius* in the creative activity of man, in particular of course in *art*. But Schelling also mentioned the part which a *scientific* genius can play in the cognition of nature. For him, and also for Hegel, Kepler was the prototype of such a genius who grasped the *Whole* before the parts, as his three laws clearly demonstrate. In contrast, Newton was the example of a reflecting scientist who reached the same goal by a series of mechanical steps and by introducing elements foreign to the organic wholeness of nature.

In his inaugural paper written in 1801 when Hegel entered the University of Jena, and entitled "De orbitis planetarum," he follows up Schelling's ideas as regards Kepler and his antithesis Kepler-Newton. This paper is full of mistaken and incorrect statements, but some of its ideas are precursors of Hegel's great work of 1817.

From 1804 on Hegel entered a new phase of his philosophic life, breaking with Schelling, renouncing romanticism and building up his panlogistic idealism, based on the dialectical method invented by him. However, it is interesting to note that he took over two essential formal features of Schelling's philosophy and made use of them in the context of his own new philosophy of nature. The one is the romantic rejection of reflective philosophy and the insistence on a direct, concrete approach

to nature. Since for Hegel in his panlogistic phase nature is one of the manifestations of the rule of *Reason* in the world, the essential tool for the understanding of nature is the direct and concrete approach to it through reason, which evolves by a succession of *Notions* (*Begriffe*) culminating in the concept of the Idea.

The second feature is that of contradiction or polarity which Hegel transposed into his dialectical method. One notion is produced by another through the process of thesis, antithesis and synthesis, i.e. of positing the notion, of negating it, and of arriving on a higher level at a new notion.

Since nature is *produced* by Mind, and Mind is not subordinated to nature, Hegel is not interested in the *a posteriori*, in experiments and verification. He is exclusively concerned with the *a priori*, the product of the complete freedom of thought. The facts are self-understood, but philosophy imbues the empirical content with "the essential form of freedom of the *a priori* of thought, and furnishes it with the confirmation of necessity, instead of the mere unification of data and empirical facts." (§ 12).

In another passage (§246) Hegel admits:

"In the progress of philosophical knowledge, we must not only give an account of the object as *determined by its Notion*, but we must also name the *empirical* appearance which corresponds to it, and we must show that this appearance does, in fact, correspond to its Notion." But he adds: "However, this is not an appeal to experience with regard to the necessity of its content. . . . Even less admissible is an appeal to what is called *intuition*, which is usually nothing but a fanciful and sometimes fantastic exercise of the imagination by *analogies*, which may be more or less significant, but which impress determination and schemata on objects only *externally*."

Hegel thus rejects precisely that method by which physics since Newton has succeeded in acquiring a steadily expanding knowledge of the nature of the physical world.

In contrast to the scientific method Hegel undertakes to derive by pure thought the system of nature as a system of "*Notions*." This method of Hegel's consists in the application of his philosophical principles to the philosophy of nature.

In his introduction to the *Encyclopedia* Hegel says that Mind, in the

process of thought, gets entangled in contradictions. However: "thought does not desist . . . *in order to prevail*, and to accomplish the solution of its own contradictions by its very thinking. . . . The conclusion that dialectics is the very nature of thought, that understanding must enter into the negation of its own self, into the contradiction—this conclusion is the main aspect of logic." (§11).

Since logic is the basis of Hegel's metaphysics, his method applies to all three parts of his *Encyclopedia*—the Science of Logic, the Philosophy of Nature, and the Philosophy of Mind. This method is based on the fact that every notion is only of a *partial*, finite, one-sided character; it thus will always contain its own *negation*, and will necessarily lead to a second notion which is its contradiction.

The notion and its contradiction, *thesis* and *antithesis* together, are the one-sided moments of a third notion, which represents a higher level of development, in that it unites them both into a higher unit—the *synthesis*.

As soon as thought has posited this third notion, it again turns out to be finite and incomplete, and its further negation leads to higher notions. These well-known tenets of Hegel's philosophy are also the backbone of his philosophy of nature, which he builds up as a system of notions successively derived from each other to form the idea of nature.

Hegel remains faithful to his triadic principle also with regard to the division of the philosophy of nature. Its three sections are mechanics (divided into space and time, matter and motion, and absolute mechanics—Hegel's name for the theory of gravitation), physics and organics.

I shall mainly restrict myself to a short account of his mechanics, the only consistent and significant chapter of his philosophy of nature which is still of interest to us and where he foreshadowed some of the results of relativity. The rest is a *mixtum compositum* of partly wrong, partly mistaken notions, and of essential and unessential details.

The reason for this is that Hegel's philosophy of nature is in fact a philosophical paraphrase of the achievements of science. He thus succeeded best with mechanics which in the 150 years from Newton to Hegel had been developed into a conceptually and formally perfect system.

Hegel could benefit from the achievements of classical mechanics and

transliterated its results into his own language, criticizing some of its shortcomings, and ingeniously foreshadowing some of its future results.

In all other fields, where physics or biology themselves were still in the process of clarification, or hardly made the first steps towards it Hegel's paraphrases are to a large extent unclear and senseless formulations which sometimes amount to grotesque curiosities.

These parts of his philosophy probably provoked an anonymous positivist at the close of the last century to the following definition: "Philosophy is the systematic misuse of a terminology invented especially for this purpose."

In order to illustrate Hegel's mode of physical thought, let me pick out some of his considerations about space, time, motion and matter.

Space is for Hegel the immediate determination of nature, the fact of its *self-externality* (*Aussersichsein*), its ideal continuous *side-by-sideness*. From this "indifference of self-externality" (§259) Hegel arrives at the *negation* of space, namely to the *point*. This negation is in the first instance of a spatial character, and starting from it one can deduce dialectically lines, planes and other geometrical determinations as negations of a higher order.

But there is yet another aspect of the negativity of the point, namely that of its being *for itself*. In this respect, the point "appears as indifferent to the inert side-by-sideness of space" (§257). This kind of negativity is nothing else but Time, for "the differences of time have not this inert self-externality which constitutes the immediate determinateness of space" (§259).

Obviously the dimensions of time, *present*, *future* and *past*, do not constitute an indifferent side-by-sideness, but they are characterized as "the resolution of [becoming] into the differences of being as passing over into nothing, and of nothing as passing over into being." (§259).

Thus time, according to Hegel, can be defined as "being which, inasmuch as it *is*, is *not*, and inasmuch as it is *not*, *is*" (§258).

To the best of my knowledge, Hegel was the first philosopher who, through his method, established a dialectical concatenation of space and time, and thus foreshadowed one of the basic conceptions of relativity. Time is for him the negation of space, but on the other hand space is also the negation of the opposed moments of past and future, their "collapse into indifference, into undifferentiated asunderness" (§260).

Through its double negativity the point is given as a spatio-temporal entity, and is thus defined by Hegel as *place* (*Ort*), as the posited identity of space and time, as a posited contradiction. Hegel's definition of Place as a "Spatial Now" (*Räumliches Jetzt*) (§261) is indeed more than an ingenious formulation; it is an anticipation of the point in the four-dimensional relativistic universe.

The definition of place (*Ort*) or Spatial Now is one of the few essential innovations in Hegel's philosophy of nature. Some other of his comments on time are also worthwhile quoting. "In nature, where time is a Now," he says, the other dimensions "are, of necessity, only in subjective imagination, in *remembrance*, and in *fear or hope*" (§259). Whitehead, in the footsteps of Hegel has given a similar definition of the present: "The present is the vivid fringe of *memory*, tinged with *anticipation*."^{*}

While discussing the notion of Time, Hegel makes a remark of significance for the philosophy of mathematics: "There is no *science of time* corresponding to *science of place*, to *geometry*. The differences of time have not this *indifference* of self-externality which constitutes the immediate determinateness of space, and they are consequently not capable of being expressed, like space, in configurations. The principle of Time is only capable of being expressed in this way when Mind has paralyzed it and reduced its negativity to the *unit*. This inert One, the uttermost externality of thought, can be used to form external combinations, and these, the numbers of *arithmetic*, can in turn be brought by the Understanding under the categories of equality and inequality, of identity and difference." (§259).

By an analogous train of thought Hegel in his Aesthetics arrives at the connection of music and time. Music abolishes the indifferent self-externality of space, idealizing it into the individual One of the point. This idealization "which appears not more as spatial but as temporal ideality is the tone . . . the abstract visibility of which has been transformed into audibility."^{**} (Cf. Leibniz's "musica est arithmetic a nesciendi se numerare animi"). Indeed, music is the only form of art existing solely in time.

From the notions of space and time Hegel proceeds to that of *motion*,

^{*}A. N. Whitehead, *The Concept of Nature* (Cambridge 1955), p. 73.

^{**}Hegel's *Werke*, ed. H. Glockner (Stuttgart 1927), Vol. 12, p. 129.

of the "vanishing and *self-regeneration* of space in time and of time in space," as he puts it (§261). Indeed, he continues, motion is a process in which "time posits itself spatially as *place*, but in which *place*, too, . . . is immediately posited as temporal."

Here of course Hegel goes along trodden paths, for we know that since Zeno and Plato's *Parmenides* motion has been a dialectical concept *par excellence*, and that Plato was Hegel's precursor in the dialectics of being with its inherent contradiction.

On the other hand, Hegel's interpretation of matter is decidedly original. Motion, so he says, namely the exchange of *this* place by the negation of itself, by *another* place, is pure ideality, pure abstraction. However, this becoming "is itself just as much the collapse within itself of its contradiction, the *immediately identical* and *existent* unity of both (space and time), namely *matter*" (§261).

Matter thus appears to Hegel as the transition from the abstraction of space and time to the concrete existence, from ideality to reality. This transition is incomprehensible to our understanding, and therefore it presents itself as something external and already given.

Hegel rejects the usual conception of space and time which "takes them to be *empty* and indifferent to what fills them and existing by themselves." It is wrong "to regard material things as indifferent to space and time and yet at the same time as essentially spatial and temporal."

Here implicitly begins Hegel's polemic against Newton, first against his theory of absolute space and time, a critique which was already applied a hundred years earlier by Leibniz, from a somewhat different angle.

But Hegel's emphasis on the *identity* of space and time on the one hand and matter on the other goes far beyond Leibniz in his letters to Clark. For him both are the ideal and the real aspect of one and the same thing. This identity is evident according to Hegel from the fact that "ideality can take the place of reality, and vice versa," it can be inferred from the interchangeability of space, time and mass in the elementary laws of mechanics: "In connection with the *lever*, for instance, *distance* can take the place of *mass*, and vice versa, and a quantum of ideal moment produces the same effect as the corresponding real amount. Similarly, in connection with the *quantity of motion*, *velocity*, which is simply the quantitative relationship of space and time, can take the

place of *mass* and conversely, the real effect is the same if the mass is increased and the velocity proportionately decreased. A brick does not kill a man just because it is a brick, but brings about such a result only by virtue of the velocity it has acquired; that is to say, the man is killed by space and time." (§261).

Since matter is nothing else but the real aspect of space and time, it is obvious that it exhibits the characteristic features of both of them. It is *composite*, i.e. it has the property of side-by-sideness or *asunderness*, like space, and it has also the property of existing *for itself*, like time. The union of these opposing moments, the *sundered being-for-self* (*Auseinanderseindes Fürsichsein*) (§262) constitutes the true essence of matter, namely its *gravity*. Thus gravity is again an expression of that contradiction inherent in the whole of being, and it is an essential quality which cannot be separated from matter. On the one hand it expresses itself by the *substantiality* of matter, its *being-for-itself*, which prevents one body from taking the place occupied by another, and on the other hand it expresses itself by the *striving* of a body towards a point *outside* itself, by the tendency of matter of being *self-external*.

For Hegel gravity is the "Urphänomen" of matter (to use Goethe's expression with regard to colors). The motions of freely falling bodies and the planetary motions are immediate expressions of this primary quality of matter, and thus they are "absolutely *free* motions," deriving solely from the conceptual determinateness of space and time. For this reason Hegel pays the highest tribute to Galileo and Kepler whose laws are "discoveries of immortal fame," for they have described these laws in a purely kinematic way, i.e. they restricted themselves to the spatio-temporal aspects associated by Hegel with matter and gravity.

Hegel combines his admiring and detailed exposition of Kepler's laws—which he regards as the simplest concrete embodiment of the idea of gravitation—with detracting comments on Newton's theory. Newton symbolized for him the evil spirit of physics and its defection from the purity of "Notion." He rejects, lock, stock and barrel, the Newtonian dynamics which so magnificently unified all the separate kinematic phenomena into one conceptual system through the vectorial concept of force, including gravitation.

When discussing the movement of a thrown body Hegel takes Newton to task for decomposing this movement into the two incompatible

components of *inertial* motion in the direction of the thrust, and *falling* motion in the direction of gravity. "Such a separation of external and essential movement belongs neither to experience nor to the Notion but only to abstract reflection. It is one thing to . . . represent them mathematically as separate lines . . . , but it is another thing to regard them as physically independent existences." (§266). The method of theoretical mechanics converts "the moments of the *mathematical* formula into physical forces, into an *accelerating* force . . . and into a force of *inertia* . . . , determinations utterly devoid of empirical sanction and equally inconsistent with the *Notion*" (§267). Hegel attacks Newton's "notionless reflection that the so-called forces are regarded as *implanted* in matter, that is to say, as originally *external* to it, with the result that this very identity of time and space . . . which in truth constitutes the *essence* of matter, is posited as something *alien* to it and *contingent*, something introduced into it from outside" (§261).

The historian of science today must regard these considerations of Hegel as a remarkable foreshadowing of a development which eventually led to Einstein's general relativity. Hegel's attempt to eliminate the concept of force and to explain gravitation as the sole immanent expression of the reality of space and time, anticipates in a way Einstein's idea of the geometrization of physical quantities. I am very well aware of the limitations of this parallel between Hegel and Einstein. Hegel did not yet know anything about non-Euclidean geometries, and for him *gravity* was the determining factor in the movement of bodies, whereas Einstein transformed gravity into a generalized *inertia* in a four-dimensional universe of non-Euclidean metric. But both Hegel and Einstein have one idea in common, namely that the Newtonian *dualism* of inertia and gravitation must be abolished and that the planetary motions must be explained as *free* movements. In this respect we see indeed Hegel at the threshold of general relativity.

These are the few, though remarkable insights with which the physicist of today can credit Hegel and his dialectical method.

It goes without saying that in the theory of light Hegel enthusiastically adopts Goethe's theory of colors with his explanation of color as the synthesis of the opposing concepts of light and darkness. And I shall pass over in silence all of Hegel's expositions on chemistry, electricity and magnetism, which would have been less absurd, were they

written fifty years later, after Faraday and Maxwell. Only his definition of heat I should like to quote, formulated after Rumford's experiments and the return to the concept of heat as movement of the ultimate particles of matter: "Heat is the self-restoration of matter to its formlessness, its fluidity, the triumph of its abstract homogeneity over specific determinateness." (§303).

One is tempted to quote Byron's lines on the metaphysical utterances of Coleridge:

"Explaining metaphysics to the nation;
I wish, he would explain his explanation."

(Dedication II of
Don Juan)

In his *Encyclopedia* Hegel states again and again that in the realm of nature *contingency* plays an important role. In nature we find a lot of "properties external to one another and more or less indifferently related to each other" (§250). The reason for this he sees in "the impotence of Nature," which "preserves the determinations of the Notion only abstractly, and leaves their detailed specification to external determination" (§250). He goes on to say: "This impotence of Nature sets limits to philosophy, and it is quite improper to expect the Notion to comprehend—or as it is said, construe or deduce—these contingent products of Nature." (§250).

But this alleged "impotence of nature," which Hegel so much deplores, is nothing else but the impotence of Hegel's method to arrive at the cognition of nature by the exclusive dialectical development of the Notion. The student of Hegel's philosophy of nature has very often occasion to regret that he did not live longer or that he did not live a hundred years later.

Had Hegel lived to see at least the establishment of the first and second laws of thermodynamics, he could not have stated that "the idea of Nature in its piecemeal character subsides into contingencies" (§16). This statement has been refuted by the physical sciences in the 100 years after Hegel's death, during which they succeeded to a considerable degree in purifying step by step the idea of nature from contingencies. These successes of physics were due to the scientific method, rejected or belittled by Hegel, which consists in building theories on the basis of cor-

respondences between empirical data and conceptual constructions—theories of sufficient flexibility and fertility to lead to extensions of their structure and to the possibility of further predictions.

One of the numerous examples of such theories is of course Newton's theory of gravitation which led to Laplace's and Lagrange's theory of perturbation.

Hegel himself had to concede this, although only *en passant* and so to say in brackets: "It is acknowledged that—apart from the basis of analytical treatment . . . —the really material addition made by Newton to Kepler's laws is the principle of *perturbation* . . ." (§270). This understatement clearly shows that Hegel either failed to see or refused to see the enormous difference between Newton's dynamical theory with its potential fertility and Kepler's kinematic laws; he restricted himself to a repeated emphasis on the equivalence of both theories as statements on the planetary orbits.

There is a definite remnant of romanticism in the preference which Hegel gave to Kepler over Newton and his failure to see Kepler as an intermediate, though vital, stage on the way to Newton and to classical mechanics of the 18th century. There might have been also an element of romanticism in Hegel's admiration for the geometrical beauty of the first two laws of Kepler and the simplicity of the proportions of his third law. It was probably the romantic longing for the "good old times" lost for ever, for Pythagoras and ancient and medieval Neoplatonism and its resurrection in Kepler's *Weltanschauung*. Some evidence of this search for the "blue flower of the romantics" can be found in Hegel's words, when he refused "to see with Laplace, only the *confusion* of a visionary *imagination* in Kepler's attempt to arrange the solar system in accordance with the laws of musical harmony, and not to esteem his deep faith in the presence of *Reason in this system* . . ." (§280).

Summing up one may say that Hegel's philosophy of nature is ingenious but sterile, fascinating but not productive; some of its statements are splendid formulations of results already arrived at by science, even occasionally rising to brilliant foreshadowings of future developments, without however furthering the cognition of nature as a whole.

Above all Hegel's method has done such immense damage in that it drove the scientists of the 19th century into the arms of positivism and the equally untenable position of the other extreme, the hostility

to metaphysics. This hostility which is subsiding now, after Planck, Einstein and Pauli, was already subject of a prophetic distich by Schiller in 1796, after he read Schelling, a distich referring to scientists and transcendental philosophers:

“Enmity be between you! Not yet is time ripe for a union.
If each will search for himself, then the truth will be found.”

In a retrospective view, the historian of science must remember the fact that the scientists achieved the unification of their world picture often by changing the direction of their advance, if one road turned out to be impassable, and thus precisely doing what Hegel sarcastically condemned in the method of the scientists: “. . . When they arrive at the limit of their rational progress, they help themselves the easy way. They stop short of its consequences and take whatever they need, often the opposite of the preceding, from the outside, from their imagination, opinion, perception, or whatever it may be.” (§231).

Precisely such a revolutionary giving up of consequences led to the great systems of relativity and quantum physics, which have so immensely enriched our knowledge. These great achievements illustrate what Hegel so beautifully expressed at the conclusion of his Inaugural Lecture in the University of Berlin in 1818:

“The secretive nature of the universe has no power which could resist man’s courage of cognition. It must disclose itself to him, it must reveal to him its treasures and its depths and make him delight in them.”

This was achieved by the method of science, and not by that of Hegel’s system of Notions. But even if we must reject his Naturphilosophie, we should not forget that some of its results, some of its formulations, deserve a permanent place in the annals of the history of scientific thought.

As far as I am concerned, the rejection of Hegel’s method will always be accompanied by an unqualified admiration for the ingenuity of his great mind.

DISCUSSION

On papers by Z. BECHLER and S. SAMBURSKY

MURDOCH: This afternoon it seems to me we have heard essentially about two separate things—on the one hand Mr. Bechler’s paper dealing with issues of methodology and particularly questions of theory acceptance and then on the other hand Hegel. This being the case, I think it is best, unless you think otherwise, that we keep the discussions of the papers relatively separate.

I think Prof. Lakatos has a comment with which he would like to begin the discussion of Mr. Bechler’s paper.

LAKATOS: I should like to make a short plea in defense of Newton. Let me try to explain what seems to me to be the reason why Newton rejected the criticism of people like Hooke, Glanvill and Huygens. The basic reason was this: Newton was, by education, a Cartesian “sceptico-dogmatist” and so when he heard the sentence, “This proposition is unproven,” he automatically associated with it the dictum from the *Discours de la Méthode*, “if unproven then reject as false.” (This I discussed further in my article “Popkin on Scepticism.”)

Now there are two related patterns of sceptical criticism. Both have their roots in ancient Greek scepticism. The first concentrates not on assessing a theory’s empirical success or failure, but on showing that the evidence does not *fully prove* the theory and hence that the theory is to be rejected as false, *via* the dictum which I mentioned earlier. The second type of criticism, which goes back to antiquity, I have previously called the method of *sceptical proliferation of theories*. This consists in casting doubt on a theory by showing that there are other theories which fit the facts equally well—hence the original theory cannot have been proved and therefore cannot constitute true knowledge.

Hooke seems to have employed especially this second method of criticism. I quite agree that Hooke, Glanvill, Huygens and the rest did not think that by showing that there were alternatives to a theory one thereby showed it to be false. In this sense they were not Cartesian sceptico-dogmatists, but held to a weak-kneed sort of fallibilism: they thought that all scientific theories are bound to be fallible. The main

trouble with this weak-kneed fallibilism is that it does not demarcate a dilettante's bright idea from a theory which forms the hard core of a progressive research programme, like, for instance, that of Newton. Newton felt (quite rightly, in my opinion) that simply to write down the inverse square law of gravitation, as Hooke did, is nothing more than irrelevant speculation, mere trial-and-error. The scientific achievement comes with the "positive heuristic" and its progress, which depends to a very considerable degree on mathematical sophistication and detailed, elaborate, theoretical work. Newton held back the publication of the *Principia* for years after he had hit upon the inverse square idea: it took a long time before the brainwave matured into the hard core of a progressive programme. So Newton looked for a demarcation criterion which would demarcate what his scientific sense told him were good theories from what his scientific sense told him were poor theories. However, as I said, the weak-kneed fallibilism of Glanvill *et al.* did not provide him with such a criterion, and he made out a false inductivist case for a valid cause.

Finally, of course Newton's achievements destroyed the two patterns of criticism which I previously mentioned, even though his explicit methodological statements are still in the sceptico-dogmatist vein. Newton's long term impact on methodology is the finest example of how achievements can undermine standards just like great works of art can undermine aesthetic theories. Newtonian science came to be recognized as great, yet it in fact did not measure up to the *a priori* standards of the time, not even to those explicitly held by its creator. So the standards, the methodology, had to change. (My forthcoming book, *The Methodology of Scientific Research Programmes*, will discuss this in detail.)

ROSENFELD: I think there is another aspect in this controversy, namely, on the part of the critics, especially Huygens, there was a misunderstanding about the whole trend of Newton's statements: this was the same confusion which was also made by Goethe later, between the physiological perception of color and the connection of color with refrangibility, which is a physical phenomenon. The two phenomena are equally real and they do not coincide in the sense that, as you know, the physiological definition of color is based on three elements, corresponding to the three kinds of color receptors we have, whose absorption spectra cover wide domains of wavelengths. Huygens was all the time thinking of this physiological aspect and contradicting Newton on this ground.

Newton was clear about the difference, as appears from his answer to Linus, not from the published answer but from a last letter to Oldenburg, which was not printed, at Newton's request, and has only recently been published. There you see clearly that Newton had already made the distinction, and could use it to show that part of the objections of Linus arose just from this failure to distinguish between the physiological and the physical mixture of colors. So Newton knew the situation perfectly well, but he was impatient with his critics. Thus, by his peevish comments on Huygens' observations, he discouraged the latter from continuing the controversy, and this important point remained unnecessarily in doubt. This does not exclude of course your point, which may indeed have played a part, because Newton insisted in all this controversy on what we would call—a horrible word—his "phenomenological" attitude, contrasting with the then current one of making "hypotheses."

The same with gravitation. When Huygens and Leibniz reproached him with reintroducing occult qualities, he protested that he knew very well that there must be a mechanism for the propagation of gravitation, which he could not find. But, he added, it is sufficient that I have established the law according to which gravitation works, whatever its propagation mechanism.

ELKANA: I am in extreme sympathy with Zeev Bechler's attitude of not being prepared to start from the point that Hooke did not understand Newton, and Pardies and Huygens did not understand Newton, and I would like to turn the argument round: I don't accept the other side of the argument either, that Newton did not understand what was going on. On the contrary. I think the beauty of this whole highly interesting methodological debate is that there are two methodological attitudes clashing, which has another element in it—it involves also the methodology of how to carry on an argument. Not only their views on science, (Hooke's and the others), but their views on how to wage philosophical battles were more tolerant than Newton's methodological attitudes both to doing science and to carrying on a battle in philosophy, which was much more dogmatic. Newton well understood what they meant, but his attitude to the argument was the following: "if you want to carry your point, then simply do not shift, make as if you do not understand and just repeat it again, and again, and again." Newton was doing this systematically always all along the line. This is one point.

The other point is that I still think that Huygens cannot be put in the same class as the other two opponents of Newton because methodologically I think Huygens was much nearer to Descartes than the others. Between Newton and Huygens it was a scientific discussion in the same methodological school, and not a clash between two methodological schools.

AGASSI: I wish to misunderstand Prof. Lakatos. Descartes recommended the rejection of all uncertain theories as false. Newton, who did not want his doubtful theory to be rejected had no option, says Lakatos, but to reject Descartes' recommendation. But he had an alternative to the rejection of Descartes' recommendation, and that was to claim certitude for his theory.

Unlike Lakatos, I do not see how Newton could reject Descartes' recommendation. Newton could not overcome the methodological difficulties of the period any more than others (except perhaps Boyle). There is a genuine methodological problem here which they could not solve. Joseph Glanvill, for example, in his attack on dogmatism, says he doesn't want to accept as certain any theory which has no proof—and he admits there is no proof to a lot of theories. Yet, he feels, he has to explain why he accepts any unproven theory. Although he is not an instrumentalist, when he comes to this, he becomes an instrumentalist: instrumentalism, he implies, is the only way to justify the endorsement of an unproven theory. This position has an ancestry—see Edward Rosen's preface to his *Three Copernican Treaties*. Copernicus didn't want his heliocentric theory to be viewed as a mere instrument, an idea to be treated "as if" true; he therefore didn't want heliocentrism to be viewed as a mere hypothesis, and he therefore claimed that heliocentrism had been proven. It is the same mistake that almost all classical thinkers made: they didn't have the category which Prof. Lakatos rightly says should exist—"unprovable yet not to be rejected off hand." They could not justify such a category, as they felt they should, so they repeatedly denied its existence. For almost all thinkers of the 18th century (the exception is Boyle and perhaps Hooke), the hypothetical nature of any theory was the same as its "as if" quality.

By Descartes' rule of method, of course, an "as if" theory ought to be rejected. Had Newton rejected Descartes' rule according to which whatever theory is unproven should be rejected, he would have said:

"though my theory is not certain, don't reject it off hand." What he said, rather, was that his theory enjoyed the status of absolute certitude (even though it might have to be further explained).

ELKANA: Let me pursue my point further—that acting as if not understanding is the style of argument at this time: I think the best source for this is the political arguments of the time. This kind of dealing with arguments—"do not even try to understand what the opponent says"—you can follow, for example in the Kimball volume; two volumes of correspondence, not just scientific but state letters, etc., in Newton's time. It is systematically shown in political letters between Germany and England for example, that one partner to the argument must understand what the other means, but he does not want to understand and then he goes on answering exactly the same point again and again and again as if the other hadn't said anything, and this repeats itself. This is also how the Royal Society brought about a systematic change in scientific values—there was absolutely no argument going on between the Royal Society and the outside world, but just repeated statements again and again and again at the beginning of every single lecture until it penetrated systematically, without them even wanting to answer people who would have opposed it.

LAKATOS: I am afraid you may have missed the point. All this has nothing to do with the political arguments going on at the time. Professed scientific standards evolved from *theological* standards—it is very dangerous to utter a theological falsehood since then one will have to burn in hell for eternity. Given this, the safest thing to do is to reject anything about which one has the slightest doubt. These standards entered into science, because no one had the logical or epistemological equipment to develop a methodological hard-headed fallibilism on 20th century Popperian lines, which as well as being fallibilistic, can distinguish between a dilettante exercise (like that of Hooke) and a real scientific achievement (like that of Newton).

Now the only thing that can happen in a situation like this—where actual scientific *achievements* do not measure up to the professed and accepted scientific *standards* and the theory of standards (the "methodology") has not yet got adjusted—is that a false awareness develops. Newton's theory comes to be seen as a "proven" theory. What Zeev Bechler and Yehuda Elkana seem to regard as a psychological stubborn-

ness in repeating one's position is in fact an ostensive display. What else could Newton do? His achievements set the scientific standards, he simply had no means of explaining *why* his achievements were great.

ELKANA: The Royal Society created standards before this.

LAKATOS: If you read Boyle's *Sceptical Chymist*, then you will see that on the standards of the early Royal Society Newton would not score particularly highly. Surely even today we are still debating why exactly Newton's achievement is great and why astrology is not great.

ELKANA: Standards changed all the time—they had a standard which Newton then changed.

LAKATOS: Then you agree with me that there were standards inherited from theology and from the whole sceptico-dogmatist debate, and Newton was at the same time seen *both* to be great and yet to be falling down on the standards of his contemporaries—falling down on the *a priori* standards of intelligibility, of proveness and so on, of his time.

MENDELSON: The standards of the time had many different sources. I mean surely this must be the difference between history and reconstruction!

LAKATOS: Newton's achievement by its sheer weight demolished the standards which in general were accepted by the leading intellectuals of his time. It was an ostensive definition—science was created before the standards of science were created.

MENDELSON: No, science never existed without standards but I think what you are saying is they were articulated. I think if you look at the 17th century what you will find is a plurality of articulated standards and the point comes out. I suspect that what seems to be prime varies, not only does it vary across the English Channel, but it varies through valleys across England, to put it in a direct fashion. The very simple model you give of the derivation of standards from theological debate just makes no sense in 17th century England where you also had major parliamentarian conflicts at the time which were imposing enormous intellectual strains. I would not at the moment pick either of these as the sole source for the nature of argument in science.

LAKATOS: Your argument would only be interesting if you could specify a pattern of standards which came from British political life and not from theology, as I claim it.

MENDELSON: O.K. let's sit down with Sprat's *Apologia* and I will take you through chapter and verse of the existence of both of these.

TOULMIN: The answer varies, depending on which of Newton's theories you talk about. Lakatos is talking about the *Principia*, whereas the paper was about Newton's very early ideas about optics.

But even here there is a difference even at this level between what Newton is doing with the phenomena of optics, and with the complete system of laws of motion and gravitational hypothesis; so you can't set on a single method or standard as applicable even in 1672.

ROSENFELD: There was no method in existence and Newton was very consciously realizing that he was constructing a method.

HOLTON: Newton had one more important thing on his side—he *knew*, and I liked that in Manuel's book on Newton: he was a chosen one, one of the elect. He was not a man to offer himself for debates with just anybody; he was bringing down the Truth. And this aspect is characteristic of certain types of genius in the sciences (though not all, I don't think Galileo or Bohr had this feeling—those were rather dialectical people who had to understand themselves by arguing with others). For one of the elect, the way to handle a dispute was largely to insist on his point of view. Now that coincides well with their liberal idea of what a theory is all about. As Einstein says, the purpose, the characteristic, of a good theory is, first, that it is *not in conflict* with experiment—he does not say that it is proved by experiment—and secondly that the creator of the theory feels that it has a certain internal harmony.

It is of course an empirical fact that this attitude can be valid and fruitful when held by these very exceptional people. The myth of science as an open-minded discussion among equals who bring experimental proof or calculation to each other and, so patiently, harvest together the grain of truth out of it all—this may apply at the levels below the one which we have now considered.

MENDELSON: Let me just ask a simple but direct question on something just like this. You say that it is these great men who spoke and therefore in a sense imposed their view—they were not to be questioned. I really wonder historically whether the reason you can say that is because you don't know that the lesser figures who turn out in history to be judged wrongly did not do the same thing. I have the feeling that this personal attitude is not confined to genius and that what you are doing is saying that in genius this happens to be present now and again. This is an important distinction.

HOLTON: It is obvious that Newton is different from the people who

try to engage him in an argument. And he knows it and says to them: I do not want to engage in your arguments.

ELKANA: I accept your typological remark, except that even those types of people don't behave exactly the same in each century. They choose from the existing standards the one that suits them best. Newton had something he could choose, he had available standards from which to choose.

HIEBERT: And the standards are not necessarily those accorded to scientific activity.

BECHLER: Lakatos made some distinction between Cartesians and fellows of the Royal Society and pointed out two different methodologies—Newton's which was somehow Cartesian versus a Royal Society methodology. I only wish to point out that I don't believe that Descartes and most of the Royal Society (except Newton) had two different methodologies and I argued this at length on another occasion. Descartes did not offer a new methodology—he offered a new articulation of an old scientific ideology which had already been expressed for example in Plato and Aristotle. On the other hand when he comes to talk science, that is to say for example at the end of his Principia, he stresses that what he has to offer is not certainties and insists that no one should attribute to him the contention of proposing certainties to the world since this is only a theory. He has the analogy of decoding a code, which he employs in order to stress that the success of a code cannot constitute a proof of its truth.

Among the Cartesians this was an accepted etiquette—when talking ideology one may use certain slogans. The aim of science is certainty and certainty alone. On the other hand, when you do science such slogans are out of place. This is principally a matter of etiquette.

Now what Newton had to offer against this etiquette truly was a new methodology, and I hinted that I have some theory about what he had to offer. I hinted that it was his desire for mathematization—which answers Lakatos' objection that I ignore the demarcation principle that Newton had and they had not. Well, Newton had a demarcation principle which he thought was a new one—but it was not. Already Copernicus had it and he offered it to the world as being the “harmony” and uniqueness of his theory, but people like Descartes and Huygens and all the Royal Society members simply did not accept this new methodology

as a final one since they saw the inherent irrationality of such a criterion.

So they understood quite well what Newton was going to say, or what he intended to say. For example, Hooke understood quite well the existence of a mathematical background beyond this finality criterion that Newton implied. But he simply rejected it as irrational—that is all. He said you can't bring in such criteria at this time, 100 years after Copernicus, or 80 years after Copernicus was refuted by Tycho Brahe for example, who showed that there was another theory as harmonious as that of Copernicus. So the whole business of harmony and uniqueness had no rational ground on which to stand. That was the whole point of the fallibilism of the Royal Society members.

LAKATOS: What is your evidence that harmony and uniqueness had anything to do with Newton and mathematization?

BECHLER: That is the whole point—when Newton says that the mathematization of a theory is the proof of it—the proof of its certainty—he always bases this on the claim of its uniqueness, or the impossibility of building another theory as harmonious as this. For example, in this debate he points out that Hooke's alternative is no alternative really—and that is his argument. He says there are no alternatives—therefore it is the only theory that is certain.

So there is an asymmetry between the two sides. Newton has the same sort of blindness that Copernicus had and all the geniuses in history had, and which the mediocre scientists did not have simply because they did not need it.

MURDOCH: Let us now leave the 17th century, and come on to the 19th century and Hegel.

HIEBERT: I think the seriousness with which we take, for example, Hegel's analysis of mechanics, depends a great deal upon what the basis of his remarks was. There are a number of alternatives which I might suggest and with Shmuel Sambursky's reading of Hegel I hope that we can take the problems seriously in one way or another. Was he trying to be funny? I don't think so. Was he insincere? I doubt that. Or don't we understand the words that he used? Possibly. Was he stupid? I don't think so.

Now the question that I would like to raise is whether it is possible that one of the problems was that he had misconceptions, that his knowledge of mechanics was poor. Now if Hegel knew the mechanics

of his day very well, and if there is evidence in his writing that he had in a way mastered mechanics, and then made these statements in spite of that—that is one thing.

But—and you know the literature is full of crazy statements based upon scientific notions—if he did this on the basis of a poor mastery of the mechanical details, then I think one would look at the statements in a little different light.

So I am asking you Shmuel, to what extent did Hegel master the principles of mechanics that come down in his writings? Or do you find that he did not really understand mechanics?

SAMBURSKY: Well, Hegel really read Lagrange and he read him very well. Of course he did not yet know Hamilton—Hamilton wrote years after Hegel's death, but the latest word in classical mechanics in Hegel's time was Lagrange, and he understood Lagrange and he understood calculus and the equations of Lagrange and still he believed that the whole of classical mechanics based on Newton's idea of force—the vectorial idea of force—was wrong.

HIEBERT: Mathematically, or conceptually?

SAMBURSKY: Conceptually wrong. He said that if we make mathematical calculations we must forget the physics, and when we come to physics we must forget the mathematics, and we must not be tempted to introduce mathematical conceptions into physical considerations—the mathematical apparatus is only "Hilfsapparat."

HIEBERT: He is imposing on the Newtonian conception his own particular categories.

SAMBURSKY: Yes, absolutely. The empirical is self-understood and therefore he was not afraid of refuting his own conclusions of twenty years before—he said "now we know more—we must only comment in my philosophical framework."

HIEBERT: Do you think he read and understood Lagrange? To quote Lagrange is not enough. I am asking whether in his writings you can see whether he really did somehow master what he is talking about from Lagrange in his discussion?

SAMBURSKY: It is a high probability that he understood it, yes. But he still believed that this specific method of science, which proved to be so fruitful, was wrong, this idea, this going to and fro between the observational plane and the conceptual plane—this pendulum movement. He disagreed with that in principle.

LAKATOS: I am sorry to have to say that some of the things that I have been hearing today are intellectually dangerous. If one were to set a monkey typing for long enough, he could no doubt produce a few sentences which some clever and informed historian could interpret as significant anticipations of great ideas, like relativity theory. The probability of a monkey typing out a short sentence like "Time and space are interconnected" is of course a billion times more probable than his typing out Einstein's huge research programme with its immense conceptual and mathematical sophistication. To claim that Hegel's empty "dialectical" rubbish in any sense anticipated Einstein is irresponsible propaganda for the foremost enemy of Reason and perhaps the foremost ancestor of fascism in the nineteenth century.

ELKANA: I want to add first of all that I think the most interesting point is first of all to verify—whether these people really understood their science or not. If you are sure on that point, after that the most interesting question becomes why did their scientific discussions constitute a progressive shift in some cases at some times, while it created a degeneracy in other fields at other times? To be explicit, I do not think that without Hegel and Schelling and Fichte conservation ideas would have developed in the 19th century as they did—on the other hand their ideas led to a complete degeneration in German electrodynamics.

MENDELSON: I come in here really to ask a couple of questions. One thing that I sense being done is an attempt to outline history as it should have been, and I worry about this, in that it was not that way so far as we can tell. I am interested in the whole problem that Yehuda began putting his finger on and I think it is worth pushing further. In what context is it that a group of individuals spanning the sciences can come to their study of nature with a methodology, or methodologies, drastically at odds with what went shortly before them and with what was going on simultaneously in other countries? Why is it that the understanding of mechanics in a Hegel takes the direction it does? I don't know whether Hegel understood mechanics well enough or not, but the implication is that he truly did. Schelling understood his science, certainly the biologists among the natural philosophers ranging from a classical Friedrich Lower to a Lorenz von Behr understood the phenomena with which they were dealing as well as the next man, in some cases better. But underlying what they were doing was a dramatically different perspective as to how you deal with nature, how you talk about it, indeed how you work with it.

What I sense happening here are several things. One is the creation of an ideology, and in a sense Hegel stands as a major figure in an ideology of science, and what I think we have got to begin to ask about, in attempting to understand the aspects of Hegel's writing as an ideology of German science at the time, is the relationship of that to the internal practice of German science in terms of the values used for measuring evidence. I don't know how clearly they go. I can read the biologists—the biologists used values in measuring evidence not that distinct from their materialist counterparts in France at the same time, but the manner in which they reconstructed or developed that evidence was radically different—they saw it leading in other directions.

So that we really come to an underlying problem of why is it that science was explained in that fashion in a strong mode and taking it so far from what we would like to see as the antecedents of today's science. Indeed, the very next generation had a different science.

ROSENFELD: Since it is a well formulated question, I will try to answer it. You can only understand what happened in Germany at that time if you take account, to speak in Hegelian terms, of the deep contradiction in German society, which was reflected not only in Hegel, but in the whole of intellectual activity. Take the case of Ritter: he was a physicist—at least he dabbled not without success in electrochemistry—and he was a "Naturphilosoph" of the most romantic description. What was the reason for this contradiction? Well, the romanticism was the expression of the disillusion felt by the German bourgeoisie in realizing its political and economic backwardness; on the other hand, there was even in Germany an effort to follow the trend of what we call the industrial revolution and a realization of the role science had to play in this process. Ritter was sensitive to both moods, so he was a completely split personality, and that is reflected in his work. He made pioneering chemical experiments that led him to a formulation of essential features of electrochemistry; but at the same time he was mixing this up quite uncritically with the wildest speculations and he did not in his own mind distinguish between fact and fantasy. He missed a big prize of the French Academy just because of this confusion. Anyhow that was just an example.

But I feel that these people were the victims of their environment, if you can call them victims. They were at any rate motivated by this deep contradiction that was dominating the scene in Germany. It was also

present in France—after all there were also romantics in France—but to a lesser extent: the difference was the existence in France of a very powerful scientific school, with the towering personality of Laplace.

I think it is only against this background that you can find some logic in this madness.

LAKATOS: You are trying to *explain* the Hegelian movement, but explanation is no excuse. Also you seem to be talking about a unified *Zeitgeist* of the romantic period. But in fact there are different rival schools of thought, even within Hegelianism.

TOULMIN: One can give an answer in terms of the content of what was going on here. Firstly, if we consider what we say about Hegel in relation to mechanics: Hegel is clearly not trying to contribute to mechanics in the sense in which Hamilton was contributing to mechanics. What he was doing was giving an *interpretation* of mechanics, and the important question therefore is what *kind* of interpretation, in relation, to what other kinds of questions. And he is giving an interpretation of mechanics which is in a tradition which goes back to Kant—nobody has mentioned the *Metaphysische Anfangsgründe*—and back to Euler too.

Now, suppose we ask what kind of interpretation. I think it is an interpretation made with an eye to a certain kind of epistemological dispute with which German *Naturphilosophie* was preoccupied: just as Goethe was preoccupied with light and optics with an eye to the experience of perception rather than to the physics of transmission—the sensory physiology and sensory psychology of perception rather than physical optics. Many of the German scientific intellectuals of this period felt rightly or wrongly that a scientific issue was not completely dealt with, unless you could put it in its epistemological or perceptual context. So I think that Hegel's mechanics is still nonsense, but not *contentless* nonsense: I think it is still wrong, but wrong in a way that we can make sense of. One can see what is going on, if only one looks at it in relation to this whole obsession with the perceptual and epistemological aspects of science, and with the necessity to embed what the English would regard as the "objective" explanation of any situation into its epistemological framework.

I think Helmholtz is the classic example here. Helmholtz is clearly indebted to the German philosophical tradition in many, many ways; and a treatise like his *Physiological Optics* should not have come out

of England in the 19th century; for the very reason that, in England, this sharp division between the things of the mind and the things of the material world made it impossible for English scientists in the strict Newtonian tradition to face the problems of physiological optics in the way Helmholtz was able to.

HIEBERT: I think that if you take seriously the interpretation of science and the dependence of the positivists on Hegelian dialectic, as for example the notion of organic wholes and anti-mechanistic notions and so forth—if you want to understand Marxism, if you want to understand Helmholtz, if you want to understand the positivists—you have to read Hegel.

ROSENFELD: Oersted under the spell of "Naturphilosophie" lived for more than 20 years with a potentially fruitful idea in his head—he could not get any concrete result. When he broke this spell under the influence of the French physicists, he made his great discovery.

In this connection, I have made a study of Oersted's work, which will soon be published, with all the documents justifying my statement.

JAMMER: The same applies to Faraday. You can trace the exact transition from the "Naturphilosophie" to Faraday—step by step. But I doubt whether this has any relation to Hegel as such.

MURDOCH: I think that maybe it is time for a few parting words, from the speaker.

SAMBURSKY: I listened to this discussion of course with a bit of mixed feelings, because I seem not to have been able to persuade my friends to see anything serious in Hegel and I believe that Hegel was not an impostor, nor an arbitrary juggler with concepts. He was an extremely serene, serious philosopher who tried to build up a self-consistent system and I brought to you—I hope you don't forget it—a few things, a few paraphrases of the achievements of mechanics that seem to me to be the essentials. I think that Hegel is really the first man in the history of science who achieved a concatenation of space and time—neither did the late neo-Platonists do it, nor did Leibnitz do it. You can reject his method, but by his dialectical method he came to the Raumzeitpunkt.

Now if we talk about historical influences it is of course clear that Hegel's philosophy influenced people who later became anti-Hegelian, as for instance Helmholtz. The idea to have comprehensive laws—I mean the idea of Robert Mayer and Helmholtz which was rejected—was a direct descendant of Hegel's ideas.

I believe that Hegel—who failed finally—made a serious attempt to build up a whole *a priori* system of nature. He did not care for the *a posteriori* at all, he again and again emphasizes it. And in a few points he succeeded and I think that some of his formulations are interesting and even correct.

And I wanted lastly only to say this of Hegel with his idea of dialectics—that of course what happened after his death proved him to be right, because the process of science is a dialectical one. As we know today, scientific thought—if you consider just the mechanistic view which was overthrown only when it came to its complete fulfillment—is a real dialectical process. At the moment when the mechanistic view reached its climax, it then turned out to be impossible and it was replaced by other views, and that is an absolutely essential and genuinely dialectical process and probably some of us will see in the future how the developments go.

So I can only recommend to you—read a bit of Hegel.

THE END OF THE PARMENIDEAN ERA

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I

Philosophers, scientists and historians alike are happy to meet and honor our friend and colleague Shmuel Sambursky for one very special reason. While his historical work has been excellent as intellectual history, and his scientific work as natural science, he has had the further, quite unusual merit of looking beyond all the particular discoveries of science and history to the more general ideas and methods which give them significance. In doing so, he has taught us about the relations between science and philosophy, in a profound sense: i.e. about the relations between the philosophical theory of knowledge, on the one hand, and the scientific practice of knowers on the other.

By reading *The Physical World of the Greeks* and *The Physics of the Stoics*, we have learned to look at ancient philosophy with new eyes. In his hands, metaphysical issues cease to be merely stratospheric, and acquire a new relevance to the scientific preoccupations of the philosophers concerned. His work has thus provided countless illustrations to establish that, where the enterprise of natural philosophy gives rise to metaphysical problems or discussions, these are only one face of a coin whose reverse face is methodological, if not straightforwardly theoretical.

As Shmuel Sambursky depicts him, for instance, Plato the metaphysical realist and Plato the mathematical astronomer were not two men joined by an historical accident, but one unitary human grappling with related aspects of a single intellectual enterprise. Of course, an astronomer committed to giving his explanations in geometrical terms did not need to equate the properties of his fundamental concepts (or "ideas") with those of any actually existent material objects. The validity of Euclidean geometry or Newtonian planetary theory does not require that any actual straight line, or elliptical orbit, exists which exemplifies

with perfect exactness the relations analyzed in that mathematical system. In this sense the existence of Ideas is quite independent of the existence of material or sensible objects. Conversely, Aristotle the metaphysical essentialist was by no means independent of Aristotle, the zoologist and marine biologist. There is, of course, no room to theorize in terms of the essential nature of Man or the dogfish, or the planets, unless there do exist in actual fact certain authentic exemplars of those explanatory notions. So, for Aristotle, Ideas or Essences had evidently to be embodied in particular objects.

Nor have the lessons Sambursky has taught us been confined to his beloved Greeks. Just because of his pervasive methodological insights, his commentaries on the science of antiquity have always carried with them overtones relevant to the science of our own era. His analysis of the issues dividing Democritus and the Epicureans from their Stoic critics and opponents refers implicitly to the issues over which Boscovich was later to take issue with Newton as well; and, equally, to the difficulties over the wave-particle duality in twentieth century quantum physics.

So, while Collingwood may be right to argue that metaphysics is an historical science, Sambursky's work reminds us that there are also certain patterns of theoretical debate—of which the dialectical alternation between “particulate” and “field” theories is only one example—that have recurred repeatedly in the historical development of scientific thought in many different, and empirically unrelated fields of investigation and explanation. (This same alternation has made its appearance in genetics as well as in physics; and it is currently being replayed in the field of molecular biology where, in its detailed application to the fact of intra-cellular synthesis, the Central Dogma of Crick and Watson—viz. that there is a specific and general coding of DNA for RNA and of RNA for proteins—is having to be qualified by respect for “position effects.”) In many different fields of investigation, it appears, certain modes of theorizing and explanation—certain “themata,” as Gerald Holton has called them—still present themselves to us today, as much as they did 2,500 years ago, as being “absolute and pleasing to the mind.”

So Sambursky's seventieth birthday is a legitimate occasion to take up some general issues about the relevance of the classical Greek ex-

perience in science to our own scientific situation. I shall here argue three theses in turn. These are (1) that the programs for scientific theory formulated by all the Greek natural philosophers—with certain qualifications in the case of Heraclitus and the Epicureans—presupposed, as the *a priori* scaffolding for rational enquiry, an ahistorical framework of principles, of which the most important I shall call “the Parmenidean axiom”; (2) that the classical physics and biology of the modern era were constructed within the intellectual scaffolding so erected; and (3) that the conceptual changes that have taken place in scientific theory within Sambursky's lifetime—what is more, the epistemological changes that have taken place in our understanding of scientific theory as a result—have at last put us in a position to dismantle that scaffolding, and to continue the construction of natural science without regard to those axioms and principles.

This is not to say that, over the last 70 years, scientific theory has at last emancipated itself from its earlier dependence on metaphysical assumptions. It is to say, rather, that there has been a fundamental change within the philosophical assumptions of working scientists: one which has modified, at a single stroke, their beliefs both about the ultimate constituents of the world, and about the relationship of our fundamental explanatory concepts to the natural world they are used to explain. As a result, the possibility finally opens up of carrying human thought beyond a point at which it has halted for some 2,000 years: the point at which—if we may stand Whitehead's epigram on its head—philosophy has been condemned to be “a series of footnotes to Plato.” Now, in the late twentieth century, we may at last be witnessing the end of the Parmenidean era.

II

By “the Parmenidean axiom,” then, I mean the following doctrine: that behind the things that change (the appearances, or phenomena) there lie things that do not change (the realities, or elements) and that the task of explaining the appearances, or phenomena, always requires us to see behind them to the eternal realities, or unchanging elements, of which those phenomena are the transitory manifestations.

Now, Sambursky has shown us very beautifully how the rival systems

of the pre-Socratic philosophers can be seen, not as wild, irrelevant and pre-scientific speculations, but rather as alternative recipes for constructing a "rational" natural science—a natural science, that is, which provides a rational explanation by this Parmenidean standard. For all the Greek philosophers, the underlying "elements" of the world (*stoicheia*) were also "principles" or "roots" (*archai*): the central disagreement between them was over the question "What *are* those elements and principles?" Are they one or more basic states, or kinds of matter? Are they mathematical forms? Are they unit-particles? Or are they, perhaps, a bit of all three?

Seen from this point of view, the famous Problem of the One and the Many, "How do the Permanent Entities of the natural world preserve their identity despite all the Appearance of Change?", turns out to have a methodological reverse: in the form of the question, "When we set out to make the actual Appearances of Change intelligible in a coherent, consistent and rational manner, what sorts of Permanent Entities should we postulate as the unchanging elements or Eternal Realities of the natural world?" And my first thesis is this: that, for the great majority of Greek natural philosophers, *the validity of this methodological question never even came under examination*. It served (in Collingwood's phrase) as an "absolute presupposition" of all Greek science: that is, as a prior requirement to which any scientific theory claiming "rational" status was obliged to conform.

This is true not just of the pre-Socratics, but of Plato and Aristotle also. Over this particular issue, their work introduced no real changes. Neither man went so far as to challenge the original pre-Socratic question, "How is the identity of the permanent realities preserved through the appearances of change?" All they did was to argue for the Ideas or the Essences, as alternative solutions to the same old problem: as alternative "elements and principles"—i.e. two further ways of tackling the traditional metaphysical problem, coupled with two further recipes for building a rational science of nature. Much later, both recipes were to have a profound influence on the historical development of modern science also, though in different fields of enquiry. And both recipes committed their advocates to certain very general existence-assumptions. For Plato and Aristotle alike gave detailed specifications of the requirements to be met by the ultimate explanatory entities of science; according to

both recipes, a rational science of nature takes for granted the possibility of recognizing in nature the existence of some particulars answering, at least approximately, to the specification in question; and, in both systems, this assumption operated as an unquestioned presupposition, for which an intellectual underpinning could be found only outside the realm of natural philosophy.

On the one hand, Plato's models of knowledge being geometrical, all that he could truly *know* was (for him) a timeless system of mathematical propositions; and our experience of changing phenomena could be pulled together into a single intelligible whole, only in terms of this timeless structure. Correlatively, Plato's conception of the objects about which we have knowledge was also timeless and mathematical. What we fully understand is (e.g.) the possibility of analyzing an eicosahedron into its 20 faces, and reconstituting them to form 2 octahedrons and one tetrahedron: the best instances of three-dimensional objects about which we can properly be said to have knowledge are such geometrical figures as these. If we go on to identify these figures as the atomic shapes of water, air and fire respectively, that identification will be only speculative—a "likely story"—as Plato put it in the *Timaeus*. But, granted that identification, we shall then have an illustration of genuine knowledge about the physical world. Normally, again, such identification of sensible objects with underlying realities will be only approximate—material things, Plato says, are never *tode* but only *toiouto*, never "just so" but only "ish." (A table is never "a circle," but at best more or less "circular.") Still, the possibility of physical knowledge, i.e. the possibility of our understanding of Forms becoming an authentic knowledge of nature, did depend on some such identifications being correct: i.e. on the existence of some physical objects or systems whose behavior could be seen as exemplifying the underlying geometrical relations and realities to a sufficient degree of accuracy, and so understood.

On the other hand, Aristotle's model of knowledge was taxonomic. What we truly *know* is an unchanging set of correlations between the observable characters of actual objects and systems, and it is by appeal to those correlations that we ultimately make sense of "appearances." Correspondingly, with Aristotle's account of the objects about which we have knowledge: what we fully understand is the unchanging necessity for an acorn to develop into an oak rather than a vine, and a grape-pip

into a vine rather than an oak. So the kinds of things about which we can properly be said to "know" are the essential relations manifested in the life cycles of animals and plants. Once again, problems may arise about our capacity to recognize the essences of particular species, or to identify actual instances of those species for what they are. Still, leaving these difficulties aside, the possibility of scientific knowledge, i.e. the possibility of our knowledge of "essences" being not just a grasp of logical entailments but an authentic knowledge of nature, did depend on the assumption that some such species in fact exist, and that they can in fact be characterized in terms of their unchanging essences.

III

So much for my first thesis: viz. that the natural philosophers of antiquity—by and large—took the Parmenidean axiom for granted. Now let me take up the second thesis: viz. that this Parmenidean axiom in due course served as a scaffolding for the classical science of the modern era. Turning to the founding documents of modern science, one can at once produce evidence of this. In the crucial years around 1700, the implications of both the Platonist and the Aristotelian programs for science were made explicit, and in both cases their final underpinning was—inevitably—located outside natural philosophy: specifically, in theology.

Newton's position illustrates beautifully the logic of the steps by which the implications of the Platonist program worked themselves out in practice. In his famous Scholium to the Definition at the beginning of the *Principia*, he declared that our knowledge of space, time and motion is of two distinct kinds: "absolute" or "true," which he equated with "mathematical," and "relative" or "apparent," which he equated with "sensible." But he did not restrict these Platonist conclusions to kinematic notions. On the contrary: they applied equally to all theoretical knowledge. We have absolute, true or *exact* knowledge only of the mathematical truths and relations in the sciences. Sensible knowledge of appearances is always relative to arbitrarily chosen empirical measures of the fundamental mathematical magnitudes or realities, and is never more than *approximate*.

Unlike "time," the mathematical magnitude, about whose constant rate of passage no question can even be posed, measured "time" flows

at best more-or-less "equably," depending on our choice of a standard motion (or clock) to serve as an index of this passage-of-time. Similarly, when we move on to Newton's substantive axioms and theorems: "mass" and "momentum," "force" and "motion," also have the same two aspects. We can treat them as mathematical magnitudes, about which our understanding is exact; or we can alternatively treat them as incorrect or "sensible" magnitudes, about which our understanding is more or less approximate. What we fully understand is (say) the timeless relationships between ellipticity of trajectory, inverse-squareness of central force, and so on; and such mathematical forms and relations are the true "realities," or objects of exact knowledge.

How this exact knowledge enables us to explain appearances, or "sensible" phenomena, Newton shows in Book III of the *Principia*. For there he moves on from axioms and exact theorems, and points to the observations of men like Kepler as exemplifying the mathematical relationships proved in the two preceding books accurately enough to justify the necessary identifications of particular physical systems with the corresponding general mathematical specifications. Given these observations, it is (in Plato's sense) a "likely story," or (as Descartes would have put it) a "moral certainty," that the actual physical planets—Mercury, Venus, Mars etc.—represent, nearly enough for theoretical purposes, "bodies moving freely under an inverse-square, centrally-directed force only." But that identification is problematic, in a way that the theorem is not.

As in Plato's case, however, Newton could go beyond dynamics to construct a comprehensive system of nature philosophy only by making some very general *existence* assumptions about the ultimate constituents of the natural world. These concern the famous "solid, massy, hard, impenetrable, moveable Particles" argued for at the end of his *Opticks*. As Anaximander had already understood, the unchanging properties of the ultimate element or "realities" cannot be accounted for in the same kind of way, or in the same terms, as the physical "appearances" which are in due course explained by appeal to those properties. Newton, too, saw the force of this argument; so it was at just this point that he turned from physical science to theology. If the ultimate Particles were in fact "so very hard as never to wear or break in pieces," that is because "God Himself made (them) one in the first Creation"; and, this being so, "it's

unphilosophical to seek for any other Origin of the World," or to look for a naturalistic account of the manner in which the ultimate Particles and Laws of Nature themselves came to be as they are. For, after all, any such account would seemingly discredit the essential claim of those things to be the "ultimate, unchanging and underlying realities": i.e. the final level of explanatory analysis.

If Newton put flesh on Plato's skeleton program for science and gave it a more specific and detailed application than it ever had in antiquity, the same was done for the Aristotelian program by the naturalists of the late 17th and early 18th century. The clearest and most articulate of them was John Ray. Far from all pre-Darwinian taxonomy being dominated by Aristotelian essentialism (one must recall) the doctrine of fixed species took a definitive hold on botany and zoology *only after 1600*; and it was men like Ray who worked out the implications of this doctrine explicitly. For Ray, organic species were indeed among the "true realities" of Nature: their unchanging characters were the underlying principles by which the changing appearances of organic life was to be explained. Yet—as before—these essential realities could claim to be "ultimate," only if they were themselves accounted for in quite different terms from the appearances which they explain. Faced with this problem, Ray too turned to theology. In appealing to *The Wisdom of God* as manifested in *The Works of the Creation*, Ray was not just adding a pious frill to an otherwise empirical body of natural history. He was providing the intellectual underpinning on which his whole system of natural philosophy was dependent, both for the cogency of its zoological arguments, and for the authenticity of the resulting biological knowledge.

So, in both the classical physics and the classical zoology of the 18th century, the Parmenidean axiom retained its hold. The basic task of scientific explanation was still to relate "changing appearances" back to underlying "invariant realities"; and the origin of those invariant realities was not a question that could legitimately arise *within* the scientific field. Newton's "Particles" and Ray's "Species" were, in the event, only the two most fundamental out of the dozen-odd invariant elements which the men of the 18th century saw as constituting that cosmic system which they referred to as "the Order of Nature"—that Fixed Order or Structure whose rational beauty, providential utility and reassuring stability provoked pious exclamations from contemporary natural theologians.

IV

The first irremediable crack in that Order came in zoology. True: peripheral elements, such as the supposedly eternal stability of the planetary system, had been in doubt from 1715 on, but it took Darwin to undermine either of the two most fundamental elements. And I want, at this point, to say a little about the deeper *philosophical* significance of Darwin's ideas.

The theological interpretations of classical science (I said) were not in all cases mere pious frills or exclamations. Often enough they were introduced to deal with genuine methodological problems in the explanatory programmes of the New Philosophy. Conversely, the theological hostility to Darwin—like (e.g.) the Soviet hostility to Mendel-Morganism—recognized and underlined a genuine and basic intellectual novelty in Darwin's explanatory methodology. For, in fact, Darwin insisted on pushing the horizon of explanation beyond the limits previously permitted by the Parmenidean axiom, in its Aristotelian interpretation. By producing a naturalistic theory of the "origin" of species, Darwin was in fact bringing within the sphere of natural philosophy a question that had hitherto been kept firmly outside it.

Darwin himself knew clearly what he was doing. In his private notebooks (M and N written around 1839–41) he confessed to being "a materialist"; and, like Descartes before him, he recognized the necessity of concealing the radical nature of his opinions. Yet those who have studied the matter clearly (such as Ernst Mayr) are clear that Darwin had a sure general grasp of the "populational" character of organic species, and understood the novelties it involved. So far as organic species went, the Parmenidean question did not arise. Species do not retain a permanent underlying reality through the *appearances* of change: so understood, species are not "permanent entities" at all. Rather, organic populations owe their specific status to a balance between variation and selective perpetuation, which maintains within the populations concerned a sufficient degree of unity and continuity, despite the occurrence of *genuine* change. So the primary question for Darwin's theory was not the question, how one species turned into another: it was, how it comes that there exist in natural population recognizable organic species *at all*. Thus,

Darwin's theory was, indeed, what he himself called it: a theory of *origins*, rather than a theory of *evolution*.

The transition from Aristotle to Darwin accordingly has a metaphysical as well as a zoological aspect. For Aristotle, the metaphysical permanence of entities-in-general was all of a piece with the historical fixity of organic species in particular: the doctrine of fixed organic species simply exemplified, in the special sphere of biology, the permanent character of all "rationally intelligible" entities. Conversely, Darwin's work in zoology demonstrated that Aristotle's most plausible and favored examples did not really support the metaphysical assumption on which all of Greek natural philosophy had been based. Species were not, after all, *permanent* entities. While they maintained a quite genuine unity and continuity through change—which made it legitimate to classify them into temporary species, recognizable by objective constellations of characters—the earlier "typological" approach to taxonomy inherited from Aristotle misrepresented the actual nature and long-term history of living things. In the zoological sphere at least, Darwin insisted, there were *no* "permanent entities"; and in this respect twentieth-century populational studies, as inaugurated by Fisher, Haldane and Sewall Wright, have merely reinforced the intellectual foundations of Darwin's original insights.

Meanwhile, the pupils of Cuvier attempted to make the idea of interspecific descent look ridiculous, by appealing to the traditional notion of "species." Flourens, for instance, argued in his *Ontologie Universelle* that the very description of such an evolutionary change was internally self-contradictory—as it indeed was, on the older typological definition of "species." More significantly, those scientists who saw the necessity of writing off organic species from the list of permanent entities clung the more insistently to the other invariant elements in the 18th century Order of Nature, viz. the Newtonian "Particles." James Clerk Maxwell became untypically eloquent in addressing the British Association in 1873:

No theory of evolution can be formed to account for the similarity of molecules, for evolution necessarily implies continuous change, and the molecule is incapable of growth or decay, of generation or destruction. None of the processes of nature, since the time when Nature began, have produced the

slightest difference in the properties of any molecule. We are therefore unable to ascribe either the existence of the molecules or the identity of their properties to the operation of any of the causes which we call natural. . . .

They continue this day as they were created—perfect in number and measure and weight, and from the ineffaceable characters impressed on them we may learn that those aspirations after accuracy in measurement, truth in statement, and justice in action, which we reckon among our noblest attributes as men, are ours because they are essential constituents of the image of Him who in the beginning created, not only the heaven and the earth, but the materials of which heaven and earth consist.

And those of us who remember the intellectual excitement that surrounded atomic physics in the early 1930s—I speak as one born in 1922—can recall the touch of magic and wizardry that surrounded the whole idea of Atomic Transmutation from the discovery of radioactivity on. This was not just one more of natural phenomena *comme les autres*. This was a kind of intellectual sacrilege: a breaking of the tablet on which (as Maxwell put it) the Almighty had written His "ineffaceable characters" at the Original Creation.

V

This quotation from Maxwell gives me a pivot on which to turn to my third and final thesis. For, after all, how much has changed, even as compared with the early 1930's: still more, as compared with 1873! How antique Maxwell's assertions in some ways sound to us today! The *forms* of our theoretical schemata (laws of motion, eigen-equations or whatever) may have remained fixed and invariant. We may, that is, have raised our theoretical analysis in physics to a level of a generality at which its application is, in principle at least, "universal." Yet, by now, we no longer need to assume, in physics or chemistry any more than in zoology, the unchanging existence in empirical Nature of those underlying realities which provided the ultimate level of explanatory analysis even for Maxwell. To compress the point into a nutshell, "even protons and neutrons do not have infinite half-lives." Whatever form our fundamental scientific explanations may take in the late 20th Century—how-

ever far we may still employ "universal" theoretical concepts and categories—they are no longer of the traditional Parmenidean type. We can no longer regard our explanations as "ultimate" and "rational," only if they relate the appearances of change back to "invariant and permanent elements."

What then should our own position be now? If the "permanent entities" of Greek metaphysics are not exemplified by organic species, what else in the real world does so? To this question, there is only one honest answer. Two hundred years of historical research have had their effect: whether we turn to social or intellectual history, evolutionary zoology, historical geology or astronomy, whether we consider explanatory theories of star-clusters, societies or cultures, languages or disciplines, organic species or the Earth itself, we must return the same Heraclitean verdict. *Nothing in the empirical world*, as we have now come to understand it, *possesses the kind of permanent unchanging identity* which all the Greek natural philosophers (the Epicureans apart) presupposed as a necessary character of the ultimate elements of Nature. As a result, if we are to entertain today any "metaphysical" thoughts about the nature of things-in-general, of a kind that will be consistent with the rest of our late twentieth-century ideas, we must take with full seriousness the failure of the older, Parmenidean approaches, not only in particle physics and zoological taxonomy but in all other spheres of thought also.

Let me sum up and bring my argument to a focus.

If we carry the historical transformation in our natural philosophy through to the end, it allows us to cut off the traditional debate in metaphysics at its starting-point, by discrediting the initial problem around which the tradition has developed—that is to say, the opening gambit from which the whole subsequent philosophical chess-game started in the first place. If this analysis is correct, the Parmenidean problem of the One and the Many—viz., "How do the permanent, eternal constituents of the world preserve their underlying identity, through all the surface appearances of change?"—simply does not arise *in that form*. Taking the question as asked, the only straightforward and candid reply we can now give is, "There are no such permanent, eternal objects."

The Greek metaphysicians' belief in Eternal Objects had two sources in their scientific view of the world, both of which have since been undercut. In its Platonist form, the belief was nourished by the success

of mathematical physics (or natural philosophy) in building *empirically-relevant* theories around "timeless" concepts and "universal" principles. As expounded by Descartes, Newton and even Maxwell, the theories of physics apparently relied for their validity on the assumption that the World of Nature consisted of eternal, changeless constituents—figures or particles, corpuscles or molecules—whose interactions gave rise to all physical phenomena. By now, we understand the sources of this "timelessness" or "tenselessness" better. Physical concepts and principles taken in isolation (as Hertz and Wittgenstein demonstrated) tell us nothing directly about the empirical world. Their empirical relevance, or applicability, depends on the auxiliary associations we make between abstract, theoretical categories and actual physical systems; and no actual material system whatever any longer exemplifies such an abstract category eternally or permanently. We must accordingly take care to distinguish the timelessness, tenselessness, of theoretical concepts and principles from the eternity (or unbounded antiquity) formerly attributed to the ultimate material elements or constituents of the world. We shall then see that to accept timeless concepts and tenseless principles into our *theoretical* physics in no way commits us to assuming the existence of permanent, unchanging objects in the *empirical* World of Nature.

In its other, Aristotelian form, the belief in Eternal Objects was linked just as closely to an interpretation of science—in this case, of zoology—which is by now discredited. In this form, the metaphysical doctrine of "permanent essences" drew support and credence not least from the success of Aristotle's zoological theory of "fixed species," which represented its most convincing application to our actual experience of the world. So, however irrelevant the empirical details of Darwin's work may be to philosophy generally, the form of his explanatory schema has a much broader philosophical significance. For, when Darwin and his successors showed that Aristotle's "essentialist" taxonomy was incompatible with organic evolution, and that the whole *zoological* concept of "species" must be re-analyzed in populational terms, their demonstration also knocked away the other scientific prop from under the traditional *metaphysical* debate.

To conclude: let me just hint at how this historical transformation of *natural philosophy* can open up new options for us in other areas of philosophy also.

For the future we cannot afford any longer to take metaphysical problems and questions seriously in their original forms. Instead, we must be prepared to reconsider them in the light of the changes that have transformed other fields of thought since Parmenides' day, and conclude, if need be, that many of the traditional questions in metaphysics—as in epistemology—are no longer relevant or operative.

If, after all, the truth about the basic entities of the world is more nearly Heraclitean than Parmenidean, this puts us in a position (e.g.) to tackle the problems of epistemology from a new standpoint: to integrate it, in fact, with *empirical* science—a proposal in which I find myself in unexpected agreement with Quine and Feyerabend. For the classical problems of epistemology themselves, like those of classical physics and zoology, go back to the 17th century. They were formulated at a time when man's epistemic situation was seen as involving a Fixed Mind confronting a Fixed Order of Nature, and so presumably operating according to Fixed and Universal Principles of Understanding. And now that we have *no* assurance of a Fixed Order of Nature, nor any guarantee of a Fixed cognitive structure either, the assumption of Fixed Universal and Necessary Principles of Pure Reason, Understanding or Human Knowledge loses its hold on us.

Where such speculations as those can lead us, is another and much larger question. But, if I am ready to pose it now with such confidence, that too is one more tribute to Shmuel Sambursky's ability to show us how the practice of scientific knowers remains, today as ever, crucially relevant to the philosophical theory of knowledge.

DISCUSSION

On paper by S. TOULMIN

BECHLER: I wish first to express my general agreement with the general picture which you drew, but only as a preface to pointing out a certain distortion which I believe you built into your picture in some essential way. I mean the following: Plato's concept of natural science as a system of ideas applied to nature is at its basis an irrational picture. That is to say Plato does not believe in the possibility of a completely rational science of Nature. He allows the possibility of a completely rational science, that is to say mathematics, and in a higher degree dialectic, but not when it comes to be applied to Nature. This appears in more than one place that I can recall. First we have the passages in the *Republic* where he points out that astronomical phenomena are irreducible in principle to any law or theory and therefore they can be used only as a starting point from which to study astronomy. Astronomy proper cannot be in the last resort a science of the observed celestial bodies since, he says, no one would seriously accept the view that the stars go around in certain exact trajectories or periods. They have an essential irrationality in them which prevents them from being reduced to exact rational science.

The second place is in that part of the *Timaeus* where the work of the Demiurge is characterized as against "What comes of necessity." The work of the Demiurge is characterized by a compromise which is reached between him and the material world. As Plato puts it—the Demiurge tries or makes an effort to convince necessity by his reason. The compromise is only partial and there exists in the last resort always an irrational element since the Demiurge as such is not an omnipotent entity.

In contrast with this, the Newtonian god is omnipotent—as well as the Cartesian god—and all Nature is governed as a result by exact laws, which it remains only for the human intellect to unearth, and this is the basis of Newton's dogmatic belief in the possibility of an exact

mathematical science of Nature. So to present Newton as a Platonist, as a man who at last put some flesh on the skeleton of Plato's programme, is extremely misleading.

On the other hand, I believe we are living in a Platonic era again, which started with Darwin's evolutionary theory. This was the first theory, I believe, which incorporated chance, through his concept of modifications—which are essentially chance concepts—into the conceptual framework. Today we have reached the apex of this trend in quantum mechanics, which I don't need to go into much further.

TOULMIN: The point I was seeking to make—the resemblance between Newton and Plato which led me to say that Newton was putting flesh on the skeleton of Plato's methodology—was one which I think can be presented in a way which your criticism doesn't seriously touch. I think the point you make about Plato's limited expectations for this programme is a perfectly genuine point, and Newton clearly saw the possibility of this programme being carried through further. What I was claiming was that, for Plato, in so far as true understanding of Nature is possible, it has to be by way of constructing formal mathematical analyses of the relations in terms of which the phenomena of nature are to be understood. And I don't really think that what you have been saying touches the point. There is an argument about just how far true understanding of Nature is possible, and about this you may be right: Newton was more courageous than Plato in the expectations he formed about how far this programme of mathematizing our understanding of Nature could be carried. In particular, in planetary astronomy—and, for heaven's sake, Newton had reason to see this in astronomy—there was reason for him to think that the scope of mathematical explanation could be carried much further than Plato had reason in his time to imagine. To insist on an identity between Newton and Plato at this point would be gravely misleading, but I think there is enough in common between us for us to reach a compromise on that particular point.

On the other hand I am very unhappy about your second point. As I understand it, the passages in the *Timaeus* in which Plato talks about the limited extent to which the Demiurge can impose his Will, as it were, on Necessity, on what you call the irrational element in the world, cannot properly be thought of as introducing any kind of a statistical, random, or even acausal element into the world. What he is saying, in

my view, is something much more like what we might ourselves say, e.g., "If a bird were the size of an elephant, its breast-bone would be so big that it couldn't fly." There are indeed limits, in the nature of things, within which the forms whose rational intelligibility one can conceive in principle are capable of being realized in practice. Far from this corresponding to a random element, it corresponds rather to those aspects of nature about which Newton had nothing left to say except "God did it that way."

If we are to speak in a 20th century way about what Plato means by "necessity" in the *Timaeus*, and the respects in which the Demiurge is not able to fix the thing up entirely in a rational way, it is better to construe this word "necessity" in terms of some such phrase as "arbitrary parameters." What Plato is saying here is that how the world ends up depends, partly on the rational relationships embodied in the forms and the mathematical systems through which properties of these forms are analyzed, partly on certain arbitrary parameters; and certain forms can be exemplified in the empirical world in one way rather than another, only insofar as the values of the arbitrary parameters in fact permit.

BECHLER: I feel that by your interpretation of Plato you just cannot connect the two passages, the *Republic* and the *Timaeus* passage, into one whole view, whereas my interpretation takes it as two aspects of the same view.

TOULMIN: To be quite candid, what I am really doing is interpreting Newton rather than Plato. I did not explore, partly because I had set myself too large an agenda, the question just how completely the various Greek natural philosophers thought that their various methodological programmes could be carried through. And of course what you say is perfectly correct. Their hopes were in some cases a good deal less ambitious than ours have been.

ROSENFELD: Their means too!

TOULMIN: Their means too. Perhaps it would have been irrational to nourish more hopes in Plato's time than those he in fact nourished, which, by the standards of what was available, were ambitious enough in all conscience.

MENDELSONH: If I can take a point that I think you were getting at—when you turned to the problem of Darwin, I appreciated the general

thrust of where you were going with Darwin introducing a means of dealing with change. But as I look at Darwin I wonder if you could not almost have turned it on its head, and said Darwin was not dealing with the origin of species so much as he was dealing with a survival of the species. That Darwin really never was able to get to the problem of origin and was more really able to deal with how some things survived in Nature. You can see why I am separating these two, because the one deals with the creation of novelty which eluded Darwin—the other deals with novelty perhaps already being there. If you look at natural selection—natural selection is a conservative process and not a creative one.

TOULMIN: I am very happy to take this point up because there is a distinction that one has at once to make here. I deliberately set my statement about Darwin giving us a theory of the *origin* of species alongside my statements about the role of natural theology in relation to theoretical physics. I did this because I wanted to bring out the fact that questions about Origins, with a capital O, have always been questions of a quasi. . . .

MENDELSON: Of a theological basis . . .

TOULMIN: No, no, I then also wanted to say that the theology was a disguise: that the questions about origins have always been, in effect, philosophical—*wie ist ein so-und-so überhaupt möglich*—questions about how things of the requisite kind are possible at all. How is it, for example, that there are “hard, massy, impenetrable, moveable Particles” which never wear or break in pieces—“God made it that way,” this is how it comes to be that there are these ultimate elements at all. And the point I was trying to make when I said that Darwin’s theory was a theory of the origin of species, was that the first task which this dynamic equilibrium of variation and selection has to perform is the task of explaining *wie ist eine organische Spezies überhaupt möglich*.

How is it that there can be such things at all? This indeed is the point you are making: that the balance between variation and natural selection is, in the first place, a conservative thing. It is the conservative power of this dynamic equilibrium mechanism of Darwin’s that accounts for the fact that species—though temporary—do indeed have specificity.

ELKANA: Would you agree first with the formulation that the trend in the last 70 or let’s say the last 20 years has been to replace those funda-

mental underlying, unchangeable entities—whatever they are—much more with something which we could call elementary processes, by trying to describe things more and more in terms of units of change? If you don’t agree, then it is a different story. If you do, would it not be still much more of a kind of Parmenidean programme than part of a Heraclitean programme and can you imagine any kind of fruitful programme on an “everything changes” basis?

TOULMIN: It is true that I might have spent more time distinguishing between the different *kinds* of ways in which this idea of *stoicheia kai archai*, or “elements and principles,” was used by the Greek natural philosophers. Certainly, one could argue that these elements were often appealed to in Greek antiquity as involving principles or propositions, rather than concepts and objects. And my argument here was directed, primarily, against the view that scientific explanation must necessarily assume the existence of some population or some set of eternally-existing objects or concepts. Again, if all that one insists on is some universally-applicable set of laws of nature (as Shmuel Sambursky argued to me in conversation), then so long as we operate with the same universally applicable Schrödinger or post-Schrödinger equations—so long as we have a single mathematical formalism which we apply equally to events here and now and to those millions of light-years away—to this extent (as Sambursky wanted to say) there is still hope for the Parmenidean programme.

That is o.k., with one qualification. One might well argue that in a world which was totally Heraclitean—if one could conceive what it would be like to have nothing at all to appeal to in our explanations of natural phenomena—science would be pretty poverty-stricken. Nevertheless, the fact is that both Newton’s way of looking at the world and Ray’s way of looking at it involved appeals to “species” or “particles” as ultimate explanatory factors, which were *concepts or objects* rather than *laws or propositions*; and all such “eternal objects” as these have fallen by the wayside in subsequent years.

Now, you are saying, “Don’t we now have in mind some sort of constant unit-processes of change?” Perhaps one can speak this way; but these units are still characterized in an abstract way, as particular exemplifications of Sambursky’s universal laws or formalisms—and it would be hard to quarrel with that. But what is still a novelty about our

post-Kantian situation is the fact that we can develop and handle mathematical theories of Nature in a way which no longer involves the particular kinds of existence-assumptions that were previously universal—at any rate, from the time of Descartes up to the time of Maxwell.

Possibly, I should have made a much sharper distinction between Plato on the one hand and Descartes on the other. For Descartes, the mathematical structure of Plato's natural philosophy had a direct existential import—an existential necessity—that it did not have for Plato. Plato was quite prepared to say: we have the "Forms" which we understand, but whether any actual material objects exemplify them we just don't know.

ROSENFIELD: With the reservations that we have heard, I think you were right in a general way to stress the prevailing attitude of a search, if I may say so, for stability in the early development of science, which was then replaced in the 19th century by more dynamic views. You mentioned Darwin; of course in physics there are examples also of a more dynamic attitude. But my impression is that you could have gone one step further taking the kind of situations in which, as I see it, we have reached a synthesis resolving this opposition of points of view. Quantum mechanics, which is after all the basic theory at the moment—I am thinking of the principle of the existence of the quantum of action and its consequences—embodies both aspects. We have on the one hand the motion, the description in space and time of an evolution of the systems, and on the other hand we have the conservation laws, and those two aspects are intimately connected in the formalism. You cannot separate them: the motion, if I may be technical for a moment, is governed by a group of transformations, and every time you have a group the operators defining the elementary transformations are invariants for the group. So as soon as you use this group concept—and it is a most powerful concept that we are using all the time—you have necessarily the two aspects embodied in that representation; and this corresponds of course to reality, since this description is a good one—it fits all the phenomena.

Now in this context we have physicists who insist on the dynamic aspect: you will hear in discussions among physicists that what we lack in the theory of elementary particles is dynamics. You will also hear other physicists, such as Heisenberg, defending the opposite thesis, that we are coming back to the *Timaeus*. I don't know if you have seen a recent

booklet by Heisenberg: it is a lecture that he chose, in a rather spectacular way, to deliver on the top of the Pnyx in Athens. To argue his strange case he insisted on the invariants that are contained in any theory that one can try to make. So, if you only look on one side of the question, you can say that we are back to Parmenides or you can say that we are fully Heraclitean. I think that we are one better—we are now in a position to embody both aspects in a coherent, unique description.

TOULMIN: That is clearly another legitimate way of using these historical parallels between the science of the Greeks and the modern era.

Indeed, as John Murdoch was saying earlier today, we must contrast the sorts of sharp opposition one finds between rival scientific theories, and the sharp changes by which scientific theses succeed one another, with the very different manner in which the balance of opinion shifts from one philosophical position to another. In looking at the relationship between Greek natural philosophy and modern science, we can see how men began by conceiving of their rival methodological recipes for producing a "rational science of Nature" in a way which pitted them too absolutely against one another. They saw it as their business to tell us how to do science in general, and it seemed to them for the time being that there must be one and only one way of doing science. Whereas, now, we can distinguish the different kinds of inquiry, and fields of study, to which each of these different recipes is clearly appropriate; so that, now, we can say that this area of scientific inquiry or that shows what there could be gained by pursuing Aristotle's methodological suggestions about "life-cycles." Or that such-and-such another area of science shows us what there could be gained by pursuing Empedocles' arguments about "mixture and separation" of different states of matter . . . and so on.

To begin with, these philosophical recipes for producing possible rational sciences of nature were direct rivals. But now we see that they were, rather, complementary ways of doing things, all of which have a place within the overall scientific description of nature. It is a question of seeing where each method is most appropriately at home, rather than of coming down finally on one side rather than another. So, if you can provide a good compromise between Parmenides and Heraclitus, it would be unlike me to stand in your way.

AGASSI: I wanted to comment on the attempt to find a compromise be-

tween the Parmenidean and the Heraclitean. It reminds me of the sophism or witticism concerning the amoeba—is it a monocellular or an acellular body? Does it have no cells or is it one cell? It is really very hard to decide. Of course if you take the cell theory as Hooke presented it, to say the cell is the atom of living matter, then of course the amoeba being living matter would be a cell. But you might also specify a bit more and you say what would be something that would generally qualify as acellular. For instance, consider the speculation about primordial protein in its amorphous state hanging about in marshes, that would be living but would not be a cell. You would then say the amoeba is a cell because it has cellular structure, it contains a nucleus, cytoplasm, walls. And then you can look at algae and you say they are acellular because although they have nuclei they have no walls.

And so, what initially looks like a sophism, transforms through a discussion and a clarification of certain problems, and becomes determinate, and you decide that the amoeba is not acellular but monocellular.

Now the real question is—what problems do Parmenides and Heraclitus come to solve? It seems to me, following my reading of Aristotle's *Metaphysics*—that the problem is of individuation: Thales asks, in what sense is any Tom, Dick, or Harry—any Socrates, as Aristotle put it, of today, the same as the Socrates of yesterday, and in what sense not? And then he says there is an unchanging little man in the changing man, who is the individual, the substratum. Now comes Parmenides and says that if there is a substratum, then what is changing is only illusion because there is no relation between the unchanging substratum and the illusion of change. And so the problem of individuation is solved by Parmenides in his recognition of one and only one individual—the whole world is one. Whereas Heraclitus says not appearances are illusion but the little man inside the man is an illusion—there is no individuality. So again we have the whole world an amoeba and Parmenides says the amoeba is monocellular and Heraclitus says the amoeba is acellular. And both really do not solve the problem of individuation which is interesting to us and which is a very nice question; both evade Thales' original question: why do we identify Rip van Winkle with young Rip van Winkle even though young Rip van Winkle is much more similar to Rip's son?

One has to find out in which senses and degrees and so on, and this problem of individuation has been left in abeyance. It has been claimed by some recent modern thinkers—Schrödinger and Borges—that the only sincere modern attempt to see the degrees of individuality is that which follows the footsteps of Schopenhauer. On that rock bottom, he says, there is only the Parmenidean solution, and on the very superficial level—the Heraclitean level—there would be no individuality at all. But if this be so, one has to really square this with process philosophy which sees the structure of the universe as expressed in abstract natural laws and I don't think anybody did it. Moreover, this leaves open the problems of moral philosophy which lays responsibility at the door of the individual. So I don't really see whether we can decide between Parmenides and Heraclitus or whether we can compromise between them because the problem of individuation is really a neglected problem—and that is the one that it seems they tried to solve.

TOULMIN: Do you really think that this was the problem they were trying to solve?

AGASSI: If Aristotle says that is the central one—I believe it. He may be mistaken.

TOULMIN: That is perhaps *one* of the things that Aristotle says.

AGASSI: He says this is the central problem—at least so he presents it in his philosophy.

TOULMIN: Well, we will have to get together around the text. What I indeed said was that all these metaphysical issues have methodological implications; and, insofar as this is the case, one can then see these methodological implications having certain fruits. I agree with you that the problem of individuation may be another of the problems which is at issue here. And there is a third group of problems, also, which we haven't talked about tonight, having to do with the intelligibility of language: these arguments lead one into the Theory of Ideas by yet another route. But I have concentrated here on the ones on which Shmuel Sambursky's individual searchlight has most effectively been cast.

HISTORY OF SCIENCE AND ITS RATIONAL
RECONSTRUCTIONS

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Earlier versions of this paper were read and criticized by Colin Howson, Alan Musgrave, John Watkins, Élie Zahar, and especially John Worrall.

This paper further develops some of the theses proposed in my [1970]. I have tried, at the cost of some repetition, to make it self-contained. It was originally read at the Biennial Meeting of the Philosophy of Science Association in October 1970 and was first published in R.C. Buck and R. S. Cohen, eds., *PSA 1970, Boston Studies in the Philosophy of Science*, vol. 8, Reidel Publishing House, 1971, together with comments by Thomas S. Kuhn, Herbert Feigl, Richard Hall and Noretta Koertge and my *Replies to Critics*.

INTRODUCTION

"Philosophy of science without history of science is empty; history of science without philosophy of science is blind." Taking its cue from this paraphrase of Kant's famous dictum, this paper intends to explain how the historiography of science should learn from the philosophy of science and *vice versa*. It will be argued that (a) philosophy of science provides normative methodologies in terms of which the historian reconstructs "internal history" and thereby provides a rational explanation of the growth of objective knowledge; (b) two competing methodologies can be evaluated with the help of (normatively interpreted) history; (c) any rational reconstruction of history needs to be supplemented by an empirical (socio-psychological) "external history".

The vital demarcation between normative-internal and empirical-external is different for each methodology. Jointly, internal and external historiographical theories determine to a very large extent the choice of problems for the historian. But some of external history's most crucial problems can be formulated only in terms of one's methodology; thus internal history, so defined, is primary, and external history only secondary. Indeed, in view of the autonomy of internal (but not of external) history, external history is irrelevant for the understanding of science.¹

§1. RIVAL METHODOLOGIES OF SCIENCE: RATIONAL RECONSTRUCTIONS AS GUIDES TO HISTORY

There are several methodologies afloat in contemporary philosophy of science; but they are all very different from what used to be understood by "methodology" in the seventeenth or even eighteenth century. Then it was hoped that methodology would provide scientists with a mechanical book of rules for solving problems. This hope has now been given up: modern methodologies or "logics of discovery" consist merely of a set of (possibly not even tightly knit, let alone mechanical) rules

¹"Internal history" is usually defined as intellectual history; "external history" as social history (cf. e.g. Kuhn [1968]). My unorthodox, new demarcation between "internal" and "external" history constitutes a considerable problemshift and may sound dogmatic. But my definitions form the hard core of a historiographical research programme; their evaluation is part and parcel of the evaluation of the fertility of the whole programme.

for the *appraisal* of ready, articulated theories.² Often these rules, or systems of appraisal, also serve as "theories of scientific rationality", "demarcation criteria" or "definitions of science".³ Outside the legislative domain of these normative rules there is, of course, an empirical psychology and sociology of discovery.

I shall now sketch four different "logics of discovery". Each will be characterized by rules governing the (scientific) *acceptance* and *rejection* of theories or research programmes.⁴ These rules have a double function. First, they function as a *code of scientific honesty* whose violation is intolerable; secondly, as hard cores of (*normative*) *historiographical research programmes*. It is their second function on which I should like to concentrate.

A. *Inductivism*

One of the most influential methodologies of science has been inductivism. According to inductivism only those propositions can be accepted into the body of science which either describe hard facts or are infallible inductive generalizations from them.⁵ When the inductivist *accepts* a scientific proposition, he accepts it as provenly true; he *rejects* it if it is not. His scientific rigour is strict: a proposition must be either proven from facts, or—deductively or inductively—derived from other propositions already proven.

Each methodology has its specific epistemological and logical problems. For example, inductivism has to establish with certainty the truth of "factual" ("basic") propositions and the validity of inductive inferences. Some philosophers get so preoccupied with their epistemological and logical problems that they never get to the point of becoming interested in actual history; if actual history does not fit their standards

²This is an all-important shift in the problem of normative philosophy of science. The term "normative" no longer means rules for arriving at solutions, but merely directions for the appraisal of solutions already there. Thus *methodology* is separated from *heuristics*, rather as value judgments are from "ought" statements. (I owe this analogy to John Watkins.)

³This profusion of synonyms has proved to be rather confusing.

⁴The epistemological significance of scientific "acceptance" and "rejection" is, as we shall see, far from being the same in the four methodologies to be discussed.

⁵"Neo-inductivism" demands only (provably) highly probable generalizations. In what follows I shall only discuss classical inductivism; but the watered down neo-inductivist variant can be similarly dealt with.

they may even have the temerity to propose that we start the whole business of science anew. Some others take some crude solution of these logical and epistemological problems for granted and devote themselves to a rational reconstruction of history without being aware of the logico-epistemological weakness (or, even, untenability) of their methodology.⁶

Inductivist criticism is primarily sceptical: it consists in showing that a proposition is unproven, that is, pseudoscientific, rather than in showing that it is false.⁷ When the inductivist historian writes the *prehistory* of a scientific discipline, he may draw heavily upon such criticisms. And he often explains the early dark age—when people were engrossed by “unproven ideas”—with the help of some “external” explanation, like the socio-psychological theory of the retarding influence of the Catholic Church.

The inductivist historian recognizes only two sorts of *genuine scientific discoveries*: *hard factual propositions* and *inductive generalizations*. These and only these constitute the backbone of his *internal history*. When writing history, he looks out for them—finding them is quite a problem. Only when he finds them, can he start the construction of his beautiful pyramids. Revolutions consist in unmasking [irrational] errors which then are exiled from the history of science into the history of pseudoscience, into the history of mere beliefs: genuine scientific progress starts with the latest scientific revolution in any given field.

Each internal historiography has its characteristic victorious paradigms.⁸ The main paradigms of inductivist historiography were Kepler's generalizations from Tycho Brahe's careful observations; Newton's discovery of his law of gravitation by, in turn, inductively generalizing Kepler's “phenomena” of planetary motion; and Ampère's discovery of his law of electrodynamics by inductively generalizing his observations of electric currents. Modern chemistry too is taken by some inductivists as having really started with Lavoisier's experiments and his “true explanations” of them.

But the inductivist historian cannot offer a *rational* “internal” explanation for *why* certain facts rather than others were selected in the first

⁶Cf. below, p. 217.

⁷For a detailed discussion of inductivist (and, in general, justificationist) criticism cf. my [1966] and especially my [1974].

⁸I am now using the term “paradigm” in its pre-Kuhnian sense.

instance. For him this is a *non-rational, empirical, external* problem. Inductivism as an “internal” theory of rationality is compatible with many different supplementary empirical or external theories of problem-choice. It is, for instance, compatible with the vulgar-Marxist view that problem-choice is determined by social needs;⁹ indeed, some vulgar-Marxists identify major phases in history of science with the major phases of economic development.¹⁰ But the choice of facts need not be determined by social factors; it may be determined by extra-scientific intellectual influences. And inductivism is equally compatible with the “external” theory that the choice of problems is primarily determined by in-born, or by arbitrarily chosen (or traditional) theoretical (or “metaphysical”) frameworks.

There is a radical brand of inductivism which condemns all external influences, whether intellectual, psychological or sociological, as creating impermissible bias: radical inductivists allow only a [random] selection by the empty mind. Radical inductivism is, in turn, a special kind of *radical internalism*. According to the latter, once one establishes the existence of some external influence on the acceptance of a scientific theory (or factual proposition), one must withdraw one's acceptance: proof of external influence means invalidation.¹¹ But since external influences always exist, radical internalism is utopian, and, as a theory of rationality, self-destructive.¹²

When the radical inductivist historian faces the problem of why some great scientists thought highly of metaphysics and, indeed, why they thought that their discoveries were great for reasons which, in the light of inductivism, look very odd, he will refer these problems of “false consciousness” to psychopathology that is, to external history.

B. Conventionalism

Conventionalism allows for the building of any system of pigeon

⁹This compatibility was pointed out by Agassi on pp. 23–27 of his [1963]. But he did not point out the analogous compatibility within his own falsificationist historiography; cf. below, pp. 204–205.)

¹⁰Cp. e.g. Bernal [1965], p. 377.

¹¹Some logical positivists belonged to this set: one recalls Hempel's horror at Popper's casual praise of certain external metaphysical influences upon science (Hempel [1937]).

¹²When German obscurantists scoff at “positivism”, they frequently mean radical internalism, and in particular, radical inductivism.

holes which organizes facts into some coherent whole. The conventionalist decides to keep the centre of such a pigeonhole system intact as long as possible: when difficulties arise through an invasion of anomalies, he only changes and complicates the peripheral arrangements. But the conventionalist does not regard any pigeonhole system as provenly true, but only as "true by convention" (or possibly even as neither true nor false). In *revolutionary* brands of conventionalism one does not have to adhere forever to a given pigeonhole system: one may abandon it if it becomes unbearably clumsy and if a simpler one is offered to replace it.¹³ This version of conventionalism is epistemologically, and especially logically, much simpler than inductivism: it is in no need of valid inductive inferences. Genuine *progress* of science is cumulative and takes place on the ground level of "proven" facts;¹⁴ the *changes* on the theoretical level are merely instrumental. Theoretical "progress" is only in convenience ("simplicity"), and not in truth-content.¹⁵ One may, of course, introduce revolutionary conventionalism also at the level of "factual" propositions, in which case one would accept "factual" propositions by decision rather than by experimental "proofs". But then, if the conventionalist is to retain the idea that the growth of "factual" science has anything to do with objective, factual truth, he must devise some metaphysical principle, which he then has to superimpose on his rules for the game of science.¹⁶ If he does not, he cannot escape scepticism or, at least, some radical form of instrumentalism.

¹³For what I here call *revolutionary conventionalism*, see my [1970], pp. 105–6 and 187–9.

¹⁴I mainly discuss here only one version of revolutionary conventionalism, the one which Agassi, in his [1966], called "unsophisticated": the one which assumes that factual propositions—unlike pigeonhole systems—can be "proven". (Duhem, for instance, draws no clear distinction between facts and factual propositions.)

¹⁵It is important to note that most conventionalists are reluctant to give up inductive generalizations. They distinguish between the "*floor of facts*", the "*floor of laws*" (i.e. inductive generalizations from "facts") and the "*floor of theories*" (or of pigeonhole systems) which classify, conveniently, both facts and inductive laws. (Whewell, the conservative conventionalist and Duhem, the revolutionary conventionalist differ less than most people imagine.)

¹⁶One may call such metaphysical principles "inductive principles". For an "inductive principle" which—roughly speaking—makes Popper's "degree of corroboration" (a conventionalist appraisal) the measure of Popper's verisimilitude (truth-content minus falsity-content) see my [1968a], pp. 390–408 and my [1971a], § 2. (Another widely spread "inductive principle" may be formulated like this: "What the group of trained—or up-to-date, or suitably purged—scientists decide to accept as 'true', is true".)

(It is important to clarify the *relation between conventionalism and instrumentalism*. Conventionalism rests on the recognition that false assumptions may have true consequences; therefore false theories may have great predictive power. Conventionalists had to face the problem of comparing rival false theories. Most of them conflated truth with its signs and found themselves holding some version of the pragmatic theory of truth. It was Popper's theory of truth-content, verisimilitude and corroboration which finally laid down the basis of a philosophically flawless version of conventionalism. On the other hand some conventionalists did not have sufficient logical education to realize that some propositions may be true whilst being unproven, and others false whilst having true consequences, and also some which are both false and approximately true. These people opted for "instrumentalism": they came to regard theories as neither true nor false but merely as "instruments" for prediction. Conventionalism, as here defined, is a philosophically sound position; instrumentalism is a degenerate version of it, based on a mere philosophical muddle caused by lack of elementary logical competence.)

Revolutionary conventionalism was born as the Bergsonians's philosophy of science: free will and creativity were their slogans. The code of scientific honor of the conventionalist is less rigorous than that of the inductivist: it puts no ban on unproven speculation, and allows a pigeon-hole system to be built around *any* fancy idea. Moreover, conventionalism does not brand discarded systems as unscientific: the conventionalist sees much more of the actual history of science as rational ("internal") than does the inductivist.

For the conventionalist historian, major discoveries are primarily inventions of new and simpler pigeonhole systems. Therefore he constantly compares for simplicity: the complications of pigeonhole systems and their revolutionary replacement by simpler ones constitute the backbone of his internal history.

The paradigmatic case of a scientific revolution for the conventionalist has been the Copernican revolution.¹⁷ Efforts have been made to show

¹⁷Most historical accounts of the Copernican revolution are written from the conventionalist point of view. Few claimed that Copernicus's theory was an "inductive generalization" from some "factual discovery"; or that it was proposed as a bold theory to replace the Ptolemaic theory which had been "refuted" by some celebrated "crucial" experiment.

For a further discussion of the historiography of the Copernican revolution, cp. my [1973c].

that Lavoisier's and Einstein's revolutions too were replacements of clumsy theories by simple ones.

Conventionalist historiography cannot offer a *rational* explanation of why certain facts were selected in the first instance or why certain particular pigeonhole systems were tried rather than others at a stage when their relative merits were yet unclear. Thus conventionalism, like inductivism, is compatible with various supplementary empirical—"externalist" programmes.

Again, the conventionalist historian, like his inductivist colleague, frequently encounters the problem of "false consciousness". According to conventionalism, for example, it is a "matter of fact" that great scientists arrive at their theories by flights of their imaginations. Why then do they often claim that they derived their theories from facts? The conventionalist's rational reconstruction often differs from the great scientist's own reconstruction—the conventionalist historian relegates these problems of false consciousness to the externalist.¹⁸

C. Methodological Falsificationism

Contemporary falsificationism arose as a logico-epistemological criticism of inductivism and of Duhemian conventionalism. Inductivism was criticized on the grounds that its two basic assumptions, namely, that factual propositions can be "derived" from facts and that there can be valid inductive (content-increasing) inferences, are themselves unproven and even demonstrably false. Duhem was criticized on the grounds that comparison of intuitive simplicity can only be a matter for subjective taste and that it is so ambiguous that no hard-hitting criticism can be based on it. Popper, in his *Logik der Forschung*, proposed a new "falsificationist" methodology.¹⁹ This methodology is another brand of revolutionary conventionalism: the main difference is that it allows factual, spatio-temporally singular "basic statements" rather than spatio-tem-

¹⁸For example, for non-inductivist historians Newton's "*Hypotheses non fingo*" represents a major problem. Duhem, who unlike most historians did not over-indulge in Newton-worship, dismissed Newton's inductivist methodology as logical nonsense; but Koyré, whose many strong points did not include logic, devoted long chapters to the "hidden depths" of Newton's muddle.

¹⁹In this paper I use this term to stand exclusively for one version of falsificationism, namely for "naive methodological falsificationism", as defined in my [1970], pp. 93–116.

porally universal theories, to be accepted by convention. In the code of honour of the falsificationist a theory is scientific only if it can be *made* to conflict with a basic statement; and a theory must be eliminated if it conflicts with an accepted basic statement. Popper also indicated a further condition that a theory must satisfy in order to qualify as scientific: it must predict facts which are *novel*, that is, unexpected in the light of previous knowledge. Thus it is again Popper's code of scientific honour to propose unfalsifiable theories or "*ad-hoc*" hypotheses (which imply no *novel* empirical predictions)—just as it is against the [classical] inductivist code of scientific honour to propose unproven ones.

The great attraction of Popperian methodology lies in its clarity and force. Popper's deductive model of scientific criticism contains empirically falsifiable spatio-temporally universal propositions, initial conditions and their consequences. The weapon of criticism is the *modus tollens*: neither inductive logic nor intuitive simplicity complicate the picture.²⁰

(Falsificationism, though logically impeccable, has epistemological difficulties of its own. In its "dogmatic" proto-version it assumes the provability of propositions from facts and thus the disprovability of theories—a false assumption.²¹ In its Popperian "conventionalist" version it needs some (extra-methodological) "inductive principle" to lend epistemological weight to its decisions to accept "basic" statements, and in general, to connect its rules of the scientific game with verisimilitude.²²)

The Popperian historian looks for great, "bold", falsifiable theories and for great negative crucial experiments. These form the skeleton of his rational reconstruction. The Popperians's favourite paradigms of great falsifiable theories are Newton's and Maxwell's theories, the radiation formulas of Rayleigh, Jeans and Wien, and the Einsteinian revolution; their favourite paradigms for crucial experiments are the Michelson-Morley experiment, Eddington's eclipse experiment, and the experiments of Lummer and Pringsheim. It was Agassi who tried to turn this naive falsificationism into a systematic historiographical research programme.²³ In

²⁰Since in his methodology the concept of intuitive simplicity has no place, Popper was able to use the term "simplicity" for "degree of falsifiability". But there is more to simplicity than this: cf. my [1970], pp. 130–132.

²¹For a discussion cf. my [1970], especially pp. 99–100.

²²For further discussion cf. below, pp. 218–219.

²³Agassi [1963].

particular he predicted (or "postdicted", if you wish) that behind each great experimental discovery lies a theory which the discovery contradicted; the importance of a factual discovery is to be measured by the importance of the theory refuted by it. Agassi seems to accept at face value the value judgments of the scientific community concerning the importance of factual discoveries like Galvani's, Oersted's, Priestley's, Roentgen's and Hertz's; but he denies the "myth" that they were chance discoveries (as the first four were said to be) or confirming instances (as Hertz first thought his discovery was).²⁴ Thus Agassi arrives at a bold prediction: all these five experiments were successful refutations—in some cases even *planned* refutations—of theories which he proposes to unearth, and, indeed, in most cases, claims to have unearthed.²⁵

Popperian internal history, in turn, is readily supplemented by external theories of history. Thus Popper himself explained that [on the positive side] (1) the main *external* stimulus of scientific theories comes from unscientific "metaphysics", and even from myths (this was later beautifully illustrated mainly by Koymé); and that [on the negative side] (2) facts do *not* constitute such external stimulus—factual discoveries belong completely to internal history, emerging as refutations of some scientific theory, so that facts are only noticed if they conflict with some previous expectation. Both theses are cornerstones of Popper's *psychology* of discovery.²⁶ Feyerabend developed another interesting *psychological* thesis of Popper, namely, that proliferation of rival theories may—externally—speed up *internal* Popperian falsification.²⁷

²⁴An experimental discovery is a *chance discovery in the objective sense* if it is neither a confirming nor a refuting instance of some theory in the objective body of knowledge of the time; it is a *chance discovery in the subjective sense* if it is made (or recognized) by the discoverer neither as a confirming nor as a refuting instance of some theory he personally had entertained at the time.

²⁵Agassi [1963], pp. 64–74.

²⁶Within the Popperian circle, it was Agassi and Watkins who particularly emphasized the importance of unfalsifiable or barely testable "*empirical*" theories in providing *external* stimulus to later properly *scientific* developments. (Cf. Agassi [1964] and Watkins [1958].) This idea, of course, is already there in Popper's [1934] and [1960]. Cf. my [1970], p. 184; but the new formulation of the difference between their approach and mine which I am going to give in this paper, will, I hope, be much clearer.

²⁷Popper occasionally—and Feyerabend systematically—stressed the catalytic (*external*) role of alternative theories in devising so-called "crucial experiments". But alternatives are not merely catalysts, which can be later removed in the rational reconstruction, they are *necessary* parts of the falsifying process. Cf. Popper [1940] and Feyerabend [1965]; but cf. also Lakatos [1970], especially p. 121, footnote 4.

But the external supplementary theories of falsificationism need not be restricted to purely intellectual influences. It has to be emphasized (*pace* Agassi) that falsificationism is no less compatible with a vulgar-Marxist view of what makes science progress than is inductivism. The only difference is that while for the latter, Marxism might be invoked to explain the discovery of *facts*, for the former it might be invoked to explain the invention of *scientific theories*; while the choice of facts (that is, for the falsificationist, the choice of "potential falsifiers") is primarily determined internally by the theories.

"False awareness"—"false" from the point of view of *his* rationality theory—creates a problem for the falsificationist historian. For instance, why do some scientists believe that crucial experiments are positive and verifying rather than negative and falsifying? It was the falsificationist Popper who, in order to solve these problems, elaborated better than anybody else before him the cleavage between objective knowledge (in his "third world") and its distorted reflections in individual minds.²⁸ Thus he opened up the way for my demarcation between internal and external history.

D. Methodology of Scientific Research Programmes

According to my methodology the greatest scientific achievements are research programmes which can be evaluated in terms of progressive and degenerating problemshifts; and scientific revolutions consist of one research programme superseding (overtaking in progress) another.²⁹ This methodology offers a new rational reconstruction of science. It is best presented by contrasting it with falsificationism and conventionalism, from both of which it borrows essential elements.

From conventionalism, this methodology borrows the license rationally to accept by convention not only spatio-temporally singular "factual statements" but also spatio-temporally universal theories: indeed, this becomes the most important clue to the continuity of scientific growth.³⁰

²⁸Cf. Popper [1968a] and [1968b].

²⁹The terms "progressive" and "degenerating problemshifts", "research programmes", "superseding" will be crudely defined in what follows—for more elaborate definitions see my [1968b] and especially my [1970].

³⁰Popper does not permit this: "There is a vast difference between my views and conventionalism. I hold that what characterizes the empirical method is just this: our conventions determine the acceptance of the *singular*, not of the *universal* statements." (Popper [1934], section 30).

The basic unit of appraisal must be not an isolated theory or conjunction of theories but rather a "research programme", with a conventionally accepted (and thus by provisional decision "irrefutable") "hard core" and with a "positive heuristic" which defines problems, outlines the construction of a belt of auxiliary hypotheses, foresees anomalies and turns them victoriously into examples, all according to a preconceived plan. The scientist lists anomalies, but as long as his research programme sustains its momentum, he may freely put them aside. *It is primarily the positive heuristic of his programme, not the anomalies, which dictate the choice of his problems.*³¹ Only when the driving force of the positive heuristic weakens, may more attention be given to anomalies. The methodology of research programmes can explain in this way the *high degree of autonomy of theoretical science*; the naive falsificationist's disconnected chains of conjectures and refutations cannot. What for Popper, Watkins and Agassi is *external*, influential metaphysics, here turns into the *internal* "hard core" of a programme.³²

The methodology of research programmes presents a very different picture of the game of science from the picture of the methodological falsificationist. The best opening gambit is not a falsifiable (and therefore consistent) hypothesis, but a research programme. Mere "falsification" (in Popper's sense) must not imply rejection.³³ Mere "falsifications" (that is, anomalies) are to be recorded but need not be acted upon. Popper's great negative crucial experiments disappear; "crucial experiment" is an honourific title, which may, of course, be conferred on certain anomalies, but only *long after the event*, only when one programme has been defeated by another one. According to Popper a crucial experiment is described by an accepted basic statement which is inconsistent with a theory—according to the methodology of scientific research program-

³¹The falsificationist hotly denies this: "Learning from experience is learning from a refuting instance. The refuting instance then becomes a problematic instance." (Agassi [1964], p. 201). In his [1969] Agassi attributed to Popper the statement that "we learn from experience by refutations" (p. 169), and adds that according to Popper one can learn *only* from refutation but not from corroboration (p. 167). Feyerabend, even in his [1969], says that "*negative instances suffice in science*". But these remarks indicate a very one-sided theory of learning from experience. (Cf. my [1970], p. 121, footnote 1, and p. 123.)

³²Duhem, as a staunch positivist within philosophy of science, would, no doubt, exclude most "metaphysics" as unscientific and would not allow it to have any influence on science proper.

³³Cf. my [1968a], pp. 383–6, my [1968b], pp. 162–7, and my [1970], pp. 116ff. and pp. 155ff.

mes no accepted basic statement *alone* entitles the scientist to reject a theory. Such a clash may present a problem (major or minor), but in no circumstance a "victory". Nature may shout *no*, but human ingenuity—contrary to Weyl and Popper³⁴—may always be able to shout louder. With sufficient resourcefulness and some luck, any theory can be defended "progressively" for a long time, even if it is false. The Popperian pattern of "conjectures and refutations", that is the pattern of trial-by-hypothesis followed by error-shown-by-experiment, is to be abandoned: no experiment is crucial at the time—let alone before—it is performed (except, possibly, psychologically).

It should be pointed out, however, that the methodology of scientific research programmes has more teeth than Duhem's conventionalism: instead of leaving it to Duhem's unarticulated common sense³⁵ to judge when a "framework" is to be abandoned, I inject some hard Popperian elements into the appraisal of whether a programme progresses or degenerates or of whether one is overtaking another. That is, I give criteria of progress and stagnation within a programme and also rules for the "elimination" of whole research programmes. A research programme is said to be *progressing* as long as its theoretical growth anticipates its empirical growth, that is, as long as it keeps predicting novel facts with some success ("progressive problemshift"); it is *stagnating* if its theoretical growth lags behind its empirical growth, that is, as long as it gives only *post-hoc* explanations either of chance discoveries or of facts anticipated by, and discovered in, a rival programme ("degenerating problemshift").³⁶ If a research programme progressively explains more than

³⁴Cf. Popper [1934], section 85.

³⁵Cf. Duhem [1905], Part II, Chapter VI, § 10.

³⁶In fact, I define a research programme as degenerating even if it anticipates novel facts but does so in a patched-up development rather than by a coherent, pre-planned positive heuristic. I distinguish three types of *ad hoc* auxiliary hypotheses: those which have no excess empirical content over their predecessor ("*ad hoc*₁"), those which do have such excess content but none of it is corroborated ("*ad hoc*₂"), and finally those which are not *ad hoc* in these two senses but do not form an integral part of the positive heuristic ("*ad hoc*₃"). Examples for an *ad hoc*₁ hypothesis are provided by the linguistic prevarications of pseudosciences, or by the conventionalist stratagems discussed in my [1963-4], like "monsterbarring", "exceptionbarring", "monsteradjustment", etc. A famous example of an *ad hoc*₂ hypothesis is provided by the Lorentz-Fitzgerald contraction hypothesis; an example of an *ad hoc*₃ hypothesis is Planck's first correction of the Lummer-Pringsheim formula (also cf. p. 211). Some of the cancerous growth in contemporary social "sciences" consists of a cobweb of such *ad hoc*₃ hypotheses, as shown by Meehl and Lykken. (For references, cf. my [1970], p. 175, footnotes 2 and 3.)

a rival, it "supersedes" it, and the rival can be eliminated (or, if you wish, "shelved").³⁷

(Within a research programme a theory can only be eliminated by a better theory, that is, by one which has excess empirical content over its predecessors, some of which is subsequently confirmed. And for this replacement of one theory by a better one, the first theory does not even have to be "falsified" in Popper's sense of the term. Thus progress is marked by instances verifying excess content rather than by falsifying instances;³⁸ empirical "falsification" and actual "rejection" become independent.³⁹ Before a theory has been modified we can never know in what way it had been "refuted", and some of the most interesting modifications are motivated by the "positive heuristic" of the research programme rather than by anomalies. This difference alone has important consequences and leads to a rational reconstruction of scientific change very different from that of Popper's.)⁴⁰

It is very difficult to decide, especially since one must not demand progress at each single step, when a research programme has degenerated hopelessly or when one of two rival programmes has achieved a decisive advantage over the other. In this methodology, as in Duhem's conven-

³⁷The rivalry of two research programmes is, of course, a protracted process during which it is rational to work in either or, if one can, in both. The latter pattern becomes important, for instance, when one of the rival programmes is vague and its opponents wish to develop it in a sharper form in order to show up its weakness. Newton elaborated Cartesian vortex theory in order to show that it is inconsistent with Kepler's laws. (Simultaneous work on rival programmes, of course, undermines Kuhn's thesis of the psychological incommensurability of rival paradigms.)

The progress of one programme is a vital factor in the degeneration of its rival. If programme P_1 constantly produces "novel facts" these, by definition, will be anomalies for the rival programme P_2 . If P_2 accounts for these novel facts only in an *ad hoc* way, it is degenerating by definition. Thus the more P_1 progresses, the more difficult it is for P_2 to progress.

³⁸Cf. especially my [1970], pp. 120–1.

³⁹Cf. especially my [1968a], p. 385 and [1970], p. 121.

⁴⁰For instance, a rival theory, which acts as an *external catalyst* for the Popperian falsification of a theory, here becomes an *internal factor*. In Popper's (and Feyerabend's) reconstruction such a theory, after the falsification of the theory under test, can be removed from the rational reconstruction; in my reconstruction it has to stay within the internal history lest the falsification be undone. (Cf. above, footnote 27.)

Another important consequence is the difference between Popper's discussion of the Duhem-Quine argument and mine; cf. on the one hand Popper [1934], last paragraph of section 18 and section 19, footnote 1; Popper [1957b], pp. 131–3; Popper [1963a], p. 112, footnote 26; pp. 238–9 and p. 243; and on the other hand, cp. my [1970], pp. 184–9.

tionalism, there can be no instant—let alone mechanical—rationality. *Neither the logician's proof of inconsistency nor the experimental scientist's verdict of anomaly can defeat a research programme in one blow.* One can be "wise" only after the event.⁴¹

In this code of scientific honor modesty plays a greater role than in other codes. One *must* realize that one's opponent, even if lagging badly behind, may still stage a comeback. No advantage for one side can ever be regarded as absolutely conclusive. There is never anything inevitable about the triumph of a programme. Also, there is never anything inevitable about its defeat. Thus pigheadedness, like modesty, has more "rational" scope. *The scores of the rival sides, however, must be recorded⁴² and publicly displayed at all times.*

We should here at least refer to the main epistemological problem of the methodology of scientific research programmes. As it stands, like Popper's methodological falsificationism, it represents a very radical version of conventionalism. One needs to posit some extra-methodological inductive principle to relate—even if tenuously—the scientific gambit of pragmatic acceptances and rejections to verisimilitude.⁴³ Only such an "inductive principle" can turn science from a mere game into an epistemologically rational exercise; from a set of lighthearted sceptical gambits pursued for intellectual fun into a—more serious—fallibilist venture of approximating the Truth about the Universe.⁴⁴

The methodology of scientific research programmes constitutes, like any other methodology, a historiographical research programme. The historian who accepts this methodology as a guide will look in history for rival research programmes, for progressive and degenerating problemshifts. Where the Duhemian historian sees a revolution merely in simplicity (like that of Copernicus), he will look for a large scale progressive programme overtaking a degenerating one. Where the falsificationist sees a crucial negative experiment, he will "predict" that there was none, that behind any alleged crucial experiment, behind any alleged single battle between theory and experiment, there is a hidden war of at-

⁴¹For the falsificationist this is a repulsive idea; cf. e.g. Agassi [1963], pp. 48ff.

⁴²Feyerabend seems now to deny that even this is a possibility; cf. his [1970a] and especially [1970b] and [1972].

⁴³I use "verisimilitude" here in Popper's technical sense, as the difference between the truth content and falsity content of a theory. Cf. his [1963a], Chapter 10.

⁴⁴For a more general discussion of this problem, cf. pp. 218–219.

trition between two research programmes. The outcome of the war is only later linked in the falsificationist reconstruction with some alleged single "crucial experiment".

The methodology of research programmes—like any other theory of scientific rationality—must be supplemented by empirical-external history. No rationality theory will ever solve problems like why Mendelian genetics disappeared in Soviet Russia in the 1950's or why certain schools of research into genetic racial differences or into the economics of foreign aid came into disrepute in the Anglo-Saxon countries in the 1960's. Moreover, to explain different speeds of development of different research programmes we may need to invoke external history. Rational reconstruction of science (in the sense in which I use the term) cannot be comprehensive since human beings are not *completely* rational animals; and even when they act rationally they may have a false theory of their own rational actions.⁴⁵

But the methodology of research programmes draws a demarcation between internal and external history which is markedly different from that drawn by other rationality theories. For instance, what for the falsificationist looks like the (regrettably frequent) phenomenon of irrational adherence to a "refuted" or to an inconsistent theory and which he therefore relegates to *external* history, may well be explained in terms of my methodology *internally* as a rational defense of a promising research programme. Or, the successful predictions of novel facts which constitute serious evidence for a research programme and therefore vital parts of internal history, are irrelevant both for the inductivist and for the falsificationist.⁴⁶ For the inductivist and the falsificationist it does not really matter whether the discovery of a fact preceded or followed a theory: only their logical relation is decisive. The "irrational" impact of the historical coincidence that a theory happened to have *anticipated* a factual discovery has no internal significance. Such anticipations constitute "not proof but [mere] propaganda".⁴⁷ Or again, take Planck's

⁴⁵Also cf. above, pp. 199, 202, 204, and below, pp. 216 and 235-236.

⁴⁶The reader should remember that in this paper I discuss only naive falsificationism; cf. above, footnote 19.

⁴⁷This is Kuhn's comment on Galileo's successful prediction of the phases of Venus (Kuhn [1957], p. 224). Like Mill and Keynes before him, Kuhn cannot understand why the historic order of theory and evidence should count, and he cannot see the importance of the fact that Copernicans predicted the phases of Venus, while Tychonians only explained them by *post hoc* adjustments. Indeed, since he does not see the importance of the fact, he does not even care to mention it.

discontent with his own 1900 radiation formula, which he regarded as "arbitrary". For the falsificationist the formula was a bold, falsifiable hypothesis and Planck's dislike of it a non-rational mood, explicable only in terms of psychology. However, in my view, Planck's discontent can be explained internally: it was a rational condemnation of an "*ad hoc*" theory.⁴⁸ To mention yet another example: for falsificationism irrefutable "metaphysics" is an external intellectual influence; in my approach it is a vital part of the rational reconstruction of science.

Most historians have hitherto tended to regard the solution of some problems as being the monopoly of externalists. One of these is the problem of the high frequency of *simultaneous discoveries*. For this problem vulgar-Marxists have an easy solution: a discovery is made by many people at the same time, once a social need for it arises.⁴⁹ Now what constitutes a "discovery", and especially a major discovery, depends on one's methodology. For the inductivist, the most important discoveries are factual, and indeed such discoveries are frequently made simultaneously. For the falsificationist a *major* discovery consists in the discovery of a theory rather than of a fact. Once a theory is discovered (or rather invented), it becomes public property; and nothing is more obvious than that several people will test it simultaneously and make, simultaneously, (minor) factual discoveries. Also, a published theory is a challenge to devise higher-level, independently testable explanations. For example, given Kepler's ellipses and Galileo's rudimentary dynamics, simultaneous "discovery" of an inverse square law is not so very surprising: a problem-situation being public, simultaneous solutions can be explained on *purely internal* grounds.⁵⁰ The discovery of a new problem however may not be so readily explicable. If one thinks of the history of science as of one of rival research programmes, then most simultaneous discoveries, theoretical or factual, are explained by the fact that research programmes being public property, many people work on them in different corners of the world, possibly not knowing of each other. However, really *novel, major, revolutionary* developments are rarely invented simultaneously. Some alleged simultaneous discoveries of novel programmes are seen as having been simultaneous discoveries only with false

⁴⁸Cf. above, footnote 36.

⁴⁹For a statement of this position and an interesting critical discussion cf. Polanyi [1951], pp. 4ff. and pp. 78ff.

⁵⁰Cf. Popper [1963b] and Musgrave [1969].

hind sight: in fact they are *different* discoveries merged only later into a single one.⁵¹

A favourite hunting ground of externalists has been the related problem of why so much importance is attached to—and energy spent on—*priority disputes*. This can be explained only *externally* by the inductivist, naive falsificationist, or the conventionalist; but in the light of the methodology of research programmes some priority disputes are vital *internal* problems, since in this methodology it becomes all-important for rational appraisal which programme was first in anticipating a novel fact and which fitted in the by now old fact only later. Some priority disputes can be explained by rational interest and not simply by vanity and greed for fame. It then becomes important that Tychonian theory, for instance, succeeded in explaining—only *post hoc*—the observed phases of, and the distance to, Venus which were originally precisely anticipated by Copernicans;⁵² or that Cartesians managed to explain everything that the Newtonians predicted—but only *post hoc*. Newtonian optical theory explained *post hoc* many phenomena which were anticipated and first observed by Huygensi ans.⁵³

All these examples show how the methodology of scientific research programmes turns many problems which had been *external* problems for other historiographies into internal ones. But occasionally the borderline is moved in the opposite direction. For instance there may have been an experiment which was accepted *instantly*—in the absence of a better theory—as a negative crucial experiment. For the falsificationist such ac-

⁵¹This was illustrated convincingly by Elkana for the case of the so-called simultaneous discovery of the conservation of energy; cf. his [1971].

⁵²Also cf. above, footnote 46.

⁵³For the Mertonian brand of functionalism—as Alan Musgrave pointed out to me—priority disputes constitute a *prima facie* disfunction and therefore an anomaly for which Merton has been laboring to give a general socio-psychological explanation. (Cf. e.g. Merton [1957], [1963], and [1969].) According to Merton “scientific knowledge is not the richer or the poorer for having credit given where credit is due: it is the social *institution* of science and individual men of science that would suffer from repeated failure to allocate credit justly” (Merton [1957], p. 648). But Merton overdoes his point: in important cases (like in some of Galileo’s priority fights) there was more at stake than institutional interests: the problem was whether the Copernican research programme was progressive or not. (Of course, not all priority disputes have scientific relevance. For instance, the priority dispute between Adams and Leverrier about who was first to discover Neptune had no such relevance: whoever discovered it, the discovery strengthened the same (Newtonian) programme. In such cases Merton’s external explanation may well be true.)

ceptance is part of internal history; for me it is not rational and has to be explained in terms of external history.

Note. The methodology of research programmes was criticized both by Feyerabend and by Kuhn. According to Kuhn: “[Lakatos] must specify criteria which can be used at the time to distinguish a degenerative from a progressive research programme; and so on. Otherwise, he has told us nothing at all.”⁵⁴ Actually, I do specify such criteria. But Kuhn probably meant that “[my] standards have practical force only if they are combined with a time limit (what looks like a degenerating problemshift may be the beginning of a much longer period of advance).”⁵⁵ Since I specify no such time limit, Feyerabend concludes that my standards are no more than “verbal ornaments”.⁵⁶ A related point was made by Musgrave in a letter containing some major constructive criticisms of an earlier draft, in which he demanded that I specify, for instance, at what point dogmatic adherence to a programme ought to be explained “externally” rather than “internally”.

Let me try to explain why such objections are beside the point. One may rationally stick to a degenerating programme until it is overtaken by a rival *and even after*. What one must not do is to deny its poor public record. Both Feyerabend and Kuhn conflate *methodological* appraisal of a programme with firm *heuristic* advice about what to do.⁵⁷ It is perfectly rational to play a risky game: what is irrational is to deceive oneself about the risk.

This does not mean as much license as might appear for those who stick to a degenerating programme. For they can do this mostly only in private. Editors of scientific journals should refuse to publish their papers which will, in general, contain either solemn reassessments of their position or absorption of counterevidence (or even of rival programmes) by *ad hoc* linguistic adjustments. Research foundations, too, should refuse money.⁵⁸

⁵⁴Kuhn [1970], p. 239; my italics.

⁵⁵Feyerabend [1970a], p. 215.

⁵⁶Ibid.

⁵⁷Cf. above, footnote 2.

⁵⁸I do, of course, not claim that such decisions are necessarily uncontroversial. In such decisions one has to use also one’s *common sense*. Common sense (that is, judgment in *particular* cases which is not made according to mechanical rules but only follows general principles which leave some *Spielraum*) plays a role in all brands of non-mechanical methodologies. The Duhemian conventionalist needs

These observations also answer Musgrave's objection by separating rational and irrational (or honest and dishonest) adherence to a degenerating programme. They also throw further light on the demarcation between internal and external history. They show that internal history is self-sufficient for the presentation of the history of disembodied science, including degenerating problemshifts. External history explains why some people have false beliefs about scientific progress, and how their scientific activity may be influenced by such beliefs.

E. Internal and External History

Four theories of the rationality of scientific progress—or logics of scientific discovery—have been sketchily discussed. It was shown how each of them provides a theoretical framework for the rational reconstruction of the history of science.

Thus the internal history of *inductivists* consists of alleged discoveries of hard facts and of so-called inductive generalizations. The internal history of *conventionalists* consists of factual discoveries and of the erection of pigeonhole systems and their replacement by allegedly simpler ones.⁵⁹ The internal history of *falsificationists* dramatizes bold conjectures, improvements which are said to be *always* content-increasing and, above all, triumphant “negative crucial experiments”. The *methodology of research programmes*, finally, emphasizes long-extended theoretical and empirical rivalry of major research programmes, progressive and degenerating problemshifts, and the slowly emerging victory of one programme over the other.

common sense to decide when a theoretical framework has become sufficiently cumbersome to be replaced by a “simpler” one. The Popperian falsificationist needs common sense to decide when a basic statement is to be “accepted”, or to which premise the *modus tollens* is to be directed (cf. my [1970], pp. 106ff.). But neither Duhem, nor Popper gives a blank cheque to “common sense”. They give very definite guidance. The Duhemian judge directs the jury of common sense to agree on comparative simplicity; the Popperian judge directs the jury to look out primarily for, and agree upon, accepted basic statements which clash with accepted theories. My judge directs the jury to agree on appraisals of progressive and degenerating research programmes. But, for example, there may be conflicting views about whether an accepted basic statement expresses a *novel* fact or not. Cf. my [1970], p. 156.

Although it is important to reach agreement on such verdicts, there must also be the possibility of appeal. In such appeals inarticulated common sense is questioned, articulated and criticized. (The criticism may even turn from a criticism of law interpretation into a criticism of the law itself.)

⁵⁹Most conventionalists have also an intermediate inductive layer of “laws” between facts and theories; cf. above, footnote 15.

Each rational reconstruction produces some characteristic pattern of rational growth of scientific knowledge. But all of these *normative* reconstructions may have to be supplemented by *empirical* external theories to explain the residual non-rational factors. The history of science is always richer than its rational reconstruction. *But rational reconstruction or internal history is primary, external history only secondary, since the most important problems of external history are defined by internal history.* External history either provides non-rational explanation of the speed, locality, selectiveness etc. of historic events *as interpreted* in terms of internal history; or, when history differs from its rational reconstruction, it provides an empirical explanation of why it differs. But the *rational* aspect of scientific growth is fully accounted for by one's logic of scientific discovery.

Whatever problem the historian of science wishes to solve, he has first to reconstruct the relevant section of the growth of objective scientific knowledge, that is, the relevant section of “internal history”. As it has been shown, what constitutes for him internal history depends on his philosophy, whether he is aware of this fact or not. Most theories of the growth of knowledge are theories of the growth of disembodied knowledge: whether an experiment is crucial or not, whether a hypothesis is highly probable in the light of the available evidence or not, whether a problems-shift is progressive or not, is not dependent in the slightest on the scientist's beliefs, personalities or authority. These subjective factors are of no interest for any internal history. For instance, the “internal historian” records the Proutian programme with its hard core (that atomic weights of pure chemical elements are whole numbers) and its positive heuristic (to overthrow, and replace, the contemporary false observational theories applied in measuring atomic weights). This programme was later carried through.⁶⁰ The internal historian will waste

⁶⁰The proposition “the Proutian programme was carried through” looks like a “factual” proposition. But there are no “factual” propositions: the phrase only came into ordinary language from dogmatic empiricism. *Scientific “factual” propositions* are theory-laden: the theories involved are “observational theories”. *Historiographical “factual” propositions* are also theory-laden: the theories involved are methodological theories. In the decision about the truth-value of the “factual” proposition, “the Proutian programme was carried through”, two methodological theories are involved. First, the theory that the units of scientific appraisal are research programmes; secondly, some *specific* theory of how to judge whether a programme was “in fact” carried through. For all these considerations a Popperian internal historian will not need to take any interest whatsoever in the *persons* involved, or in their beliefs about their own activities.

little time on Prout's *belief* that if the "experimental techniques" of *his time* were "carefully" applied, and the experimental findings properly interpreted, the anomalies would *immediately* be seen as mere illusions. The internal historian will regard this historical fact as a fact in the second world which is only a caricature of its counterpart in the third world.⁶¹ Why such caricatures come about is none of his business; he might—in a footnote—pass on to the externalist the problem of why certain scientists had "false beliefs" about what they were doing.⁶²

Thus in constructing internal history the historian will be highly selective: he will omit everything that is irrational in the light of his rationality theory. But this normative selection still does not add up to a fully fledged rational reconstruction. For instance, Prout never articulated the "Proutian programme": the Proutian programme is not Prout's programme. *It is not only the ("internal") success or the ("internal") defeat of a programme which can only be judged with hindsight: it is frequently also its content.* Internal history is not just a *selection* of methodologically interpreted facts: it may be, on occasions, their *radically improved version*. One may illustrate this using the Bohrian programme. Bohr, in 1913, may not have even thought of the possibility of electron spin. He had more than enough on his hands without the spin. Nevertheless, the historian, describing with hindsight the Bohrian programme, should include electron spin in it, since electron spin fits naturally into the original outline of the programme. Bohr might have referred to it in 1913. Why Bohr did not do so, is an interesting problem which deserves to be indicated in a footnote.⁶³ (Such problems might then be solved either internally by pointing to rational reasons in the growth of objective, impersonal knowledge; or externally by pointing to

⁶¹The "first world" is that of matter, the "second" the world of feelings, beliefs, consciousness, the "third" the world of objective knowledge, articulated in propositions. This is an age-old and vitally important trichotomy; its leading contemporary proponent is Popper. Cf. Popper [1968a], [1968b] and Musgrave [1969] and [1971a].

⁶²Of course what, in this context, constitutes "false belief" (or "false consciousness"), depends on the rationality theory of the critic: cf. above pp. 199, 202 and 204. But no rationality theory can ever succeed in leading to "true consciousness."

⁶³If the publication of Bohr's programme had been delayed by a few years, further speculation might even have led to the spin problem without the previous observation of the anomalous Zeeman effect. Indeed, Compton raised the problem in the context of the Bohrian programme in his [1919].

psychological causes in the development of Bohr's personal beliefs.)

One way to indicate discrepancies between history and its rational reconstruction is to relate the internal history *in the text*, and indicate *in the footnotes* how actual history "misbehaved" in the light of its rational reconstruction.⁶⁴

Many historians will abhor the idea of *any* rational reconstruction. They will quote Lord Bolingbroke: "History is philosophy teaching by example." They will say that before philosophizing "we need a lot more examples."⁶⁵ But such an inductivist theory of historiography is utopian.⁶⁶ *History without some* theoretical "bias" is impossible.⁶⁷ Some historians look for the discovery of hard facts, inductive generalizations, others for bold theories and crucial negative experiments, yet others for great simplifications, or for progressive and degenerating problemshifts; all of them have *some* theoretical "bias". This bias, of course, may be obscured by an eclectic variation of theories or by theoretical confusion: but neither eclecticism nor confusion amounts to an atheoretical outlook. What a historian regards as an external problem is often an excellent guide to his implicit methodology: some will ask why a "hard fact" or a "bold theory" was discovered exactly when and where it actually was discovered; others will ask why a "degenerating problemshift" could have wide

⁶⁴I first applied this expositional device in my [1963–4]; I used it again in giving a detailed account of the Proutian and the Bohrian programmes; cf. my [1970], pp. 138, 140, 146. This practice was criticized at the 1969 Minneapolis conference by some historians. McMullin, for instance, claimed that this presentation may illuminate a *methodology*, but certainly not real *history*; the text tells the reader what ought to have happened and the footnotes what in fact happened (cf. McMullin [1970]). Kuhn's criticism of my exposition ran essentially on the same lines: he thought that it was a specifically *philosophical* exposition: "a historian would not include *in his narrative* a factual report which he knows to be false. If he had done so, he would be so sensitive to the offense that he could not conceivably compose a footnote calling attention to it". (Cf. Kuhn [1970] p. 256.)

⁶⁵Cf. L. P. Williams [1970].

⁶⁶Perhaps I should emphasize the difference between on the one hand, *inductivist historiography of science*, according to which *science* proceeds through discovery of hard facts (in nature) and (possibly) inductive generalizations, and, on the other hand, the *inductivist theory of historiography of science* according to which *historiography of science* proceeds through discovery of hard facts (in history of science) and (possibly) inductive generalizations. "Bold conjectures", "crucial negative experiments", and even "progressive and degenerating research programmes" may be regarded as "hard historical facts" by some inductivist historiographers. One of the weaknesses of Agassi's [1963] is that he omitted to emphasize this distinction between scientific and historiographical inductivism.

⁶⁷Cf. Popper [1957b], section 31.

popular acclaim over an incredibly long period or why a "progressive problemshift" was left "unreasonably" unacknowledged.⁶⁸ Long texts have been devoted to the problem of whether, and if so, why, the emergence of science was a purely European affair; but such an investigation is bound to remain a piece of confused rambling until one clearly defines "science" according to some normative philosophy of science. One of the most interesting problems of external history is to specify the psychological, and indeed, social conditions which are necessary (but, of course, never sufficient) to make scientific progress possible; but in the very formulation of this "external" problem *some* methodological theory, *some* definition of science is bound to enter. History of science is a history of events which are selected and interpreted in a normative way.⁶⁹ This being so, the hitherto neglected problem of appraising rival logics of scientific discovery and, hence, rival reconstructions of history, acquires paramount importance. I shall now turn to this problem.

§2. CRITICAL COMPARISON OF METHODOLOGIES: HISTORY AS A TEST OF ITS RATIONAL RECONSTRUCTIONS

Theories of scientific rationality can be classified under two main heads.

(1) *Justificationist methodologies* set very high epistemological standards: for classical justificationists a proposition is "scientific" only if it is *proven*, for neojustificationists, if it is *probable* (in the sense of the probability calculus) or *corroborated* (in the sense of Popper's third note on corroboration) to a proven degree.⁷⁰ Some philosophers of sci-

⁶⁸This thesis implies that the work of those "externalists" (mostly trendy "sociologists of science") who claim to do social history of some scientific discipline without having mastered the discipline itself, and its internal history, is worthless. Also cf. Musgrave [1971a].

⁶⁹Unfortunately there is only one single word in most languages to denote history₁ (the set of historical events) and history₂ (a set of historical propositions). Any history₂ is a theory and value-laden reconstruction of history₁.

⁷⁰That is, a hypothesis *h* is scientific only if there is a number *q* such that $p(h, e) = q$ where *e* is the available evidence and $p(h, e) = q$ can be *proved*. It is irrelevant whether *p* is a Carnapian confirmation function or a Popperian corroboration function as long as $p(h, e) = q$ is allegedly proved. (Popper's third note on corroboration, of course, is only a curious slip which is out of tune with his philosophy: cf. my [1968a], pp. 411-7.)

Probabilism has never generated a programme of historiographical reconstruction; it has never emerged from grappling—unsuccessfully—with the very problems it created. As an epistemological programme it has been degenerating for a long time; as a historiographical programme it never even started.

ence gave up the idea of proving or of (provably) probabilifying scientific theories but remained dogmatic empiricists: whether inductivists, probabilists, conventionalists or falsificationists, they still stick to the provability of "factual" propositions. By now, of course, all these different forms of justificationism have crumbled under *epistemological and logical criticism*.

(2) The only alternatives with which we are left are *pragmatic-conventionalist methodologies*; crowned by some global principle of induction. Conventionalist methodologies first lay down rules about "acceptance" and "rejection" of factual and theoretical propositions—without yet laying down rules about proof and disproof, truth and falsehood. We then get *different systems of rules of the scientific game*. The inductivist game would consist of collecting "acceptable" (not proven) data and drawing from them "acceptable" (not proven) inductive generalizations. The conventionalist game would consist of collecting "acceptable" data and ordering them into the simplest possible pigeonhole systems (or devising the simplest possible pigeonhole systems and filling them with acceptable data). Popper specified yet another game as "scientific".⁷¹ Even methodologies which have been epistemologically and logically discredited, may go on functioning, in these emasculated versions, as guides for the rational reconstruction of history. But these *scientific games* are without any genuine epistemological relevance unless we superimpose on them some sort of metaphysical (or, if you wish "inductive") principle which will say that the game, as specified by the methodology, gives us the best chance of approaching the Truth. Such a principle then turns the pure conventions of the game into fallible conjectures; but without such a principle the scientific game is just like any other game.⁷²

It is very difficult to criticize conventionalist methodologies like Duhem's and Popper's. There is no obvious way to criticize either a game or a metaphysical principle of induction. In order to overcome these difficulties I am going to propose a new theory of how to appraise

⁷¹Popper [1934], sections 11 and 85. Also cf. the comment in my [1971a], footnote 13.

The methodology of research programmes too is, in the first instance, defined as a game; cf. especially above, p. 206.

⁷²This whole problem area is the subject of my [1968a], pp. 390ff. but especially of my [1971a].

such methodologies of science (the ones, which—at least in the first stage, before the introduction of an inductive principle—are conventionalist). I shall show that methodologies may be criticized without any direct reference to any epistemological (or even logical) theory, and without using directly any logico-epistemological criticism. The basic idea of this criticism is that *all methodologies function as historiographical (or meta-historical) theories (or research programmes) and can be criticized by criticizing the rational historical reconstructions to which they lead.*

I shall try to develop this historiographical method of criticism in a dialectical way. I start with a special case: I first “refute” falsificationism by “applying” falsificationism (on a normative-historiographical *meta-level*) to itself. Then I shall apply falsificationism also to inductivism and conventionalism, and, indeed, argue that all methodologies are bound to end up “falsified” with the help of this Pyrrhonian *machine de guerre*. Finally, I shall “apply” not falsificationism but the methodology of scientific research programmes (again on a normative-historiographical *meta-level*) to inductivism, conventionalism, falsificationism and to itself, and show that—on this meta-criterion—methodologies can be constructively criticized and compared. This normative-historiographical version of the methodology of scientific research programmes supplies a general theory of how to compare rival logics of discovery in which (in a sense carefully to be specified) *history may be seen as a “test” of its rational reconstructions*.

A. Falsificationism as a Meta-criterion: History “Falsifies” Falsificationism (and any other Methodology)

In their purely “methodological” versions scientific appraisals, as has already been said, are *conventions* and can always be formulated as a definition of science.⁷³ How can one criticize such a definition? If one interprets it nominalistically,⁷⁴ a definition is a mere abbreviation, a terminological suggestion, a tautology. How can one criticize a tautology?

⁷³Cf. Popper [1934], sections 4 and 11. Popper's definition of science is, of course, his celebrated “demarcation criterion”.

⁷⁴For an excellent discussion of the distinction between nominalism and realism (or, as Popper prefers to call it, “essentialism”) in the theory of definitions, cf. Popper [1945], vol. II, ch. 11, and [1963a], p. 20.

Popper, for one, claims that his definition of science is “fruitful” because “a great many points can be clarified and explained with its help”. He quotes Menger: “Definitions are dogmas; only the conclusions drawn from them can afford us any new insight.”⁷⁵ But how can a definition have explanatory power or afford new insights? Popper's answer is this: “It is only from the consequences of my definition of empirical science, and from the methodological decisions which depend upon this definition, that the scientist will be able to see how far it conforms to his intuitive idea of the goal of his endeavors.”⁷⁶

The answer complies with Popper's general position that conventions can be criticized by discussing their “suitability” relative to some purpose: “As to the suitability of any convention opinions may differ; and a reasonable discussion of these questions is only possible between parties having some purpose in common. The choice of that purpose . . . goes beyond rational argument.”⁷⁷ Indeed, Popper never offered a theory of rational criticism of consistent conventions. He does not raise, let alone answer, the question: *“Under what conditions would you give up your demarcation criterion?”*⁷⁸

But the question can be answered. I give my answer in two stages: I propose first a naive and then a more sophisticated answer. I start by recalling how Popper, according to his own account,⁷⁹ arrived at his criterion. He thought, like the best scientists of his time, that Newton's theory, although refuted, was a wonderful scientific achievement; that Einstein's theory was still better; and that astrology, Freudianism and twentieth century Marxism were pseudo-scientific. His problem was to find a definition of science which yielded these “basic judgments” con-

⁷⁵Popper [1934], section 11.

⁷⁶*Ibid.*

⁷⁷Popper [1934], section 4. But Popper, in his *Logik der Forschung* never specifies a *purpose* of the game of science that would go beyond what is contained in its rules. The thesis that the *aim* of science is *truth*, occurs only in his writings since 1957. All that he says in his *Logik der Forschung* is that the quest for truth may be a psychological *motive* of scientists. For a detailed discussion cf. my [1971a].

⁷⁸This flaw is the more serious since Popper himself has expressed qualifications about his criterion. For instance in his [1963a] he describes “dogmatism”, that is, treating anomalies as a kind of “background noise”, as something that is “to some extent necessary” (p. 49). But on the next page he identifies this “dogmatism” with “pseudoscience”. Is then pseudoscience “to some extent necessary”? Also cf. my [1970], p. 177, footnote 3.

⁷⁹Cf. Popper [1957], pp. 33–7.

cerning particular theories; and he offered a novel solution. Now let us consider the proposal that a *rationality theory*—or *demarcation criterion*—*is to be rejected if it is inconsistent with an accepted “basic value judgment” of the scientific élite*. Indeed, this meta-methodological rule (*meta-falsificationism*) would seem to correspond to Popper's methodological rule (falsificationism) that a scientific theory is to be rejected if it is inconsistent with an (“empirical”) basic statement unanimously accepted by the scientific community. Popper's whole methodology rests on the contention that there exist (relatively) singular statements on whose truth-value scientists can reach unanimous agreement; without such agreement there would be a new Babel and “the soaring edifice of science would soon lie in ruins”.⁸⁰ But even if there were an agreement about “basic” statements, if there were no agreement about how to appraise scientific achievement relative to this “empirical basis”, would not the soaring edifice of science equally soon lie in ruins? No doubt it would. While there has been little agreement concerning a *universal* criterion of the scientific character of theories, there has been considerable agreement over the last two centuries concerning *single* achievements. While there has been no *general* agreement concerning a theory of scientific rationality, there has been considerable agreement concerning whether a particular single step in the game was scientific or crankish, or whether a particular gambit was played correctly or not. A general definition of science thus must reconstruct the acknowledgedly best gambits as “scientific”: if it fails to do so, it has to be rejected.⁸¹

Then let us propose tentatively that *if a demarcation criterion is in-*

⁸⁰Popper [1934], section 29.

⁸¹This approach, of course, does not imply that we *believe* that the scientists' “basic judgments” are unfailingly rational; it only means that we *accept* them in order to criticize universal definitions of science. (If we were to add that no such *universal* definition has been found and no such *universal* definition will ever be found, the stage would be set for Polanyi's conception of the lawless closed autocracy of science).

My meta-criterion may be seen as a “quasi-empirical” self-application of Popperian falsificationism. I introduced this “quasi-empiricalness” earlier in the context of mathematical philosophy. We may abstract from *what* flows in the logical channels of a deductive system, whether it is something certain or something fallible, whether it is truth and falsehood or probability and improbability, or even moral or scientific desirability and undesirability: it is the *how* of the flow which decides whether the system is negativist, “quasi-empirical”, dominated by *modus tollens* or whether it is justificationist, “quasi-Euclidean”, dominated by *modus ponens*. (Cf. my [1967].) This “quasi-empirical” approach may be applied to *any* kind of normative knowledge: Watkins has already applied it to ethics in his [1963] and [1967]. But now I prefer another approach: cf. below, footnote 123.

consistent with the “basic” appraisals of the scientific élite, it should be rejected.

Now if we apply this quasi-empirical meta-criterion (which I am going to reject later), Popper's demarcation criterion—that is, Popper's rules of the game of science—has to be rejected.⁸²

Popper's basic rule is that the scientist must specify in advance under what experimental conditions he will give up even his most basic assumptions. For instance, he writes, when criticizing psychoanalysis: “*Criteria of refutation* have to be laid down beforehand: it must be agreed which observable situations, if actually observed, mean that the theory is refuted. But what kind of clinical responses would refute to the satisfaction of the analyst *not merely a particular analytic diagnosis but psychoanalysis itself?* And have such criteria ever been discussed or agreed upon by analysts?”⁸³ In the case of psychoanalysis Popper was right: no answer has been forthcoming. Freudians have been nonplussed by Popper's basic challenge concerning scientific honesty. Indeed, they have refused to specify experimental conditions under which they would give up their basic assumptions. For Popper this was the hallmark of their intellectual dishonesty. But what if we put Popper's question to the Newtonian scientist: “What kind of observation would refute to the satisfaction of the Newtonian not merely a particular Newtonian explanation but Newtonian dynamics and gravitational theory itself? And have such criteria ever been discussed or agreed upon by Newtonians?” The Newtonian will, alas, scarcely be able to give a positive answer.⁸⁴ But then if analysts are to be condemned as dishonest by Popper's standards, Newtonians must also be condemned. Newtonian science, however, in spite of this sort of “dogmatism”, is highly regarded by the greatest scientists, and, indeed, by Popper himself. Newtonian “dogmatism” then is a “falsification” of Popper's definition: it defies Popper's rational reconstruction.

Popper may certainly withdraw his celebrated challenge and demand falsifiability—and rejection on falsification—only for systems of theories,

⁸²It may be noted that this meta-criterion does not have to be construed as psychological, or “naturalistic” in Popper's sense. (Cf. his [1934], section 10.) The definition of the “scientific élite” is not simply an empirical matter.

⁸³Popper [1963a], p. 38, footnote 3; my italics. This, of course, is equivalent to his celebrated “demarcation criterion” between [internal, rationally reconstructed] science and non-science (or “metaphysics”). The latter may be [externally] “influential” and has to be branded as pseudoscience only if it declares itself to be science.

⁸⁴Cf. my [1970], pp. 100–1.

including initial conditions and all sorts of auxiliary and observational theories.⁸⁵ This is a considerable withdrawal, for it allows the imaginative scientist to save his pet theory by suitable lucky alterations in some odd obscure corner on the periphery of his theoretical maze. But even Popper's mitigated rule will show up even the most brilliant scientists as irrational dogmatists. For in large research programmes there are always known anomalies: normally the researcher puts them aside and follows the positive heuristic of the programme.⁸⁶ In general he rivets his attention on the positive heuristic rather than on the distracting anomalies, and hopes that the "recalcitrant instances" will be turned into confirming instances as the programme progresses. On Popper's terms the greatest scientists in these situations used forbidden gambits, *ad hoc* stratagems: instead of regarding Mercury's anomalous perihelion as a falsification of the Newtonian theory of our planetary system and thus as a reason for its rejection, most physicists shelved it as a problematic instance to be solved at some later stage—or offered *ad hoc* solutions. This methodological attitude of treating as (mere) *anomalies* what Popper would regard as (dramatic) counterexamples is commonly accepted by the best scientists. Some of the research programmes now held in highest esteem by the scientific community progressed in an ocean of anomalies.⁸⁷ That in their choice of problems the greatest scientists "uncritically" ignore anomalies (and that they isolate them with the help of *ad hoc* stratagems) offers, at least on our meta-criterion, a further falsification of Popper's methodology. He cannot interpret as rational some most important patterns in the growth of science.

Furthermore, for Popper, working on *an inconsistent system* must invariably be regarded as irrational "a self-contradictory system must be rejected . . . [because it] is uninformative. . . . No statement is singled out . . . since all are derivable".⁸⁸ But some of the greatest scientific research programmes progressed on inconsistent foundations.⁸⁹ Indeed in such cases the best scientist's rule is frequently: "*Allez en avant et la foi vous viendra*". This anti-Popperian methodology secured a breathing space for both the infinitesimal calculus and for naive set theory when they were bedevilled by logical paradoxes.

⁸⁵Cf. e.g. his [1934], section 18.

⁸⁶Cf. my [1970], especially pp. 135ff.

⁸⁷*Ibid.*, pp. 138ff.

⁸⁸Cf. Popper [1934], section 24.

⁸⁹Cf. my [1970], especially pp. 140ff.

Indeed, if the game of science had been played according to Popper's rule book, Bohr's 1913 paper would never have been published because it was inconsistently grafted onto Maxwell's theory, and Dirac's delta functions would have been suppressed until Schwartz. All these examples of research based on inconsistent foundations constitute further "falsifications" of falsificationist methodology.⁹⁰

Thus several of the "basic" appraisals of the scientific élite "falsify" Popper's definition of science and scientific ethics. The problem then arises, to what extent, given these considerations, can falsificationism function as a guide for the historian of science. The simple answer is, to a very small extent. Popper, the leading falsificationist, never wrote any history of science; possibly because he was too sensitive to the judgment of great scientists to pervert history in a falsificationist vein. One should remember that while in his autobiographical recollections he mentions Newtonian science as the paradigm of scientificness, that is, of falsifiability, in his classical *Logik der Forschung* the falsifiability of Newton's theory is nowhere discussed. The *Logik der Forschung*, on the whole, is dryly abstract and highly ahistorical.⁹¹ Where Popper does venture to remark casually on the falsifiability of major scientific theories, he either plunges into some logical blunder⁹² or distorts history to fit his rationality theory. If a historian's methodology provides a poor rational reconstruction, he may either misread history in such a way that it coincides with his rational reconstruction, or he will find that the history of science is highly irrational. Popper's respect for great science made him choose the first option, while the disrespectful Feyerabend chose the second.⁹³ Thus

⁹⁰In general Popper stubbornly overestimates the immediate striking force of purely negative criticism. "Once a mistake, or a contradiction, is pinpointed, there can be no verbal evasion: it can be proved, and that is that" (Popper [1959], p. 394). He adds: "Frege did not try evasive manoeuvres when he received Russell's criticism." But of course he did. (Cf. Frege's *Postscript* to the second edition of his *Grundgesetze*.)

⁹¹Interestingly, as Kuhn points out, "a consistent interest in historical problems and a willingness to engage in original historical research distinguishes the men [Popper] has trained from the members of any other current school in the philosophy of science" (Kuhn [1970], p. 236). For a hint at a possible explanation of the apparent discrepancy cf. below, footnote 130.

⁹²For instance, he claims that a perpetual motion machine would "refute" (on his terms) the first law of thermodynamics ([1934], section 15). But how can one interpret, on Popper's own terms, the statement that "K is a perpetual motion machine" as a "basic", that is, as a spatio-temporally observational singular statement?

⁹³I am referring to Feyerabend's [1970b] and [1972].

Popper, in his historical asides, tends to turn anomalies into "crucial experiments" and to exaggerate their immediate impact on the history of science. Through his spectacles, great scientists accept refutations readily and this is the primary source of their problems. For instance, in one place he claims that the Michelson-Morley experiment decisively overthrew classical ether theory; he also exaggerates the role of this experiment in the emergence of Einstein's relativity theory.⁹⁴ It takes a naive falsificationist's simplifying spectacles to see, with Popper, Lavoisier's classical experiments as refuting (or as "tending to refute") the phlogiston theory; or to see the Bohr-Kramers-Slater theory as being knocked out with a single blow from Compton; or to see the parity principle "rejected" by "counterexample".⁹⁵

Furthermore, if Popper wants to reconstruct the provisional acceptance of theories as rational on *his* terms, he is bound to ignore the historical fact that most important theories are born refuted and that some laws are further explained, rather than rejected, in spite of the known counterexamples. He tends to turn a blind eye on all anomalies known before the one which later was enthroned as "crucial counter-evidence".

⁹⁴Cf. Popper [1934], section 30 and Popper [1945], vol. II, pp. 220–1. He stressed that Einstein's problem was how to account for experiments "refuting" classical physics and he "did not . . . set out to criticize our conceptions of space and time". But Einstein certainly did. His Machian criticism of our concepts of space and time, and, in particular his operationalist criticism of the concept of simultaneity played an important role in his thinking.

I discussed the role of the Michelson-Morley experiments at some length in my [1970].

Popper's competence in physics would never, of course, have allowed him to distort the history of relativity theory as much as Beveridge, who wanted to persuade economists to an empirical approach by setting them Einstein as an example. According to Beveridge's falsificationist reconstruction, Einstein "started [in his work on gravitation] from facts [which refuted Newton's theory, that is,] from the movements of the planet Mercury, the unexplained aberrancies of the moon" (Beveridge [1937]). Of course, Einstein's work on gravitation grew out from a "creative shift" in the positive heuristic of his special relativity programme, and certainly not from pondering over Mercury's anomalous perihelion or the moon's devious, unexplained aberrancies.

⁹⁵Popper [1963a], pp. 220, 239, 242–3 and [1963b], p. 965. Popper, of course, is left with the problem why "counterexamples" (that is, anomalies) are not recognized immediately as causes for rejection. For instance, he points out that in the case of the breakdown of parity "there had been many observations—that is, photographs of particle tracks—from which we might have read off the result, but the observations had been either ignored or misinterpreted" ([1963b], p. 965). Popper's—external explanation seems to be that scientists have not yet learned to be sufficiently critical and revolutionary. But is not it a better—and internal—explanation that the anomalies *had* to be ignored until some progressive alternative theory was offered which turned the counterexamples into examples?

For instance, he mistakenly thinks that "neither Galileo's nor Kepler's theories were refuted before Newton".⁹⁶ The context is significant. Popper holds that the most important pattern of scientific progress is when a crucial experiment leaves one theory *unrefuted* while it refutes a rival one. But, as a matter of fact, in most, if not in all, cases where there are two rival theories, both are known to be simultaneously infected by anomalies. In such situations Popper succumbs to the temptation to simplify the situation into one to which his methodology is applicable.⁹⁷

Falsificationist historiography is then "falsified". But if we apply the same meta-falsificationist method to inductivist and conventionalist historiographies, we shall "falsify" them too.

The best logico-epistemological demolition of inductivism is, of course, Popper's; but even if we assumed that inductivism were philosophically (that is, epistemologically and logically) sound, Duhem's historiographical criticism falsifies it. Duhem took the most celebrated "successes" of inductivist historiography: Newton's law of gravitation and Ampère's electro-magnetic theory. These were said to be two most victorious applications of inductive method. But Duhem (and, following him, Popper and Agassi) showed that they were not. Their analyses illustrate how the inductivist, if he wants to show that the growth of actual science is rational, must falsify actual history out of all recognition.⁹⁸ Therefore, if the rationality of science is inductive, actual science is not rational; if it is rational, it is not inductive.⁹⁹

⁹⁶*Op. cit.*, p. 246.

⁹⁷As I mentioned, one Popperian, Agassi, did write a book on the historiography of science (Agassi [1963]). The book has some incisive critical sections flogging inductivist historiography, but he ends up by replacing inductivist mythology by falsificationist mythology. For Agassi *only* those facts have scientific (internal) significance which can be expressed in propositions which conflict with some extant theory: only their discovery deserves the honorific title "factual discovery"; factual propositions which *follow from* rather than *conflict with* known theories are irrelevant: so are factual propositions which are *independent of* them. If some valued factual discovery in the history of science is known as a confirming instance or chance discovery, Agassi boldly predicts that on *close* investigation they will turn out to be refuting instances, and he offers five case-studies to support his claim (pp. 60–74). Alas, on *closer* investigation it turns out that Agassi got wrong all the five examples which he adduced as confirming instances of his historiographical theory. In fact all the five examples (in our normative meta-falsificationist sense) "falsify" his historiography.

⁹⁸Of course, an inductivist may have the temerity to claim that genuine science has not yet started and may write a history of extant science as a history of bias, superstition and false belief.

⁹⁹Cf. Duhem [1905], Popper [1948] and [1957a], Agassi [1963].

Conventionalism—which, unlike inductivism, is no easy prey to logical or epistemological criticism¹⁰⁰—can also be historiographically falsified. One can show that the clue to scientific revolutions is not the replacement of cumbersome frameworks by simpler ones.

The Copernican revolution was generally taken to be the *paradigm of conventionalist historiography*, and it is still so regarded in many quarters. For instance Polanyi tells us that Copernicus's "simpler picture" had "striking beauty" and "[justly] carried great powers of conviction".¹⁰¹ But modern study of primary sources, particularly by Kuhn,¹⁰² has dispelled this myth and presented a clear-cut historiographical refutation of the conventionalist account. It is now agreed that the Copernican system was "at least as complex as the Ptolemaic".¹⁰³ But if this is so, then, if the acceptance of Copernican theory was rational, it was not for its superlative objective simplicity.¹⁰⁴

Thus inductivism, falsificationism and conventionalism can be falsified as rational reconstructions of history with the help of the sort of historiographical criticism I have adduced.¹⁰⁵ Historiographical falsification of inductivism, as we have seen, was initiated already by Duhem and continued by Popper and Agassi. Historiographical criticisms of [naive] falsificationism have been offered by Polanyi, Kuhn, Feyerabend and Holton.¹⁰⁶ The most important historiographical criticism of conventionalism is to be found in Kuhn's—already quoted—masterpiece on the Copernican revolution.¹⁰⁷ The upshot of these criticisms is that all these rational reconstructions of history force history of science into the Pro-

¹⁰⁰Cf. Popper [1934], Section 19.

¹⁰¹Cf. Polanyi [1951], p. 70.

¹⁰²Kuhn [1957]. Also cf. Price [1959].

¹⁰³Cohen [1960], p. 61. Bernal, in his [1954], says that "[Copernicus's] reasons for [his] revolutionary change were essentially philosophic and aesthetic [that is, in the light of conventionalism, scientific]"; but in later editions he changes his mind: "[Copernicus's] reasons were mystical rather than scientific".

¹⁰⁴For a more detailed sketch, cf. my [1973c].

¹⁰⁵Other types of criticism of methodologies may, of course, be easily devised. We may, for instance, apply the standards of each methodology (not only falsificationism) to itself. The result, for most methodologies, will be equally destructive: inductivism cannot be proved inductively, simplicity will be seen as hopelessly complex. (For the latter cf. below, footnote 107.)

¹⁰⁶Cf. Polanyi [1958], Kuhn [1962], Holton [1969], Feyerabend [1970b] and [1972]. I should also add Lakatos [1963–4], [1968b], [1970], [1971a], [1973a], and [1973b].

¹⁰⁷Kuhn [1957]. Such historiographical criticism can easily drive some rationalists into an irrational defense of their favourite falsified rationality theory. Kuhn's historiographical criticism of the simplicity theory of the Copernican revolution shocked the conventionalist historian Richard Hall so much that he published

crustean bed of their hypocritical morality, thus creating fancy histories, which hinge on mythical "inductive bases", "valid inductive generalizations", "crucial experiments", "great revolutionary simplifications" etc. But critics of falsificationism and conventionalism drew very different conclusions from the falsification of these methodologies than Duhem, Popper and Agassi did from their own falsification of inductivism. Polanyi (and, seemingly, Holton) concluded that while proper, rational scientific appraisal can be made in *particular* cases, there can be no *general* theory of scientific rationality.¹⁰⁸ All methodologies, all rational reconstructions can be historiographically falsified: science *is* rational, but its rationality cannot be subsumed under the general laws of any methodology.¹⁰⁹ Feyerabend, on the other hand, concluded that not only can there be no general theory of scientific rationality but also that there is no such thing as scientific rationality.¹¹⁰ Thus Polanyi swung towards conservative authoritarianism, while Feyerabend swung towards sceptical anarchism. Kuhn came up with a highly original vision of irrationally changing rational authority.¹¹¹

a polemic article in which he singled out and re-asserted those aspects of Copernican theory which Kuhn himself had mentioned as possibly having a claim to higher simplicity, and ignored the rest of Kuhn's—valid—argument (Hall [1970]). No doubt, simplicity can always be defined for *any* pair of theories T_1 and T_2 in such a way that the simplicity of T_1 is greater than that of T_2 .

For further discussion of conventionalist historiography cf. my [1973c].

¹⁰⁸Thus Polanyi is a conservative rationalist concerning science, and an "irrationalist" concerning the philosophy of science. But, of course, this meta—"irrationalism" is a perfectly respectable brand of rationalism: to claim that the concept of "scientifically acceptable" cannot be further defined, but only transmitted by the channels of "personal knowledge", does not make one an outright irrationalist, only an outright conservative. Polanyi's position in the philosophy of natural science corresponds closely to Oakeshott's ultra-conservative philosophy of political science. (For references and an excellent criticism of the latter cf. Watkins [1952]. Also cf. below, pp. 236–238.)

¹⁰⁹Of course, none of the critics were aware of the exact logical character of metamethodological falsificationism as explained in this section and none of them applied it completely consistently. One of them writes: "At this stage we have not yet developed a general theory of criticism even for scientific theories, let alone for theories of rationality: therefore if we want to falsify methodological falsificationism, we have to do it before having a theory of how to do it" (Lakatos [1970], p. 114).

¹¹⁰I used the critical machinery developed in this paper against Feyerabend's epistemological anarchism in my [1973c].

¹¹¹Kuhn's vision was criticized from many quarters; cp. Shapere [1964] and [1967]), Scheffler [1967] and especially the critical comments by Popper, Watkins, Toulmin, Feyerabend and Lakatos—and Kuhn's reply—in Lakatos and Musgrave [1970]. But none of these critics applied a systematic *historiographical* criticism to his work. One should also consult Kuhn's 1970 *Postscript* to the second edition of his [1962] and its review (Musgrave [1971b]).

Although, as it transpires from this section, I have high regard for Polanyi's, Feyerabend's and Kuhn's criticisms of extant ("internalist") theories of method, I drew a conclusion completely different from theirs. I decided to look for an improved methodology which offers a better *rational* reconstruction of science.

Feyerabend and Kuhn immediately tried to "falsify" my improved methodology in turn.¹¹² I soon had to discover that, at least in the sense described in the present section, my methodology too—and any methodology whatsoever *can* be "falsified", for the simple reason that no set of human judgments is completely rational and thus no rational reconstruction can ever coincide with actual history.¹¹³

This recognition led me to propose a new *constructive* criterion by which methodologies *qua* rational reconstructions of history might be appraised.

B. *The Methodology of Historiographical Research Programmes. History—to Varying Degrees—Corroborates its Rational Reconstructions*

I should like to present my proposal in two stages. First, I shall amend slightly the falsificationist historiographical meta-criterion just discussed, and then replace it altogether with a better one.

First, the slight amendment. If a universal rule clashes with a particular "normative basic judgment", one should allow the scientific community time to ponder the clash: they may give up their particular judgment and submit to the general rule. "Second-order"—historiographical—falsifications must not be rushed any more than "first order"—scientific—ones.¹¹⁴

Secondly, since we have abandoned naive falsificationism in *method*,

¹¹²Cf. Feyerabend [1970a], [1970b] and [1974]; and Kuhn [1970].

¹¹³For instance, one may refer to the actual immediate impact of at least *some* "great" negative crucial experiments, like that of the falsification of the parity principle. Or one may quote the high respect for at least *some* long, pedestrian, trial-and-error procedures which occasionally precede the announcement of a major research programme, which in the light of my methodology is, at best, "immature science". (Cf. my [1970], p. 175; also cf. L. P. Williams's reference to the history of spectroscopy between 1870 and 1900 in his [1970].) Thus the judgment of the scientific élite, on occasions, goes also against *my* universal rules too.

¹¹⁴There is a certain analogy between this pattern and the occasional appeal procedure of the theoretical scientist against the verdict of the experimental jury; cf. my [1970], pp. 127–31.

why should we stick to it in *metamethod*? We can easily replace it with a methodology of scientific research programmes of second order, or if you wish, a methodology of historiographical research programmes.

While maintaining that a theory of rationality has to try to organize basic value judgments in universal, coherent frameworks, we do not have to reject such a framework immediately merely because of some anomalies or other inconsistencies. We should, of course, insist that a good rationality theory must anticipate further basic value judgments unexpected in the light of its predecessors or that it must even lead to the revision of previously held basic value-judgments.¹¹⁵ We then reject a rationality theory only for a better one, for one which, in this "quasi-empirical" sense, represents a *progressive* shift in the sequence of research programmes of rational reconstructions. Thus this new—more lenient—meta-criterion enables us to compare rival logics of discovery and discern growth in "meta-scientific"—methodological—knowledge.

For instance, Popper's theory of scientific rationality need not be rejected simply because it is "falsified" by some actual "basic judgments" of leading scientists. Moreover, on our new criterion Popper's demarcation criterion clearly represents progress over its justificationist predecessors and in particular, over inductivism. For, contrary to these predecessors, it rehabilitated the scientific status of falsified theories like phlogiston theory, thus reversing a value judgment which had expelled the latter from the history of science proper into the history of irrational beliefs.¹¹⁶

Also, it successfully rehabilitated the Bohr-Kramers-Slater theory.¹¹⁷ In the light of most justificationist theories of rationality the history of science is, at its best, a history of *prescientific* preludes to some *future* history of science.¹¹⁸ Popper's methodology enabled the historian to inter-

¹¹⁵This latter criterion is analogous to the exceptional "depth" of a theory which clashes with some basic statements available at the time and, at the end, emerges from the clash victoriously. (Cf. Popper [1957a].) Popper's example was the inconsistency between Kepler's laws and the Newtonian theory which set out to explain them.

¹¹⁶Conventionalism, of course, had performed this historic role to a great extent before Popper's version of falsificationism.

¹¹⁷Van der Waerden had thought that the Bohr-Kramers-Slater theory was bad: Popper's theory showed it to be good. Cf. van der Waerden [1967], p. 13 and Popper [1963a], pp. 242ff.; for a critical discussion cf. my [1970], p. 168, footnote 4 and p. 169, footnote 1.

¹¹⁸The attitude of some modern logicians to the history of mathematics is a typical example; cf. my [1963–4], p. 3.

pret more of the *actual* basic value judgments in the history of science as rational: in *this* normative-historiographical sense Popper's theory constituted progress. In the light of better rational reconstructions of science one can always reconstruct more of actual great science as rational.¹¹⁹

I hope that my modification of Popper's logic of discovery will be seen, in turn—on the criterion I specified—as yet a further step forward. For it seems to offer a coherent account of *more* old, isolated basic value judgments; moreover, it has led to new and, at least for the justificationist or naive falsificationist, surprising basic value judgments. For instance, according to Popper's theory, it was irrational to retain and further elaborate Newton's gravitational theory after the discovery of Mercury's anomalous perihelion; or again, it was irrational to develop Bohr's old quantum theory based on inconsistent foundations. From my point of view these were perfectly rational developments: some rearguard actions in the defense of defeated programmes—even after the so-called “crucial experiments”—are perfectly rational. Thus my methodology leads to the reversal of those historiographical judgments which deleted these rearguard actions both from inductivist and from falsificationist party histories.¹²⁰

Indeed, this methodology confidently predicts that where the falsificationist sees the instant defeat of a theory through a simple battle with some fact, the historian will detect a complicated war of attrition, starting long before, and ending after, the alleged “crucial experiment”; and where the falsificationist sees consistent and unrefuted theories, it predicts the existence of hordes of known anomalies in research programmes progressing on possibly inconsistent foundations.¹²¹ Where the conventionalist sees the clue to the victory of a theory over its predecessor in the former's intuitive simplicity, this methodology predicts that it will be found that victory was due to empirical degeneration in the old and empirical progress in the new programme.¹²² Where Kuhn

¹¹⁹This formulation was suggested to me by my friend Michael Sukale.

¹²⁰Cf. my [1970], section (3c).

¹²¹Cf. my [1970], pp. 138–73.

¹²²Duhem himself gives only one explicit example: the victory of wave optics over Newtonian optics (Duhem [1905], ch. VI, §10; also see ch. IV, §4). But where Duhem relies on intuitive “common sense”, I rely on an analysis of rival problem-shifts (cf. my [1974]).

and Feyerabend see irrational change, I predict that the historian will be able to show that there has been rational change. The methodology of research programmes thus predicts (or, if you wish, “postdicts”) novel historical facts, unexpected in the light of extant (internal and external) historiographies and these predictions will, I hope, be corroborated by historical research. If they are, then the methodology of scientific research programmes will itself constitute a progressive problemshift.

*Thus progress in the theory of scientific rationality is marked by discoveries of novel historical facts, by the reconstruction of a growing bulk of value-impregnated history as rational.*¹²³ In other words, the theory of scientific rationality progresses if it constitutes a “progressive” historiographical research programme. I need not say that no such historiographical research programme can or should explain *all* history of science as rational: even the greatest scientists make false steps and fail in their judgment. Because of this *rational reconstructions remain for ever submerged in an ocean of anomalies. These anomalies will eventually have to be explained either by some better rational reconstruction or by some “external” empirical theory.*

This approach does not advocate a cavalier attitude to the “basic normative judgments” of the scientist. “Anomalies” may be rightly ignored by the internalist *qua* internal and relegated to external history only as long as the internalist historiographical research programme is *progressing*; or if a supplementary empirical externalist historiographical programme absorbs them *progressively*. But if in the light of a rational reconstruction the history of science is seen as increasingly irrational *without* a progressive externalist explanation (such as an explanation of the degeneration of science in terms of political or religious terror, or of an antiscientific ideological climate, or of the rise of a new parasitic class of pseudoscientists with vested interests in rapid “university expansion”), then historiographical innovation, proliferation of historiograph-

¹²³One may introduce the notion of “degree of correctness” into the meta-theory of methodologies, which would be analogous to Popper's empirical content. Popper's empirical “basic statements” would have to be replaced by quasi-empirical “normative basic statements” (like the statement that “Planck's radiation formula is arbitrary”).

Let me point out here that the methodology of research programmes may be applied not only to norm-impregnated historical knowledge but to any normative knowledge, including even ethics and aesthetics. This would then supersede the naive falsificationist “quasi-empirical” approach as outlined above, footnote 81.

ical theories, is vital. Just as scientific progress is possible even if one never gets rid of scientific anomalies, progress in rational historiography is also possible even if one never gets rid of historiographical anomalies. The rationalist historian need not be disturbed by the fact that actual history is more than, and, on occasions, even different from, internal history, and that he may have to relegate the explanation of such anomalies to external history. But this unfalsifiability of internal history does not render it immune to constructive, but only to negative, criticism—just as the unfalsifiability of a scientific research programme does not render it immune to constructive, but only to negative, criticism.

Of course, one can criticize internal history only by making the historian's (usually latent) methodology explicit, showing how it functions as a historiographical research programme. Historiographical criticism frequently succeeds in destroying much of fashionable externalism. An "impressive", "sweeping", "far-reaching" external explanation is usually the hallmark of a weak methodological substructure; and, in turn, the hallmark of a relatively weak internal history (in terms of which most actual history is either inexplicable or anomalous) is that it leaves too much to be explained by external history. When a better rationality theory is produced, internal history may expand and reclaim ground from external history. The competition, however, is not as open in such cases as when two rival scientific research programmes compete. Externalist historiographical programmes which supplement internal histories based on naive methodologies (whether aware or unaware of the fact) are likely either to degenerate quickly or never even to get off the ground, for the simple reason that they set out to offer psychological or sociological "explanations" of methodologically induced fantasies rather than of (more rationally interpreted) historical facts. Once an externalist account uses, whether consciously or not, a naive methodology (which can so easily creep into its "descriptive" language), it turns into a fairy tale which, for all its apparent scholarly sophistication, will collapse under historiographical scrutiny.

Agassi already indicated how the poverty of inductivist history opened the door to the wild speculations of vulgar-Marxists.¹²⁴ His falsificationist

¹²⁴Cf. above, p. 199, text to footnote 9. (The term "wild speculation" is, of course, a term inherited from inductivist methodology. It should now be reinterpreted as "degenerating programme".)

historiography, in turn, flings the door wide open to those trendy "sociologists of knowledge" who try to explain the further (possibly successful) development of a theory "falsified" by a "crucial experiment" as the manifestation of the irrational, wicked, reactionary resistance by established authority to enlightened revolutionary innovation.¹²⁵ But in the light of the methodology of scientific research programmes such rear-guard skirmishes are perfectly explicable *internally*: where some externalists see power struggle, sordid personal controversy, the rationalist historian will frequently find rational discussion.¹²⁶

An interesting example of how a poor theory of rationality may impoverish history is the treatment of degenerating problemshifts by historiographical positivists.¹²⁷ Let us imagine for instance that in spite of the objectively progressing astronomical research programmes, the astronomers are suddenly all gripped by a feeling of Kuhnian "crisis"; and then they are all converted, by an irresistible Gestalt-switch, to astrology. I would regard this catastrophe as a horrifying *problem*, to be accounted for by some empirical externalist explanation. But not a Kuhnian. All he sees is a "crisis" followed by a mass conversion effect in the scientific

¹²⁵The fact that even degenerating externalist theories have been able to achieve some respectability was to a considerable extent due to the weakness of their previous internalist rivals. Utopian Victorian morality either creates false, hypocritical accounts of bourgeois decency, or adds fuel to the view that mankind is totally depraved; utopian scientific standards either create false, hypocritical accounts of scientific perfection, or add fuel to the view that scientific theories are no more than mere beliefs bolstered by some vested interests. This explains the "revolutionary" aura which surrounds some of the absurd ideas of contemporary sociology of knowledge: some of its practitioners claim to have unmasked the bogus rationality of science, while, at best, they exploit the weakness of outdated theories of scientific rationality.

¹²⁶For examples cf. Cantor [1971] and the Forman-Ewald debate (Forman [1969] and Ewald [1969]).

¹²⁷I call "*historiographical positivism*" the position that history can be written as a completely *external* history. For historiographical positivists history is a purely empirical discipline. They deny the existence of objective standards as opposed to mere beliefs about standards. (Of course, they too hold beliefs about standards which determine the choice and formulation of their historical problems.) This position is typically Hegelian. It is a special case of *normative positivism*, of the theory that sets up might as the criterion of right. (For a criticism of Hegel's ethical positivism cf. Popper [1945], vol. I, pp. 71–2, vol. II, pp. 305–6 and Popper [1961]. Reactionary Hegelian obscurantism pushed values back completely into the world of facts; thus reversing their separation by Kantian philosophical enlightenment.

community: an ordinary revolution. Nothing is left as problematic and unexplained.¹²⁸ The Kuhnian psychological epiphenomena of "crisis" and "conversion" can accompany either objectively progressive or objectively degenerating changes, either revolutions or counterrevolutions. But this fact falls outside Kuhn's framework. Such historiographical anomalies cannot be formulated, let alone be progressively absorbed, by his historiographical research programme, in which there is no way of distinguishing between, say, a "crisis" and "degenerating problemshift". But such anomalies might even be predicted by an externalist historiographical theory based on the methodology of scientific research programmes that would specify social conditions under which degenerating research programmes may achieve socio-psychological victory.

C. Against Aprioristic and Antitheoretical Approaches to Methodology

Finally, let us contrast the theory of rationality here discussed with the strictly aprioristic (or, more precisely, "Euclidean") and with the antitheoretical approaches.¹²⁹

"Euclidean" methodologies lay down *a priori general* rules for scientific appraisal. This approach is most powerfully represented today by Popper. In Popper's view there must be the constitutional authority of an *immutable* statute law (laid down in his demarcation criterion) to distinguish between good and bad science.

Some eminent philosophers, however, ridicule the idea of statute law, the possibility of any valid demarcation. According to Oakeshott and Polanyi there must be—and can be—no statute law at all: only case

¹²⁸Kuhn seems to be in two minds about objective scientific progress. I have no doubt that, being a devoted scholar and scientist, he *personally* detests relativism. But his *theory* can either be interpreted as denying scientific progress and recognizing only scientific change; or as recognizing scientific progress but as "progress" marked solely by the march of actual history. Indeed, on his criterion, he would have to describe the catastrophe mentioned in the text as a proper "revolution". I am afraid this might be one clue to the unintended popularity of his theory among the New Left busily preparing the 1984 "revolution".

¹²⁹The technical term "Euclidean" (or rather "quasi-Euclidean") means that one starts with universal, high level propositions ("axioms") rather than singular ones. I suggested in my [1967] and [1962] that the "quasi-Euclidean" versus "quasi-empirical" distinction is more useful than the "*a priori*" versus "*a posteriori*" distinction.

Some of the "apriorists" are, of course, empiricists. But empiricists may well be apriorists (or, rather, "Euclidean") on the meta-level here discussed.

law. They may also argue that even if one mistakenly allowed for statute law, statute law too would need authoritative interpreters. I think that Oakeshott's and Polanyi's position has a great deal of truth in it. After all, one must admit (*pace* Popper) that until now all the "laws" proposed by the apriorist philosophers of science have turned out to be wrong in the light of the verdicts of the best scientists. Up to the present day it has been the scientific standards, as applied "instinctively" by the scientific *élite* in *particular* cases, which have constituted the main—although not the exclusive—yardstick of the philosopher's *universal* laws. But if so, methodological progress, at least as far as the most advanced sciences are concerned, still lags behind common scientific wisdom. Is it not then *hubris* to demand that if, say, Newtonian or Einsteinian science turns out to have violated Bacon's, Carnap's or Popper's *a priori* rules of the game, the business of science should be started anew?

I think it is. And, indeed, the methodology of historiographical research programmes implies a pluralistic system of authority, partly because the wisdom of the scientific jury and its case law has not been, and cannot be, fully articulated by the philosopher's statute law, and partly because the philosopher's statute law may occasionally be right when the scientists's judgment fails. I disagree, therefore, both with those philosophers of science who have taken it for granted that general scientific standards are immutable and reason can recognize them *a priori*,¹³⁰ and with those who have thought that the light of reason illuminates only particular cases. The methodology of historiographical research programmes specifies ways both for the philosopher of science to learn from the historian of science and *vice versa*.

But this two-way traffic need not always be balanced. The statute law approach should become much more important when a tradition degenerates¹³¹ or a new bad tradition is founded.¹³² In such cases statute law may thwart the authority of the corrupted case law, and slow down

¹³⁰Some might claim that Popper does *not* fall into this category. After all, Popper defined "science" in such a way that it should include the refuted Newtonian theory and exclude unrefuted astrology, Marxism and Freudianism.

¹³¹This seems to be the case in modern particle physics; or according to some philosophers and physicists even in the Copenhagen school of quantum physics.

¹³²This is the case with some of the main schools of modern sociology, psychology and social psychology.

or even reverse the process of degeneration.¹³³ When a scientific school degenerates into pseudo-science, it may be worthwhile to force a methodological debate in the hope that working scientists will learn more from it than philosophers (just as when ordinary language degenerates into, say, journalese, it may be worthwhile to invoke the rules of grammar).¹³⁴

D. Conclusion

In this paper I have proposed a "historical" method for the evaluation of rival methodologies. The arguments were primarily addressed to the philosopher of science and aimed at showing how he can—and should—learn from the history of science. But the same arguments also imply that the historian of science must, in turn, pay serious attention to the philosophy of science and decide upon which methodology he will base his internal history. I hope to have offered some strong arguments for the following theses. First, each methodology of science determines a characteristic (and sharp) demarcation between (primary) internal history and (secondary) external history and, secondly, both historians and philosophers of science must make the best of the critical interplay between internal and external factors.

Let me finally remind the reader of my favorite—and by now well-worn—joke that history of science is frequently a caricature of its rational reconstructions; that rational reconstructions are frequently caricatures of actual history; and that some histories of science are caricatures both of actual history and of its rational reconstructions.¹³⁵ This paper, I think, enables me to add: *Quod erat demonstrandum*.

¹³³This, of course, explains why a good methodology—"distilled" from the mature sciences—may play an important role for immature and, indeed, dubious disciplines. While Polanyiite academic autonomy should be defended for departments of theoretical physics, it must not be tolerated, say, in institutes for computerized social astrology, science planning or social imagistics. (For an authoritative study of the latter cf. Priestley [1968].)

¹³⁴Of course, a critical discussion of scientific standards, possibly leading even to their improvement, is impossible without articulating them in general terms; just as if one wants to challenge a language, one has to articulate its grammar. Neither the conservative Polanyi nor the conservative Oakeshott seem to have grasped (or to have been inclined to grasp) the *critical* function of language—Popper has. (Cf. especially Popper [1963a], p. 135.)

¹³⁵Cf. e.g. my [1962], p. 157, or my [1968a], p. 387, footnote 1.

REFERENCES

- Agassi J. [1963]: *Towards an Historiography of Science*.
 Agassi J. [1964]: 'Scientific Problems and their Roots in Metaphysics', in *The Critical Approach to Science and Philosophy*, (ed. by M. Bunge), pp. 189–211.
 Agassi J. [1966]: 'Sensationalism', *Mind*, 75, pp. 1–24.
 Agassi J. [1969]: 'Popper on Learning from Experience', in *Studies in the Philosophy of Science*, (ed. by N. Rescher), pp. 162–171.
 Bernal J. D. [1954]: *Science in History*, 1st Edition, 1954.
 Bernal J. D. [1965]: *Science in History*, 3rd Edition, 1965.
 Beveridge W. [1937]: 'The Place of the Social Sciences in Human Knowledge', *Politica*, 2, pp. 459–79.
 Cantor G. N. [1971]: 'Henry Brougham and the Scottish Methodological Tradition', *Studies in History and Philosophy of Science*, 2, pp. 69–89.
 Cohen I. B. [1960]: *The Birth of a New Physics*.
 Compton A. H. [1919]: 'The Size and Shape of the Electron', *Physical Review*, 14, pp. 20–43.
 Duhem P. [1905]: *La Théorie Physique, Son Objet et Sa Structure* (English transl. of 2nd (1914) edition: *The Aim and Structure of Physical Theory*, 1954).
 Elkana Y. [1971]: 'The Conservation of Energy: a Case of Simultaneous Discovery?', *Archives Internationales d'Histoire des Sciences*, 24, pp. 31–60.
 Ewald P. [1969]: 'The Myth of Myths', *Archive for the History of Exact Science*, 6, pp. 72–81.
 Feyerabend P. K. [1964]: 'Realism and Instrumentalism: Comments on the Logic of Factual Support', in *The Critical Approach to Science and Philosophy*, (ed. by M. Bunge), pp. 280–308.
 Feyerabend P. K. [1965]: 'Reply to Criticism', in *Boston Studies in the Philosophy of Science*, 2, (ed. by R. S. Cohen and M. Wartofsky), pp. 223–61.
 Feyerabend P. K. [1969]: 'A Note on Two "Problems" of Induction', *British Journal for the Philosophy of Science*, 19, pp. 251–53.
 Feyerabend P. K. [1970a]: 'Consolations for the Specialist', in *Criticism and the Growth of Knowledge*, (ed. by I. Lakatos and A. Musgrave), pp. 197–230.
 Feyerabend P. K. [1970b]: 'Against Method', in *Minnesota Studies for the Philosophy of Science*, 4.
 Feyerabend P. K. [1972]: 'Against Method', (expanded version of Feyerabend [1970b]).
 Forman P. [1969]: 'The Discovery of the Diffraction of X-Rays by Crystals: A Critique of the Critique of the Myths', *Archive for History of Exact Sciences*, 6, pp. 38–71.
 Hall R. J. [1970]: 'Kuhn and the Copernican Revolution', *British Journal for the Philosophy of Science*, 21, pp. 196–97.
 Hempel C. G. [1937]: Review of Popper's [1934], *Deutsche Literaturzeitung*, 1937, pp. 309–14.
 Holton G. [1969]: 'Einstein, Michelson, and the 'Crucial' Experiment', *Isis*, 60, pp. 133–97.
 Kuhn T. S. [1957]: *The Copernican Revolution*.
 Kuhn T. S. [1962]: *The Structure of Scientific Revolutions*.
 Kuhn T. S. [1968]: 'Science: The History of Science', in *International Encyclopedia of the Social Sciences*, (ed. by D. L. Sills), Vol. 14, pp. 74–83.

- Kuhn T. S. [1970]: 'Reflections on my Critics', in *Criticism and the Growth of Knowledge*, (ed. by I. Lakatos and A. Musgrave), pp. 237–78.
- Lakatos I. [1962]: 'Infinite Regress and the Foundations of Mathematics', *Aristotelian Society Supplementary Volume*, 36, pp. 155–84.
- Lakatos I. [1963–4]: 'Proofs and Refutations', *British Journal for the Philosophy of Science*, 14, pp. 1–25, 120–39, 221–43, 296–342.
- Lakatos I. [1966]: 'Popkin on Skepticism', in *Logic, Physics and History*, (ed. by W. Yourgrau and A. D. Breck), pp. 220–3.
- Lakatos I. [1967]: 'A Renaissance of Empiricism in the Recent Philosophy of Mathematics', in *Problems in the Philosophy of Mathematics*, (ed. by I. Lakatos), pp. 199–202.
- Lakatos I. [1968a]: 'Changes in the Problem of Inductive Logic', *The Problem of Inductive Logic*, (ed. by I. Lakatos), pp. 315–417.
- Lakatos I. [1968b]: 'Criticism and the Methodology of Scientific Research Programmes', *Proceedings of the Aristotelian Society*, 69, pp. 149–86.
- Lakatos I. [1970]: 'Falsification and the Methodology of Scientific Research Programmes', in *Criticism and the Growth of Knowledge*, (ed. by I. Lakatos and A. Musgrave), pp. 91–195.
- Lakatos I. [1971a]: 'Popper on Demarcation and Induction' in *The Philosophy of Karl R. Popper* (ed. by P. A. Schilpp), forthcoming. (Available in German in *Neue Aspekte der Wissenschaftstheorie*, (ed. by H. Lenk). 1971)
- Lakatos I. [1973a]: 'The Role of Crucial Experiments in Science', in *Issues in Contemporary Physics and Philosophy of Science*, (ed. by J. Kockelmanns, G. Fleming and S. S. Goldman), forthcoming.
- Lakatos I. [1973b]: 'Anomalies versus "Crucial Experiments"', (A rejoinder to Professor Grünbaum)', in *Issues in Contemporary Physics and Philosophy of Science*, (ed. by J. Kockelmanns, G. Fleming and S. S. Goldman), forthcoming.
- Lakatos I. [1973c]: 'Was there a Copernican Revolution?', forthcoming.
- Lakatos I. [1974]: *The Methodology of Scientific Research Programmes*, forthcoming.
- Lakatos I. and Musgrave A. [1970]: *Criticism and the Growth of Knowledge*.
- McMullin E. [1970]: 'The History and Philosophy of Science: a Taxonomy', *Minnesota Studies in the Philosophy of Science*, 5, pp. 12–67.
- Merton R. [1957]: 'Priorities in Scientific Discovery', *American Sociological Review*, 22, pp. 635–59.
- Merton R. [1963]: 'Resistance to the Systematic Study of Multiple Discoveries in Science', *European Journal of Sociology*, 4, pp. 237–82.
- Merton R. [1969]: 'Behaviour Patterns of Scientists', *American Scholar*, 38, pp. 197–225.
- Musgrave A. [1969]: *Impersonal Knowledge: A Criticism of Subjectivism*, Ph.D. thesis, University of London, 1969.
- Musgrave A. [1971a]: 'The Objectivism of Popper's Epistemology', in *The Philosophy of Karl R. Popper*, (ed. by P. A. Schilpp), forthcoming.
- Musgrave A. [1971b]: 'Kuhn's Second Thoughts', *British Journal for the Philosophy of Science*, 22, pp. 287–297.
- Polanyi M. [1951]: *The Logic of Liberty*.
- Polanyi M. [1958]: *Personal Knowledge, Towards a Post-Critical Philosophy*.
- Popper K. R. [1934]: *Logik der Forschung*, (imprint 1935).
- Popper K. R. [1940]: 'What is Dialectic?', *Mind*, 49, pp. 403–26; reprinted in Popper [1963], pp. 312–35.
- Popper K. R. [1945]: *The Open Society and Its Enemies*, Vol. I-II.
- Popper K. R. [1948]: 'Naturgesetze und Theoretische Systeme', in *Gesetz und Wirklichkeit*, (ed. by S. Moser), pp. 65–84.

- Popper K. R. [1957]: 'Three Views Concerning Human Knowledge', in *Contemporary British Philosophy*, (ed. by H. D. Lewis), pp. 355–88; reprinted in Popper [1963], pp. 97–119.
- Popper K. R. [1957a]: 'The Aim of Science', *Ratio*, 1, pp. 24–35.
- Popper K. R. [1957b]: *The Poverty of Historicism*.
- Popper K. R. [1959]: *The Logic of Scientific Discovery*.
- Popper K. R. [1960]: 'Philosophy and Physics', *Atti del XII Congresso Internazionale di Filosofia*, 2, pp. 363–74.
- Popper K. R. [1961]: 'Facts, Standards, and Truth: A Further Criticism of Relativism', *Addendum* to the Fourth Edition of Popper [1945].
- Popper K. R. [1963a]: *Conjectures and Refutations*.
- Popper K. R. [1963b]: 'Science: Problems, Aims, Responsibilities', *Federation Proceedings*, 22, pp. 961–72.
- Popper K. R. [1968a]: 'Epistemology without a Knowing Subject', in *Proceedings of the Third International Congress for Logic, Methodology and Philosophy of Science*, (ed. by B. Van Rootselaar and J. Staal), 1968, Amsterdam, pp. 333–73.
- Popper K. R. [1968b]: 'On the Theory of the Objective Mind', in *Proceedings of the XIV International Congress of Philosophy*, Vol. 1, pp. 25–53.
- Price D. J. [1959]: 'Contra Copernicus: A Critical Re-estimation of the Mathematical Planetary Theory of Ptolemy, Copernicus and Kepler', in *Critical Problems in the History of Science*, (ed. by M. Clagett), pp. 197–218.
- Priestley J. B. [1968]: *The Image Men*.
- Scheffler I. [1967]: *Science and Subjectivity*.
- Shapere D. [1964]: 'The Structure of Scientific Revolutions', *Philosophical Review*, 73, pp. 383–84.
- Shapere D. [1967]: 'Meaning and Scientific Change', in *Mind and Cosmos*, (ed. by R. G. Colodny), pp. 41–85.
- Van der Waerden B. [1967]: *Sources of Quantum Mechanics*.
- Watkins J. W. N. [1952]: 'Political Tradition and Political Theory: an Examination of Professor Oakeshott's Political Philosophy', *Philosophical Quarterly*, 2, pp. 323–37.
- Watkins J. W. N. [1958]: 'Confirmable and Influential Metaphysics', *Mind*, 67, pp. 344–65.
- Watkins J. W. N. [1963]: 'Negative Utilitarianism', *Aristotelian Society Supplementary Vol. XXVII*, pp. 95–114.
- Watkins J. W. N. [1967]: 'Decision and Belief', in *Decision Making*, (ed. by R. Hughes), pp. 9–26.
- Watkins J. W. N. [1970]: 'Against Normal Science', in *Criticism and the Growth of Knowledge*, (ed. by I. Lakatos and A. Musgrave), pp. 25–38.
- Williams L. P. [1970]: 'Normal Science and its Dangers', in *Criticism and the Growth of Knowledge*, (ed. by I. Lakatos and A. Musgrave), pp. 49–50.

BOLTZMANN'S SCIENTIFIC RESEARCH PROGRAM
AND ITS ALTERNATIVES

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I. THE METHODOLOGY OF SCIENTIFIC RESEARCH PROGRAMMES

Like so often before, man has reached a stage in the endless process of progress where the human problem-situations do not fit any of the existing disciplines and thus man has no place to look for solutions. At these stages a new division of the spectrum of knowledge into newly conceived disciplines is indispensable. In order to do that, a reevaluation of the role of knowledge¹ must be undertaken; a stand has to be taken on the question of how knowledge grows and on the problem of the interaction between social and cognitive structures.² The latest addition to this discussion is the volume *Criticism and Growth of Knowledge*.³ This volume constitutes a most important problem-shift in the research programme of the philosophy of science, at least in the eyes of the historically-minded philosophers.

Those few recent critics of the book who have already expressed themselves in print⁴ do not seem to realize this problem-shift and spend their critical energies in looking for coherence among the different papers and would like to see this battle fought out. However, the problem which they would like to see solved is still the old hat "whether philosophy of science is normative or descriptive" and whether one should talk about revolutions, evolution or a continuing revolution. But the problem has shifted to the much more fundamental and interesting question of what

¹Yehuda Elkana, "The Problem of Knowledge," *Studium Generale*.

²Yehuda Elkana and D. V. Segré, "Philosophical Queries on the Presuppositions Underlying Technical-aid and Science-Teaching in Developing Countries," (preprint available from the VLJF, Jerusalem).

³I. Lakatos and A. Musgrave, eds., *Criticism and the Growth of Knowledge*, Cambridge University Press, 1970.

⁴J. Agassi, book review of *Criticism and the Growth of Knowledge* in *Inquiry*, 19 (1971), pp. 152-164;

S. Toulmin, book review of *Criticism and the Growth of Knowledge* in *Encounter*, January 1971, pp. 53-64.

is rational reconstruction and whether there is, and if so what is, the demarcation between external and internal history of science.

That philosophy of science in one of its methodological forms is a natural guide for history of science is nowadays not seriously questioned. It seems to me that Lakatos' paper in that volume⁵ and his paper in this volume⁶ comes to grips with the problem in its new formulation and constitutes a most interesting challenge for the historian of science. Therefore, I shall first criticize the methodology of Lakatos, suggest a modified version of it, show briefly how the modified version can deal fruitfully with the historical examples collected by Lakatos himself as examples for his methodology (thus turning them into counterexamples), and finally paint in broad lines a historical case study about the competing research programmes in Boltzmann's time and the resulting problem-shifts in Boltzmann's own scientific research programme.

The gist of Lakatos' argument is the following. There are four major theories of rationality of scientific progress—each provides a theoretical framework for the rational reconstruction of the history of science. Each has an internal history and each is a normative reconstruction, but each must be supplemented by empirical external theories to explain residual non-rational factors. For Lakatos "rational reconstruction or internal history is primary, external history only secondary, since the most important problems of external history are defined by internal history."⁷

The four are:

- A. Inductivism—its internal history is alleged discoveries of hard facts and inductive generalizations.
- B. Conventionalism—its internal history is factual discoveries and the erection of pigeon-hole systems (theoretical networks).
- C. Falsificationism—its internal history depicts bold conjectures, improvements, and great negative crucial experiments.
- D. Methodology of Scientific Research Programmes (MSRP)—its internal history depicts the possibly never-ending rivalry (the-

⁵I. Lakatos, "The Methodology of Scientific Research Programmes" in *op. cit.* above (note 3).

⁶I. Lakatos, "History of Science and its Rational Reconstructions," in Elkana, ed., *The Interaction Between Science and Philosophy*, Van Leer Jerusalem Series, Humanities Press, New York, 1974, pp. 195–241.

⁷*Ibid.*, p. 215.

oretical and empirical) between major scientific research programmes, with progressive and degenerating problem-shifts.

External history for Lakatos provides non-rational explanation of the speed, locality, selectiveness, etc. of historic events (as interpreted by internal history) or, when history differs from its rational reconstruction it provides an empirical explanation of why it differs. Moreover, "internal history is not just a selection of methodologically interpreted facts, it may be on occasion their radically improved version"⁸ (that is, it is not only that the success or the failure of the programme is judged by hindsight—even its content is so considered). Not only that hindsight is not eliminated, it is turned into a virtue.

I fully agree with Lakatos that all four methodologies are normative, and that there is gradual improvement; the MSRP is by far the most fruitful. But the reason for calling it the best is rather that in this methodology reconstruction can be and is nearest to the historically true story (true in the primitive sense in which it is clear that Bohr did not discover the spin, Newton had no energy concept etc.). Indeed, I shall try to show that the demand to present rational reconstruction as against historical narrative is now *unnecessary* and that the internal-external dichotomy constitutes a degenerating problem-shift.

Even if we leave the explanation of speed, locality, etc., of historical events to non-rational explanation, there is much *more* to be explained rationally than just the growth of objective scientific knowledge. I fully agree that whether "an experiment is crucial or not, whether an hypothesis is highly probable . . . whether a problem-shift is progressive or not, is not dependent in the slightest on the scientist's beliefs, personality or authority,"⁹ but it is dependent on what, according to the scientist, is the role of science and of the scientist; on what, according to him, are the basic concepts, theorizing in terms of which is legitimate scientific thought; on what he thinks is the accepted limit of speculation in order to get advancement, grant-wise or position-wise, in the scientific community; on what he thinks are the connections between the animate and the inanimate world; on whether, according to him, it is considered rational to explain nature on basis of conservation laws, or symmetry rules or funda-

⁸*Ibid.*, p. 216.

⁹*Ibid.*, p. 215.

mental entities; on whether daring speculation or solid minute theoretical connections are thought of as a hallmark of science. These too are rational considerations.

These cognitive considerations I shall call the *image of science*: it is the sum-total of *thoughts* on what science is and should be and it has a major rational influence on the scientific programme of individuals, schools, communities. If this be psychology, it is cognitive psychology and not motivational.¹⁰ As to its source, the image of science is created among other sources by the philosophy of science-teaching in a given scientific community and this philosophy is influenced by a mixture of considerations, part of which Lakatos would call internal, others external. The rational influences are the objective state of knowledge and the accepted theory of the growth of knowledge,¹¹ (for science teaching is at least openly normative and has always been so) as well as the accepted epistemology of the time. The non-rational factors will be the web of beliefs, social and economic factors which all contribute to the creation of what is called the spirit of an age. All in all, there are good reasons to abhor rational reconstructions in Lakatos' sense (about other senses, see below), without stooping so low to become a classical inductivist.

One can thus view the growth of knowledge as the result of sharp slow-moving critical dialogues between competing research programmes, with degenerating as well as progressive problem-shifts, and realize that these critical dialogues ensue because of differences in three interacting areas:

- A. Objective, universally understandable developments in the body of knowledge.
- B. Rational differences as to the role of science; the image of science.
- C. External non-rational influences on the men who do science: this is mainly manipulation of the body of the scientist or of the institutional framework within which he works.

¹⁰To illustrate this point see my "Euler and Kant" to be published in the *Boston Studies for the Phil. Science*.

¹¹Much has been written on different types of rationality. The latest and most relevant to us is: S. N. Eisenstadt, "Innovations and Tension Between Different Types of Rationality" to be published in the *Proc. of the Olivetti Foundation's International Seminar of the Social and Technological Innovation in the Field of Information*, Courmayeur, 1971.

It is very rarely possible to disentangle all three and it does not seem to me fruitful to attempt it. (Lakatos' argument on the necessary intellectual equipment of the historian is not relevant, for the historian has to master the discipline itself in order to write intelligent history, whatever his methodological bias.)

A scientific research programme consists of a set of theories. It has a hard core, which is very often a metaphysical view of the structure of the world, and positive and negative heuristics to defend it.¹² The choice of problems is dictated by the positive heuristic and not by anomalies.¹³ High autonomy is given to theoretical science—experiments are never crucial (except when viewed by hindsight). The central problem remains to define criteria for progressive and degenerating problem-shifts: for growth or stagnation. Lakatos claims that a research programme is progressing as long as its theoretical growth anticipates its empirical growth, that is as long as it keeps predicting novel facts with success. It stagnates when it only gives post hoc explanations of "chance" discoveries.

It is in the application of these criteria that the internal-external dichotomy is at its weakest, for in order to accept its criteria, one has to assume that stages exist when pure experimental work really precedes

¹²By scientific metaphysics I mean empirically untestable statements (in principle statements about the structure of the world). However, unlike in ethical and religious metaphysics here a rational critical dialogue can be conducted between different scientific metaphysics. Such kind of dialogue applied also to religious metaphysics in scholastic times until the "disputatio" disappeared with the outburst of scepticism around the end of the sixteenth century. A. Koyré explains this in his *Introduction to Anscombe & Geach, eds., Descartes Philosophical Writings*, Nelson, 1970.

¹³In the usual demarcation between internal and external it is often claimed that the development of new ideas can be explained by supplying the necessary conditions from the inside, and trying to point to some sufficient conditions as external i.e., non-cognitive. On this approach see J. Ben-David, *The Scientist's Role in Society*, Prentice-Hall, 1971.

As against this I claim that internal necessary conditions produce more problems ready for treatment (actually an infinite number) than the number of these which are indeed taken up and chosen as central problems, "frontiers of science," or "scientific research sites" to use R. K. Merton's language.

The choice of the problems to be dealt with is made by a cognitive process of deciding which problems fit the contemporary role of knowledge and the man-of-knowledge, i.e. the image of science. I have tried to do such necessary—sufficient analyses in several case studies, the most explicit of which was the emergence of the concept of energy in Y. Elkana: "Helmholtz 'Kraft': An Illustration of Concepts in Flux," in *Historical Studies in the Physical Sciences*, 1970, pp. 263–298. See also my forthcoming book: *The Discovery of the Conservation of Energy*, Hutchinson's Educational Series, London.

theoretical work. We even have to accept the existence of chance discoveries. But if we learned anything from the failure of the other methodologies and from recent work in anti-inductivist history of science, it is that there are no chance discoveries: that if we are looking for the theoretical framework which yielded some empirical data (instead of trying to eliminate these frameworks), we always find them and then succeed in writing factual history, rationally explained. In other words, my criticism is that Lakatos in formulating his criteria conflates his new methodology with classical inductivism.

In order to clarify this point, let me remark that Lakatos himself distinguishes between chance discoveries in the "objective" and the "subjective" sense. In my opinion there is certainly no such thing as a chance discovery in the "objective" sense—each experiment is either a confirming or refuting instance of some theory. In the "subjective" sense however, it was often the case that experiments were thought to be theory-independent, in accordance with the prevailing image of science. Planck, who was a half-hearted Kirchhoffian phenomenologist until his conversion to atomism, tried to consider his own radiation law as a chance discovery and even attacked it on methodological grounds. He did not admit at first that his discovery was a rational attempt to adapt the phenomenon of blackbody-radiation to his previous scientific research programme, the hard core metaphysics of which was that all phenomena are reducible to classical generalized thermodynamics. Only many years later, after debating this point with Boltzmann, did gradually a problem-shift occur and he could consider the radiation law and even Einstein's theory as part of his new SRP, the hard core metaphysics of which was that all phenomena are reducible to a generalized atomistics (1911–12).

My point is that these criteria (for distinguishing progressive from degenerating problem-shifts) abandons the internal-external dichotomy. If our view on the growth of science is correct, that is if scientific growth is explainable by competing SRP's, then it is also true that scientists are always aware of competing research programmes, and when planning an experiment *they always wish to confirm some of their own predictions or to refute some of their rival's predictions*. It may turn out that if no ad hoc hypothesis will be put forward, the results will confirm the rival's predictions and refute their own. In any case it does not mean that *empirical growth ever anticipates theoretical growth* in the objective

sense or that we are faced with real chance discoveries which have to be explained.

What is happening then is a process of saturation where either the number of ad hoc adjustments becomes too cumbersome or the hard-core metaphysics is so adjusted that two rival theories by way of a progressive problem-shift conflate into a new programme.¹⁴ These changes can be accounted for, that is rationally explained, by the three interacting factors (A), (B) and (C) mentioned above. The greatest advantage in the MSRP is allowing scientific metaphysics to become part of the hard-core SRP's.¹⁵

Another possibility is that a discovery is made and is immediately interpreted as a confirming or refuting instance of the SRP in the framework of which the experimenter has worked. And yet, admittedly the result was not predicted. Very often this means that an ad hoc hypothesis has been invented, the actual prediction forgotten and the discovery may seem accidental. But in reality it happens very often that the hard core of the programme is not fully articulated and the experimental set-up is dictated by a vague question or prediction, often misnamed "intuition". This is the case when the experimenter says in the well-known inductivist style: "Let us see what happens if . . ." These frequent cases are no evidence for inductivism but point to the existence of half-formulated theoretical questions which had not been clarified, either because the hard core of the programme was not fully articulated or because the scientist's image of science, which might be any inductivist methodology and the moral code accompanying it, did not permit formulation of the theoretical question in full. In these cases the results of the experiment contribute to the clarification of the programme.

Let us now return to the problem of rational reconstruction and the

¹⁴ Examples for this are the so-called "simultaneous discoveries." See my "The Conservation of Energy: a case of simultaneous discovery?", *Arch. Internationales d'Histoires des Sciences*, 22 (1970), pp. 30–60.

¹⁵ So far my argument was to show that the internal-external dichotomy is misleading and that the growth of knowledge can be explained by (A) internal, (B) cognitive image of science and (C) pure external factors with the proviso that they can never be logically separated out. Here three pairs of two-way interaction take place. History is not neat. The break-down of this popular dichotomy is parallel to the break-down of the previous problem of demarcation between science and metaphysics. For Lakatos this problem does not arise for he includes metaphysics in the core of his SRP. I attempted to show in a case study "Euler-Kant" (*op. cit.* in note 10) that the dichotomy was unnecessary.

problem of hindsight. *Lakatos juxtaposes rational reconstruction to true historical narrative*. This the historian abhors. Here I would rather hold with Agassi who says in a recent article "*this is the principle of historical reconstruction—apply no hindsight.*"¹⁶ For the historian, elimination of hindsight is the symbol of good history, and the methodology of historiographic research programmes is much better without it. This will also cast further light on the above-mentioned criterion for progressive research programmes, which according to Lakatos must predict novel facts. *Every research programme may indefinitely predict novel facts*, the problem is always whether the innovations are significant or not. Deliberation as to significance, if not simply left to hindsight, must include a host of considerations, all in a hopeless tangle, and nothing can be gained by calling part of them internal and part of them external. Reconstructing deliberations is true historical rational reconstruction.¹⁷

Naturally, Lakatos realizes that scientists very often stick to what seems to be, by hindsight, a sinking ship. "One may rationally stick to a degenerating programme until it is overtaken by a rival and even after. What one must not do is to deny its poor public record . . . it is perfectly rational to play a risky game, what is irrational is to deceive oneself about the risk."¹⁸ It is evident that one sees the game as risky again only by hindsight. In a sense any metaphysical hard-core of an SRP, being metaphysical, is risky—but this does not mean that one views the programme as degenerative. In my opinion no scientist can rationally reach a conclusion that his SRP is degenerating and yet stick to it. Patching it up and inventing ad hoc hypotheses means on the contrary that to him

¹⁶J. Agassi, *op cit.* above (see note 4).

¹⁷This means reconstructing possible cognitive processes, especially the process whereby motivation (i.e. a purely external social or political or individual psychological motivation) is translated into cognition. Such examples can be seen when Leibniz finds in his synthetic dynamics the counterpart in nature to his (motivational i.e. external) aim of uniting the churches. Kant, whose main axis of interest was to unite Wolffian philosophy with German Pietism, looked for a counterpart in nature of this unification of polar-different approaches: he found in conservation laws the unity-in-diversity. That he chose the concept of force as the one entity to be conserved is easily understood in view of the fact that force is the one useful quantifiable physical magnitude. It is only so far that he was a Newtonian. The above story is a reconstruction of a possible cognitive process. Reconstructing deliberations is also a most useful practical tool in curriculum research. See the following: J. Schwab, *The Language of the Practical and The Art of the Eclectic*; J. S. Fox, *The Practical Image of the Practical*.

¹⁸Lakatos, I., in Y. Elkana, ed., *Interactions between Science and Philosophy*, Van Leer Jerusalem Series, Humanities Press, New York, 1972, p. 195.

it has not yet degenerated. In order to be able to write an historical reconstruction of his deliberations we have to eliminate our hindsight and to juxtapose the actual deliberations of the competing research programmes. Then we shall easily detect the mutual influences and see which problem-shifts helped some problems to grow and others to fade away. Let me try to reinterpret a few of the examples brought by Lakatos.

Newton's "hypotheses non fingo": Clearly this statement is no problem for the inductivist, it only proves to him that Newton was one too. It does not pose a problem for the Duhemian conventionalist either—Duhem thought Newton's methodology nonsensical. Lakatos from the point of view of his methodology says: "Koyré whose many strong points did not include logic devoted long chapters to the 'hidden depths' of Newton's muddle."¹⁹ I think that according to Koyré, inductivism was not part of Newton's methodology, but rather his image of science and it caused him severe problems. (Clearly I am formulating Koyré views in my terminology.) It interfered with his whole SRP, and some hypotheses which seemed to him too farfetched he refused to adopt. Even if we accept Koyré's translation of the famous dictum as "I do not feign hypotheses" we are not much better off. Newton's refusal to look for the cause of gravity was motivated by what he thought natural philosophy was supposed to do. It certainly was no muddle.

The Proutian example: Lakatos brings the Proutian programme with its hard core and positive heuristic (to overthrow and replace the contemporary false observational theories applied in measuring atomic weights) as an example to prove that rational reconstruction has to be more than a normative selection of the rational parts of the story. According to him it has to give it in an improved version. It seems to be extremely fruitful for the historian to study the Prout story, guided by the MSRP. One then understands what Prout and the Proutians wanted to do. Why they invested such enormous effort in revolutionizing experimental techniques of the separation of chemical elements: one can follow the different SRP's of Stas, who never accepted the Proutian programme (it is not true that he "became frustrated by some stubborn recalcitrant instances"²⁰ and only then decided in 1860 to abandon the programme) and Marignac who shared it. Neither did Maxwell accept

¹⁹Ibid., note 18.

²⁰I. Lakatos and A. Musgrave, eds., *Criticism and the Growth of Knowledge*, Cambridge University Press, 1970, p. 139.

the Proutian programme. However, what both Stas and Maxwell opposed and Crookes in 1886 advocated was an early version of the isotope idea which was introduced as the progressive problem-shift in the Proutian programme, probably by Graham in the early 1860's. Only after this conception of mixtures between elements of slightly differing atomic weight became part of the programme, could Crookes introduce a further progressive problem-shift in 1888, namely the idea that what is chemical and what is physical separation have to be redefined and methods of physical separation between elements investigated.

There is no need to claim, with Lakatos, that in a rational reconstruction Prout knew of experimental anomalies while actual history teaches us that he admitted none. The whole dichotomy is spurious. Prout claimed that *de facto* there were no anomalies, but there were seeming ones because of the bad techniques of measurement and separation. Then again, Lakatos claims that there was no rational reason to have abandoned the Proutian programme while *de facto* it was abandoned and thus "the progress of science was hindered and slowed down by justification and by naive falsificationism"²¹ in the 19th century.

It is seen in the way I have corrected the story, that first of all the programme was never completely abandoned, and secondly its progress was severely slowed down because of the image of science in the 19th century. It was not thought (justificationist methodology?) "scientific" to support a research programme at the hard-core of which all atomic weights were considered as mean values in the absence of hard confirming experimental evidence. It was not considered rational to accept an SRP because of its overall explanatory value. Thus it was eminently rational to suspend work on the Proutian programme until experimental results in accepted research programmes called it back to life.

Mendelian genetics: For Lakatos only empirical-external history can explain (no rationality theory will ever solve . . . !) why "Mendelian genetics disappeared in Soviet Russia in the 1950's, or why certain schools of research into genetic racial differences or into the economics of foreign aid came into disrepute in the Anglo-Saxon countries in the 1960's."²²

This remark presupposes a value judgment which I happen to share,

²¹Ibid., p. 140.

²²Lakatos' paper in this volume, p. 210.

but then the conclusion is wrong. If according to Lakatos there can be only an external explanation why Mendelian genetics in Russia disappeared the presupposition is that there is no rational *bona fide* SRP behind this change but only political-external pressure to interfere with the scientists and not really with the body of knowledge; in this trivial sense *external* has a meaning, but in many documental cases the story is much more complicated. Many scientists (perhaps Lysenko himself) did develop not only a lip-service philosophy, but a completely rational scientific opinion that characteristics can be changed under due environmental influences and this opinion turned into the metaphysical hard core of an ambitious SRP. In which case, even if, in our opinion, this programme is bound to fail, it is methodologically on a par with the SRP of Mendelian genetics at its very start.

If I said that hesitantly about Soviet genetics, I am on much firmer ground when speaking of Western sociologists, educators, psychologists and even politicians who adopted a fully-fledged SRP of how to deal with the culturally disadvantaged adolescent, of which the metaphysical hard core was the view that there are no racial genetic differences. The furore about the Jensen case²³ only proves that a completely rational critical dialogue is being held between the conservative SRP according to which no racial differences exist, and the revolutionary new view which in the hard core provides for such differences.

Let me take one more example on which Lakatos does not elaborate.

The economics of foreign aid: this is a beautiful case of a problem-shift which took place in the last few years as a result of a critical dialogue between competing research programmes. The two SRP's had as their respective hard cores the following two statements:

- (1) Progress is unambiguously defined as acquisition of Western culture and especially Western science and technology. Foreign aid is by definition communication of this science and technology to the countries in need of aid. The more the better. It is also presupposed that primitive cultures have no science of their own. In other words the normative core of the programme is to introduce Western science in place of no science.
- (2) Progress is a relative concept. Every culture has its own equiv-

²³Harvard Educational Review Reprint Series, No. 2, 1969.

alent of our science and there is no need to interfere with that. If anything, what is needed is support to develop their own culture or their own lines—the result will be by definition called progress.

No. (1) is the hard core of the ramified SRP accompanying the \$13-billion worth of foreign aid invested in communicating Western science and technology to developing countries in the years 1960-1970. No. (2) is a primitive version of sophisticated anthropological SPR's which at their hard core have, in my opinion, something like statement (2). On no account do I subscribe to either (1) or (2). These are only articulations of hidden SRP's. My own SRP is outlined in the preprint cited in note 2 above.

The problem-shift that occurred during the debate between the two SRP's (and was emphasized by the growing feeling of failure of No. (1)) resulted in a completely new SRP, which I subscribe to, with the hard-core metaphysics:

- (3) Progress is towards Western culture and is an absolute concept. But every culture does have an equivalent of our science and thus in order to lead developing countries towards progress we have to help them articulate their own science and then piece-meal replace this by our own science. In other words Western science replaces an existing body of knowledge and does not fall into a vacuum.

In other words: the normative hard core of this SRP is that in order to bring about progress one has to initiate a critical dialogue between competing SRP's and thus start the process which according to the historiography of the MSRP has brought about the growth of knowledge in the West.

The Bohrian example: That the rational reconstruction should give an improved version of history is illustrated according to Lakatos by the example of the discovery of spin. "The historian, describing with hindsight the Bohrian programme, should include electron spin in it, since electron spin fits naturally in the original outline of the programme. Bohr might have referred to it in 1913. Why he did not do so is an interesting problem which deserves to be indicated in a footnote. (Such problems might then be solved either internally by pointing to rational reasons

in the growth of objective, impersonal knowledge; or externally by pointing to psychological causes in the development of Bohr's personal beliefs.)"²⁴

Now, what does it mean to "fit in naturally" or to claim that Bohr "might have referred to spin"? Does not the concept of energy fit in naturally into Newton's programme, and might he not have referred to it? Doesn't the concept of field fit into Leibniz's dynamics? Or, to become more absurd—does not the idea of relativity fit in the physics of the Stoa? By considering the spin as part of the Bohrian programme, or energy in the Newtonian programme, we miss the most fascinating chapters in the history of critical dialogues between major SRP's. We even disregard a constituent of the hard core of our methodology: that a scientific metaphysics is incorporated in the hard core of an SRP, since the metaphysical statement that knowledge grows by critical dialogue between competing SRP's of this kind is in turn the hard core of our historiographical research programme. We thereby skip over the mid-19th century debates about the place of energy in the Newtonian programme; and by including spin in Bohr's programme we deliberately skip over the fundamental early debates which caused a problem-shift ending up with four quantum numbers. In addition, by paying that price we do not gain any insight into the growth of knowledge. Moreover, by introducing "improvements" into rational reconstruction, we easily cause a degenerating problem-shift in our historiographical research programmes. It invites such improvements as Tait's letter to Thomson asking to look for the concept of energy in Newton's Principia because Newton was such a great man that he surely must have invented such an important concept.²⁵

In other words, such improvements can easily lead to hero-worship in the Victorian style of vulgar Baconianism. If, on the other hand, we introduce mathematical compatibility as a criterion for "what might have been referred to" then we end up with the inductivist picture of knowledge by accretion, where Einstein's theory does not refute Newton's but only serves as an extension of it. Just the kind of approach which the MSRP wishes to eliminate.

On the other hand, if our historiographical methodology teaches us

²⁴Lakatos, paper in this volume, pp. 216-217.

²⁵I am referring to a well-known story about which see the reference to my forthcoming book. See note 13 above.

to look for competing SRP's, we shall find rational reasons why Bohr had not thought of the spin and then it is an important topic which certainly should not be relegated to a footnote. If on the other hand the reasons for this are psychological in the development of Bohr's personal beliefs only (an opinion which I do not share), then they are out of place in Lakatos' research programme even for a footnote.

In all probability, the reasons found for Bohr not having discovered the spin, will be *rational but of that new kind which I have been advocating (as the "image of science") and which is totally ignored by all internalists, and only partially treated by externalists who wish to consider all psychological factors as motivational and not cognitive*. To be specific: Bohr, in the 1920's, had an image of science according to which the main role of science was to describe the world in general atomistic terms (this included the Einsteinian view of quantized energy), but certainly not as an attempt to speculate about hidden properties of elementary particles. For this, the Boltzmannian methodology which ruled from 1900 till round 1920 had to be replaced by a revised version of phenomenology—a development influenced at least as much by the Einsteinian revolution in science (that is, rational considerations) as by the general disenchantment about the world growing out of the First World War and well symbolized by the Vienna Circle of logical positivism.

Lakatos' account of the Bohrian programme is a masterful illustration for the historian of how useful the MSRP is and it shows precisely that this methodology does *not* have to rely either on the internal vs. external dichotomy or on the rational reconstruction vs. narrative juxtaposition.

There is one last line of argument I wish to press. I am fully convinced by Lakatos' metatheoretical refutation of the inductivist, conventionalist and falsificationist methodologies. I also think that he succeeded in demonstrating the fruitfulness of the MSRP by applying it on a meta-level to the methodology of historiographical programmes—that is to itself. In this demonstration he relied only on the hard core of the programme, namely on the view that knowledge grows in a slow dialogue between competing research programmes—a dialogue which brings about repeated problem-shifts. He himself does not rely at all on the external-internal dichotomy. This I think adds the last touch to my argument proving that this dichotomy should not form part of the MSRP.

The modified version of the MSRP will give the following injunction to historians of science:

As a guide, look in history for rival SRP's, for progressive and degenerating problem-shifts. Behind any seemingly crucial experiment look for a hidden war of attrition between two or more research programmes. *Do not draw an artificial demarcation between internal and external history but try to reconstruct every SRP and the problem-shifts occurring in it as a result of three factors:*

- A) Rational scientific considerations in the body of knowledge (according to which theory of rationality?);
- B) the image of science—rational considerations of the scientists who subscribe to the SRP about the task and nature of science;
- C) socio-historical non-rational factors influencing the metaphysical hard core of the programme and the behavior of individual scientists and scientific communities.

Every scientist is “rational for himself”—for the adherent to the MSRP inductivism is irrational and has to be empirically explained.

It is on these lines that I shall review the rival research programmes in the 1860's and 1870's which contributed to the body of knowledge the kinetic theory of gases, statistical mechanics, electromagnetic theory and the Kirchhoffian formulation of mechanics; then I shall follow the problem-shifts that took place in these research programmes under the pressure of the dialogue between them during the war of attrition, and then describe the SRP which was developed by Boltzmann around the turn of the century and which proved its usefulness in the work of many of the greatest scientists in the 20th century from Planck to Schrödinger. Ironically, as it so often happens, this last Boltzmannian research programme, which he himself considered as one of the greatest achievements of theoretical physics, was so far removed from Boltzmann's research programme of the 1860's that his own contributions could not have been made in this framework.

II. A CASE STUDY: BOLTZMANN

1) *The atomistic-realistic school in Vienna in the 1860's²⁶*

²⁶In Vienna the atomistic-realistic school in physics, the musical and literary tradition of Mahler, Karl Krauss, Stefan Zweig and others, the philosophical

Boltzmann's SRP when he started his career was the development of physics on an atomistic principle. The hard core of this programme was that atoms exist, that all seemingly continuum phenomena are explainable on an atomistic basis and that atomism and mechanistic reductionism are fundamentally equivalent. He considered it his first task to give an atomistic-mechanistic interpretation to the second law of thermodynamics and this became the foundation of his work on gas theory. Boltzmann describes this programme by ascribing it to his great friend and senior colleague Loschmidt in his eulogy to the latter. At the time (1860's) "Eine der wichtigsten Fragen . . . war die nach der Zusammensetzung der Materie. Sie ist es wohl auch noch heute; nur dass man die Fragestellung etwas anders stilisiert hat. Während man damals die letzten Elemente des Seienden der Materie selbst suchte, so fragt man heute, aus welchen einfachen Elementen man die geistigen Bilder zusammensetzen muss, um die beste Übereinstimmung mit den Erscheinungen zu erzielen."²⁷ These were the times when heat was finally explained as motion of particles, and hypotheses about their motion in various states of matter were formulated.

In the scarce literature on Boltzmann there is very little account taken of the fact that his methodology and programme underwent a gradual and tortuous evolution, and thus there is almost no documentation of the different research programmes which he propounded and problem-shifts are not even mentioned. One reason for this lies in the methodology of the historiographic research programmes of the historians who have dealt with Boltzmann: Dugas, Broda, Brush.²⁸ An exception to

schools of Wittgenstein and Popper, the unique synthesis of Freud, all belong to the tradition of great syntheses which developed in Europe as against the three polar traditions: of Pierre Charron's religious solution, Bacon's inductivism and the Cartesian rationalism which is considered (in the 20th century image of science) as solely responsible for the success-story of science. The tradition of great syntheses started with Leibniz, continued with Euler, Kant and Helmholtz and spread in diverse directions—Helmholtz's epistemology and Fechner's psychophysics, Freud, the Marxian synthesis and the Weberian synthesis. This is not the place to develop this theme further, but as a starting point see my "Newtonianism in the 18th Century" (Review Essay), *British Journal Phil. Science*, August 1971.

²⁷L. Boltzmann, "Zur Erinnerung an Josef Loschmidt" (1889), in *Populäre Schriften*, Leipzig, 1905, p. 241.

²⁸R. Dugas, *La Théorie Physique au Sens de Boltzmann*, Neuchâtel, 1959; Engelbert Broda, *Ludwig Boltzmann*, Wien, 1955; Stephen Brush, "Foundations of Statistical Mechanics 1845–1915," *Arch. Hist. Exact Sciences*, 4 (1967), 145–183.

this is Martin J. Klein, who included many illuminating passages on Boltzmann in his recent book on Ehrenfest.²⁹ Another reason is that almost all of Boltzmann's philosophically and methodologically relevant articles and speeches (including the *Populäre Schriften*) were written between 1886 and 1905, that is, started at a time when Boltzmann had already abandoned his initial primitive mechanistic-atomistic metaphysics. This is easy to understand if we remember that in those decades it was not the task of scientists to deal with such abstruse matters (image of science!) and they could afford to deal with them legitimately only in "Antrittsreden," "Festreden" or memorial lectures delivered only by leading senior scientists, as a political act by senior statesmen—something which Boltzmann could not have afforded to do before 1885.

Nevertheless, it is my contention that in his scientific papers between 1865 and the late 70's, the mechanistic RP is manifest. This early period covers his most important contributions on gas theory, some papers on electricity and various mathematical papers. There is only one popular essay from this period, a paper on Maxwell's theory of electricity from 1874³⁰ where Boltzmann takes realistically and extremely seriously Maxwell's mechanical model of the ether—much more realistically than Maxwell ever took it and in spite of the fact that by this time Maxwell, at least officially, had abandoned it.

This early mechanistic RP was shared by the whole Vienna School in physics: Doppler, Loschmidt and Stefan Exner. The following generation of Viennese physicists, who were either contemporaries or students of Boltzmann like Hasenohrl and Ehrenfest, propounded a research programme which was that of a later Boltzmann.

Loschmidt's work is known better from Boltzmann's memorial lectures than from his original work, which is very little read: his influence on Boltzmann is beyond doubt. In all the problems with which Loschmidt dealt, his concrete atomistic-realistic RP is manifest:

1. In 1865 his most important paper appeared, devoted to calculating the size of individual molecules of air.³¹

²⁹M. J. Klein, *Paul Ehrenfest*, North-Holland, 1970.

³⁰L. Boltzmann, "Über Maxwell's Elektrizitäts-Theorie" (1874), *Pop. Schriften*, Leipzig, 1905, pp. 11–24.

³¹L. Boltzmann, "Zur Erinnerung an Josef Loschmidt," *Pop. Schriften*, Leipzig, 1905, p. 236.

2. Thirty years before Kelvin's calculation on the characteristics of the ether, Loschmidt wrote an article "Über die Konstanzen des Licht-Äthers,"³² developing a gas-like atomistic model for the ether and then deducing the various constants.
3. He did repeated experiments (which failed) to disprove the fatal conclusion that due to the irreversibility of most thermodynamic processes a dissipation of energy and the ensuing Wärmetod were inevitable. The considerations in these attempts are such that it is clear beyond doubt, that for Loschmidt molecules and atoms behaving according to classical mechanical laws are ultimate realities.
4. Loschmidt's criticism of Boltzmann's statistical hypothesis, as we shall see below, was again motivated by his atomistic-realistic view which he thought attacked by Boltzmann's hypothesis. Many years before Maxwell introduced his supernatural sorting device, (what Thomson calls the "Maxwell demon") Loschmidt introduced an intelligent being which could sort gas-molecules according to their velocities, thus counteracting irreversibility of processes. And here again his description exhibits the hard core of his programme.³³

One must realize that when one calculates the size of molecules, their velocity, the mean free path etc., there is no doubt about their existence in the most primitive sense.³⁴ What Boltzmann calls much later only a different "Fragestellung" is something quite different. Had Loschmidt dealt with various relations between various *mental pictures*, it would have stifled the whole problem.

When, due to Maxwell and others, the slow diffusion of different

³²Ibid., p. 245.

³³Incidentally Boltzmann's opinion on Loschmidt's intelligent being shows how deeply involved in life-processes he was and how this made him advance considerations which found their place in the body of knowledge only much later: "Ich wollte sie aber schon damals nicht gelten lassen, und wandte ein, dass wenn alle Temperaturungleichheiten aufgehört hätten, auch keine intelligente Wesen sich bilden könnten." *Ibid.* p. 240.

³⁴Boltzmann in his Loschmidt lecture, *op. cit.*, note 277, says, "Freilich unsere molekulartheoretischen Begriffe existieren nur in uns; aber die Erscheinungen, die ihnen konform sind, existieren unabhängig von uns, also außer uns, und wenn wir uns heute statt zu sagen: "Die Moleküle existieren," lieber der Phrase bedienen, "unsere betreffenden Vorstellungsbilder sind ein einfaches und zweckmäßiges Bild der beobachteten Erscheinungen," so mag die neue Ausdrucksweise gewisse Vorteile haben, im Wesen aber dachte man sich bei der alten genau dasselbe." *Ibid.*, vol. 2, p. 247.

gases into each other was calculated, and Graham failed in his experimental proof of it, it was again Loschmidt who succeeded. The reason, in my opinion, is that Graham did not think in such clear atomistic-realistic terms as did Loschmidt but was much nearer to Maxwell's methodology, which in itself was an early version of the later theory of mental pictures.³⁵

The analogous proof for the conduction of heat in a gas was given somewhat later by Josef Stefan, another Viennese of the same school, a friend of both Loschmidt and Boltzmann, whose general work and especially this proof was executed in the framework of the same SRP. More work in the framework of this programme was done by Loschmidt's successor Frank Exner, and by that curious outsider Smoluchowsky, who came to work many years later in Loschmidt's laboratory and subscribed to the same metaphysical hard core of the programme; methodologically Smoluchowsky was twenty years behind his time. In 1900 he had not yet acquired all the lofty talk about "mental pictures" and did his work as dictated by atomistic realism.

Boltzmann, in his memorial lecture on Loschmidt, emphasized repeatedly the impact of Loschmidt's work on fundamental theoretical problems. This work was "die Antwort auf die Frage nach der Kontinuität der Materie" and he says "die Loschmidtsche Zahl zeigt uns nun die Grenzen dieser Teilbarkeit. Wir erhalten Individuen."³⁶ He also saw that one of the most important results of the kinetic theory was the explanation of gas pressure as due to collision of the molecules with the walls of the container and not due to mutual forces of repulsion between molecules; he remarks: "Es ist dies das erste Beispiel, dass man eine Kraft als eine bloss scheinbare, durch dem Auge verborgene Bewegung hervorgerufene, betrachtete." In other words a realistic atomism underlying the programme of matter and motion.

2. *The two problem-shifts*

Boltzmann's early work on gas theory—that is his greatest contribution to the objective body of knowledge—are admirably described in

³⁵See Graham's paper "Speculative Ideas respecting the constitution of Matter" (1864).

³⁶Op. cit., note 27, p. 245.

Martin J. Klein's book on Ehrenfest.³⁷ Here I shall refer only to a few relevant developments. Wanting to prove the second law of thermodynamics on a molecular basis and influenced by Maxwell's statistical considerations, Boltzmann finally proved his famous H-theorem, which is actually the completion of his proof of the second law of thermodynamics on the basis of molecular theory.

As mentioned above, the first objection came from Loschmidt. Loschmidt, whose SRP centred around realistic-atomism and mechanical-reductionism, found it paradoxical that kinetic theory built on classical Newtonian mechanics and thus dealing only with completely reversible processes, could seemingly explain irreversible processes as well. This was Loschmidt's "Umkehrreinwand" (reversibility criticism) of 1876.

In his reply, Boltzmann introduced the first definite problem-shift in the hard core of his programme. He gave up his mechanistic reductionism and clung only to his atoms. He explained that though Loschmidt was correct in his considerations relating to the attempt to reduce the second law to mechanics, the truth was that the atomistic proof of the second law was not based on mechanics alone but relied on mechanics and statistical laws and said something about the actual behavior of the world.³⁸

A second serious problem-shift in Boltzmann's RP occurred in the late 1890's in the course of his long drawn-out battle against the mathematical phenomenologists and the phenomenology of the energeticists. These two critical dialogues were fundamentally philosophical-methodo-

³⁷*Op. cit.*, note 29.

³⁸It is often thought that with this contribution the objective state of knowledge was ready for Planck. (That is, the necessary condition). Both Dugas and Martin J. Klein (see notes 28 and 29) adduce the fact that the model developed by Boltzmann in his 1877 memoir, where he analyzes the connection between the second law and the theory of probability, was deliberately followed by Planck in his paper, "Zur Theorie des Gesetzes der Energie-Verteilung im Normalspectrum". It is also assumed that it was Planck's conservatism (that is, psychological motivation in his opposition to atomism,) that made him struggle for so many years against probability considerations. This seems to me wrong. The hard core of Planck's research programme was an attempt to explain the world on the basis of principles and not on the basis of hypotheses about the structure of matter. Only after Boltzmann developed his methodology of atomism as mental pictures and abandoned his atomistic realism, could Planck finally accept the Boltzmannian picture and introduced a problem-shift into his own methodology according to which probability considerations became part of those general principles to which all phenomena have to be reduced.

logical and belong to what I called the cognitive influences (image of science) on the choices of problems and I shall deal with them further on.³⁹

However, a serious argument of clearly scientific nature (that is, in the body of knowledge), took place in 1896 and contributed to the same problem-shift. It has to do with the statistical foundations of Boltzmann's theory.

Ernst Zermelo, first, relying on a theorem of Poincaré, pointed out another paradox in Boltzmann's early work on gas theory. He showed that according to the kinetic theory an isolated gas behaves quasi-periodically, that is: in a system consisting of particles acting on each other by forces which are functions only of their positions in space, any configuration will recur within given limits of exactness. This contradicted irreversibility. This is the so-called "Wiederkehreinwand" or "recurrence paradox." Boltzmann answered in two or rather three papers clarifying his position.⁴⁰ He reiterated the essentially statistical nature of the second law but he added two considerations which illuminate the further shift in his problem situation. Boltzmann pointed out the time necessary for the Poincaré recurrences is so long that it is natural that the world seems to us to have an irreversible character. In view of the evolutionary development of the world, in such inconceivably long spans of time such changes might occur in the world (and perhaps even its laws) that we cannot predictably deal with it. From Boltzmann's argument we see that the very question whether a probabilistic argument is "scientific" or not depends on the image of science and not on the existing body of knowledge. The problem is: is high probability qualitatively or only quantitatively different from certainty?

³⁹It serves also as an illustration of how strong is the interaction between the image of science and the metaphysics chosen as the hard core of an SRP.

⁴⁰The Zermelo-Boltzmann debate consists of:

- E. Zermelo, "Über einen Satz der Dynamik und die mechanische Wärmetheorie," *Wied. Ann.*, 57 (1896), 485–494.
- L. Boltzmann, "Entgegnung auf die Wärmetheoretischen Betrachtungen des Hrn. E. Zermelo," *Wiss. Abh.*, III, pp. 567–578.
- E. Zermelo, "Über mechanische Erklärungen irreversibler Vorgänge. Eine Antwort auf Hrn. Boltzmann's Entgegnung," *Wied. Ann.*, 59 (1896), 793–801.
- L. Boltzmann, "Zu Hrn. Zermelo's Abhandlung 'Über die mechanische Erklärung irreversibler Vorgänge'," *Wiss. Abh.*, III, pp. 579–586.
- L. Boltzmann, Über einen mechanischen Satz Poincaré's," *Wiss. Abh.*, III, 587–595.

The idea of evolution as the most fundamental process applying to everybody and everything is added here to the hard core of Boltzmann's SRP. This is a serious shift indeed. The evolutionary idea⁴¹ introduces a change also in another way. As we shall see in the debate with the phenomenologists, Boltzmann rejected the view that we should try to adapt our mental pictures of the world to human laws of thought. The reasoning behind it was that even these are not time-independently fixed. The so-called laws of thought are influenced in every individual by the environment in which he grows up and, more important, in the species the laws of thought evolve, that is change, from generation to generation. Therefore, we should not adapt our picture of the world, to what to us seem to be the human laws of thought.⁴² Now, reversibility is one of those mechanical principles which fit our thought-processes very well and thus we would like to see the whole world accordingly. Boltzmann prefers to construct free mental pictures about the world which rely on its statistical irreversible character.

One of the famous formulations of the idea which Boltzmann rejected, is by Helmholtz: "the more I study phenomena, the more I am struck by the uniformity and agreement in the action of mental processes."⁴³ An appropriate explication of this remark is given by Meyerson in the following words: "the great physicist (Helmholtz) means that the unconscious psychological processes inseparably accompanying visual perception are identical with those of conscious thought."^{44, 45}

I have reviewed so far Boltzmann's work on gas theory as a result of his first scientific research programme. I tried to show that it was a

⁴¹Darwin's influence on Boltzmann and other turn-of-the-century natural philosophers deserves a special study, but several illustrations will be given below.

⁴²This debate was central to the neo-Kantian epistemology, but Boltzmann connected it with Darwinian theory. The later formulations and the theories of Sapir-Whorf with rebuttals by Piaget and Chomsky, all originate in this debate.

⁴³H. von Helmholtz quoted by E. Meyerson in the Preface to his *Identity and Reality*, Dover, 1956.

⁴⁴E. Meyerson, Preface to *Identity and Reality*, Dover, 1956

⁴⁵As a result of the Boltzmann-Zermelo argument, Boltzmann suggested a completely imaginary model for a system which was supposed to show the seemingly paradoxical features of the H-curve. This model showed how some of the most criticized properties of the H-curve could be realized and it seems to me that such a model constitutes a typical "Gedankenexperiment" which could have been suggested by Boltzmann only after abandoning his atomistic realism and after the development of the theory of mental pictures. The important feature of Gedankenexperimente is that one is ready to set up unrealizable situations and from these to draw conclusions about the real world.

programme typical of the Viennese school of physics and followed the problem-shifts in this progress as a result of his scientific debates especially with Loschmidt and Zermelo.

In Boltzmann's own work, it seems to be beyond doubt that the first problem-shift, where he abandoned his mechanistic reductionism and introduced probability as a principle, was a progressive problem-shift. With the assessment of the second major problem-shift I shall deal at the end. However, it is already at least clear that such questions as "what did Boltzmann really think about the existence of atoms?" as put by such a thorough critic as Brush in the introduction to his translation of Boltzmann's "Gastheorie"⁴⁶ are meaningless unless qualified by referring to a given period in his work.

3. Boltzmann and energetics

An important group of scientists led by Wilhelm Ostwald and Georg Helm developed in the 1870's and 1880's a metatheory of science, which later turned into an almost religious cult with the following hardcore metaphysics: all hypotheses should be banned from science and all observable phenomena should be reduced to one fundamental principle, namely the principle of energy. Its programme was to develop all scientific fields deductively from this unitary principle. It combined in its core the phenomenology of Kirchhoff, Mach and other great philosophers-scientists who all tried to eliminate untestable speculations from science, but it added to this phenomenology a scientific monism harking back to the "Naturphilosophie" of the beginning of the century. The energeticists distinguished between hypotheses—be they as general as they may—and principles.

For them any idea which had to do with the structure of matter could not be a principle. Thus atomic or molecular principles were for them cases of contradiction in terms. The only principle was that of energy. At first only inanimate nature was to be reduced to this principle, later phenomena of life as well, and finally from 1900 onwards, Ostwald made an attempt to deal with all psychic phenomena in the same way. There is no need here to enter into details on energetics. Enough is known about it and I shall restrict myself to two remarks relating to Boltzmann.

⁴⁶L. Boltzmann, (S. Brush tr.), *Lectures on Gas Theory*, University of California Press, 1964.

Boltzmann attacked energetics at first by proving that in science and especially in mechanics its programme had failed, in other words by attacking it on internal grounds. One could not deduce all phenomena from this principle. But later he realized that energetics was becoming more than science. It became a mood on a popular level—a new image of science—and in this he saw great danger to all science.

However there is also an external-motivational element here. Boltzmann's philosophical opposition to energetics cannot be disentangled from his complex personal relationship with Ostwald. They were good personal friends. Indeed, Ostwald was responsible for inviting Boltzmann to Leipzig in 1900. So, for two years, the heads of the two great rival schools, or "Parteien" as Boltzmann calls them, taught at the same university and participated in the same seminar. There exist several reports about the scientific discussions between the two men, and it is remarked that generally Ostwald had the better of the argument with his overbearing personality, ebullient energy and great rhetoric ability. Also, in his Leipzig period, Boltzmann went through one of his worst fits of depression and suffered greatly. There is a fascinating account of this in A. Jaffe's paper.⁴⁷

What interests us is that Boltzmann's reaction to the energeticists' monism was a theoretical-methodological pluralism which is a definite problem-shift in his SRP. Not only is he ready to add probability principles to the hard core of his programme, abandoning mechanistic reductionism, but on the way towards the development of his methodology of mental pictures, he introduces the idea that *fruitfulness* and not *truth* is the relevant question about methodologies and hypotheses and he becomes a theoretical pluralist.

4. Boltzmann and the phenomenologists

The scientific programme that went under the name of "phenomenology" (and which was strikingly different from the later philosophical

⁴⁷The best account of the Boltzmann-Ostwald argument is given in their own works; especially one should compare Ostwald's 1895 paper "The Victory over Scientific Materialism" with Boltzmann's 1899 "On the Development of the Methods of Theoretical Physics in Recent Times." The second paper appears in the original in *Pop. Schriften*, vol. II, pp. 198–227. My English translation of it with a short introduction appeared in *Philosophical Forum*; a translation with introduction is to be published soon.

doctrine of the same name, due to Husserl and others) was the representation of narrowly delimited fields of science by differential equations. The programme was formulated by theoretical physicists who wanted to save theoretical physics from the anti-theoretical campaign spreading in Europe and especially in Germany as a reaction to the *Naturphilosophie*. Its most brilliant spokesman was Kirchhoff. It is needless to stress that this is a dialogue between two different images of science in a cognitive and social sense. The differential equations served as rules for connecting numbers and geometrical concepts. This methodology was combined with a moral code—"do not form any pictures" (not infrequently connecting it with the Biblical "Thou shalt not make a graven image"). This phenomenology, which Boltzman called "mathematical phenomenology" as against the phenomenology of the energeticists, roused Boltzmann's scathing criticism.

He demonstrated that "Die Behauptung, Differenzialgleichungen gingen weniger über die Tatsachen hinaus als die allgemeinste Form atomistischer Ansichten, auf einem Zirkelschluss beruhen würde."⁴⁸ The beauty of the demonstration is that Boltzmann shows how atomism goes beyond the evidence of the senses only if we actually rely on the assumption that our perceptions are derived from continuum pictures. On the other hand, if we actually think atomistically, the continuum pictures will form the empirically not warranted results. In reality, all differential equations are built by atomistic ideas and "derjenige, welcher die Atomistik durch Differenzialgleichungen losgeworden zu sein glaubt, den Wald vor Bäumen nicht sieht."⁴⁹ In short, atomism underlies all continuum concepts and according to Boltzmann this was fully realized by scientists like Laplace, Poisson, and Cauchy. As one of the important examples, Boltzmann shows that Hertz's famous remark that Maxwell's theory is Maxwell's equation can only be understood if we presuppose an atomic structure of matter.⁵⁰

⁴⁸L. Boltzmann, "Über die Unentbehrlichkeit der Atomistik in der Naturwissenschaft," *Pop. Schriften*, Leipzig, 1905, p. 193.

⁴⁹Ibid.

⁵⁰Sir Basil Schonland in his book *The Atomists* emphasizes that Hertz admittedly never felt quite sure about having understood Maxwell correctly. The reason, in my opinion, is that Maxwell under attack withdrew his mechanical model of the ether and formulated his theory in as hypothesis-free a way as possible, yet understanding of this theory, on the deepest level, is impossible without thinking in terms of the famous abandoned "scaffolding." Hertz, on the other hand, was a

Another argument to prove that one can safely extrapolate beyond the limits of the directly observable is brought by Boltzmann in his 1899 lecture on Loschmidt. Here he points out that in astronomy, to which the phenomenologists do not apply their scepticism, all theories must go beyond the limits of the observable. One only has to take care not to extrapolate too much. The criterion as to how much is too much he thinks to have been found in the developing method of theoretical physics, meaning mainly the method of mental pictures in which he sees true progress. Boltzmann returned to his critical discussions of phenomenology in almost every lecture he gave from 1895 till the last one in 1905.

The importance of the debate lies in the fundamental problem-shift which forced Boltzmann to replace the hard core of his SRP by a very different one. Instead of atomism and probability as basic principles, which describe the world realistically, Boltzmann now introduces atomism as a *mental picture*, which is created independently of the phenomena and only afterwards compared to them. This is the hard core of his new meta-scientific RP, a new view of the role of science developed by Boltzmann in the last six or seven years of his life and which became the most successful scientific programme from Planck to Schrödinger.⁵¹

To formulate this problem-shift in simpler terms: Before his argument with the phenomenologists, Boltzmann would have answered the question "do atoms exist?" in the affirmative. The fundamental shift is that afterwards Boltzmann does not consider the question an important one any more. The proper question for him is: "Is atomism as a mental picture fruitful as the hard core of an SRP?"⁵²

thorough phenomenologist who was incapable of thinking in atomistic terms. In this light, I think, the much-abused quotation becomes clear: "To the question what is Maxwell's theory, I know of no shorter or more definite answer than that it is the theory embodied in Maxwell's equations. I have not always felt quite certain of having grasped the physical significance of his statements" (translation as given by Schonland, *op. cit.*, p. 61).

⁵¹The dynamics of growth is well-indicated here. The cognitive process, the new image of science, determines a new problem for science and becomes thus incorporated in the internal development of the body of knowledge.

⁵²Because of this new image of science there was a widespread tendency to see in Boltzmann an early logical positivist or even an instrumentalist. The commentators who claim this do not realize the depth of the difference between considering a question as not any more the most important one and considering it meaningless. Furthermore Boltzmann never gave up the idea that the task of natural science is to explain and not only to predict the phenomena.

5. *The new view of the role and nature of science*

What is left for me to do is to collect the various remarks which I have interspersed so far and to prove textually that Boltzmann held the views ascribed to him, to trace the various origins of these views and to check the impact of this new image of science on Boltzmann's own work and on his followers' work. The new programme is to develop all branches of science by constructing a mental picture of a world made of atoms to which we can apply any principles or laws without restriction (not necessarily just mechanics to which all other fields should be reducible, or electrodynamics). The criterion of a "good" picture will be that of fruitfulness, whether it helps us explain all phenomena known to us and predict novel ones. To some extent the picture should fit the world as we see it, but only as far as to avoid blatant contradictions. As a methodological device we should construct our pictures uninfluenced by what we know or by what we see. In other words, we should not first list the phenomena to be explained and then construct a picture. It is also claimed that our mental pictures should not be too specific; on the contrary, as general as possible.

This atomism is much more abstract than the realistic atomism of hard elastic spheres. This is a kind of atomism which can be easily extended (indeed it was soon after Boltzmann) from matter to space, time and energy. Boltzmann himself talked about fundamental smallest units of time and endorsed atoms of electricity. This atomism underlay all differential equations of mathematical physics. The programme was accompanied by theoretical pluralism: "Es ist selbstverständlich vorteilhaft möglichst viele Bilder zu besitzen"⁵³ with only fruitfulness to decide between them. In addition Boltzmann advocates, almost in our terms, critical dialogues between different SRP's. His view of the growth of knowledge is clearly dialectical.

Yet enough is left of the old Boltzmann insofar that the theories as mental pictures must not only be fruitful, but also depict the phenomena in the most complete and simplest way. He admits that our conceptions and pictures exist only in us, but the phenomena to which these pictures have to conform exist independently of us, and thus, instead of saying molecules exist, we should rather say "unsere betreffenden

⁵³*Op. cit.* above (note 48), p. 141.

Vorstellungsbilder sind ein einfaches und zweckmässiges Bild der beobachteten Erscheinungen.”⁵⁴

Boltzmann distinguishes between the old method of theoretical physics where axioms were laid down and all phenomena deduced from them, the inductivism of the phenomenologists and his own new method. The first one he called Euclidian methodology, while his own he describes as deductive. The axiomatics of the Euclidian methodologists try to describe the true essences of the world and this was rightly attacked by Kirchhoff and the other phenomenologists.^{55, 56}

But he does not mean that instead of such axioms we have to limit ourselves to pure observables. The new view carries with it a new image of science which influences heavily the choice of problems. By choosing a new kind of “Fragestellung,” the result is “Was die eigentliche Ursache sei, dass die Erscheinungswelt sich gerade so abspielt, was gewissmassen hinter der Erscheinungswelt verborgen ist und sie treibt, das zu erforschen, betrachten wir nicht als Aufgabe der Naturwissenschaft.”⁵⁷

As a result of this new shift, Boltzmann now deals with the ener-

⁵⁴Ibid.

⁵⁵L. Boltzmann, “Die Grundprinzipien und Grundgleichungen der Mechanik,” *Pop Schriften*, p. 258. “Alle unsere Vorstellungen und Begriffe sind ja nur innere Gedankenbilder, wenn ausgesprochen Lautkombinationen. Die Aufgabe unseres Denkens ist es nun, dieselben so zu gebrauchen und zu verbinden, dass wir mit ihrer Hilfe allezeit mit grösster Leichtigkeit die richtigen Handlungen treffen und auch andere zu richtigen Handlungen anleiten. Die Metaphysik hat sich da dem nüchternsten praktischsten Standpunkte angeschlossen, die Extreme berühren sich. Die begrifflichen Zeichen, welche wir bilden, haben also nur eine Existenz in uns, die aussern Erscheinungen können wir nicht mit dem Masse unserer Vorstellungen messen. Wir können also formell derartige Fragen aufwerfen, ob bloss die Materie existiert und die Kraft eine Eigenschaft derselben ist, oder ob letztere, von der Materie unabhängig existiert, oder ob umgekehrt die Materie ein Erzeugnis der Kraft ist; aber es haben alle diese Fragen gar keine Bedeutung, da alle diese Begriffe nur Gedankenbilder sind, welche den Zweck haben, die Erscheinungen richtig darzustellen.”

⁵⁶Ibid., p. 261. “Es wird nun eine Darstellungsweise geben, welche ganz besondere Vorzüge, aber auch wieder ihre Mängel besitzt. Diese Darstellungsweise besteht darin, dass wir eingedenk unserer Aufgabe, bloss innere Vorstellungsbilder zu konstruieren, anfangs lediglich mit gedanklichen Abstraktionen operieren. Hierbei nehmen wir noch gar keine Rücksicht auf etwaige Erfahrungstatsachen. Wir bemühen uns lediglich, mit möglichster Klarheit unsere Gedankenbilder zu entwickeln und aus denselben alle möglichen Konsequenzen zu ziehen. Erst hinterher, nachdem die ganze Exposition des Bildes vollendet ist, prüfen wir dessen Übereinstimmung mit den Erfahrungstatsachen, motivieren also in dieser Weise erst hinterher, warum das Bild gerade so und nicht anders gewählt werden musste, worüber wir vorher nicht die leiseste Andeutung geben. Wir wollen dies als die deduktive Darstellung bezeichnen.”

⁵⁷L. Boltzmann, “Die Grundprinzipien und Grundgleichungen der Mechanik,” *Pop. Schriften*, Leipzig, 1905, p. 264.

geticists and the phenomenologists together. He sees one of the great successes of the deductive way of thought in furthering non-realistic atomistic pictures which can freely develop each field independently of experimental limitation.⁵⁸

In his attempts to clarify the new methodology as much as possible, Boltzmann refers again and again to the work of Heinrich Hertz. On the one hand he sees Hertz’s attempt to develop the theory of mechanics on a basis of hypothesis-free mental pictures as a kindred attempt; he even calls Hertz’s method also deductive. Yet he is troubled by the flavor of mathematical phenomenology. The basic difference between the two of them, Boltzmann pinpoints in the fact that Hertz demanded that the pictures which we constructed have to be in accord with the laws of thought, while Boltzmann under Darwinian influence (as mentioned above) considers the laws of thought subject to the principle of evolution, thus changeable, and wants to free our mental constructs even of the constraints of the laws of thought.⁵⁹

⁵⁸L. Boltzmann, “Statistische Mechanik,” *Pop Schriften*, p. 357: “Was den ersten Punkt betrifft, so weisen die mannigfältigsten Tatsachen der Wärmetheorie, der Chemie, der Kristallographie darauf hin, dass in den dem Anscheine nach kontinuierlichen Körpern keineswegs der Raum unterschiedlos und gleichförmig mit Materie erfüllt ist, sondern dass sich darin ungemein zahlreiche Einzelwesen, die Moleküle und Atome, befinden, welche zwar außerordentlich, aber nicht im mathematischen Sinne unendlich klein sind. Man kann ihre Grösse nach verschiedenen, sehr disparaten Methoden berechnen und erhält immer das gleiche Resultat. Die Fruchtbarkeit dieses Gedankens hat sich in neuester Zeit wieder bewährt. Alle Erscheinungen, welch an den Kathodenstrahlen, Bequerelstrahlen usw. beobachtet wurden, deuten darauf hin, dass man es dabei mit winzigen fortgeschleuderten Teilchen, den Elektronen, zu tun hat. Nach einem heftigen Kampfe siegte diese Ansicht vollständig über die ihr anfangs gegenüberstehende Undulations-theorie dieser Erscheinungen. Die erstere Theorie taugte nicht nur viel besser zur Erklärung der bisher bekannten Tatsachen, sie bot auch Anregung zu neuen Experimenten und gestattete bisher unbekannte Erscheinungen vorauszusagen; hierdurch entwickelte sie sich zu einer atomistischen Theorie der gesamten Elektrizitätslehre. Wenn sich diese mit gleichem Erfolge wie in den letzten Jahren weiter entwickelt, wenn Erscheinungen, wie die von Ramsay beobachtete Umwandlung von Radiumemanation in Helium nicht vereinzelt bleiben, so verspricht diese Theorie noch zu ungeahnten Aufschlüssen über die Natur und Beschaffenheit der Atome zu führen. Die Rechnung ergibt nämlich, dass die Elektronen noch viel kleiner als die Atome der ponderablen Materie sind, und die Hypothese, dass die Atome aus zahlreichen Elementen aufgebaut sind, sowie verschiedene interessante Ansichten über die Art und Weise dieses Aufbaus sind heute in aller Munde. Das Wort Atom darf uns da nicht irreführen, es ist aus alter Zeit übernommen; Unteilbarkeit schreibt heute kein Physiker den Atomen zu.”

⁵⁹Darwin’s influence on Boltzmann’s last SRP is important and is well illustrated in the following passages from the *Pop. Schriften*, p. 258: “Besonders klar hat dies Hertz in seinem berühmten Buche über die

In spite of having pinpointed this difference, Boltzmann keeps referring to the methodology of Hertz and Maxwell in a curiously ambiguous way. One is never certain whether Boltzmann accepts, rejects or partially accepts the views of Hertz and Maxwell. Such an ambiguity is completely out of tune with Boltzmann's previous sharp arguments and crystal clear formulation of differences of opinion. As usual this is ascribed by externalists to purely psychological reasons; (growing tired and disenchanted, loosing grasp, more frequent attacks of depression etc.). To me, however, this seems to be a rational development in Boltzmann's own changing image of science. The more the Darwinian evolutionary theory became his guide, the more distant seemed to him any kind of mathematical phenomenology or any kind of mental pictures in anything fixed like mathematics or laws of thought. Here he and Hertz diverged.

With Maxwell, the problem was quite different. Maxwell developed

Prinzipien der Mechanik angesprochen, nur stellt *Hertz* daselbst als erste Forderung die auf, dass die Bilder, welche wir uns konstruieren, den Denkgesetzen entsprechen müssen. Gegen diese Forderung möchte ich gewisse Bedenken erheben oder wenigstens sie etwas näher erläutern. Gewiss müssen wir einen reichen Schatz von Denkgesetzen mitbringen. Ohne sie wäre die Erfahrung vollkommen nutzlos; wir könnten sie gar nicht durch innere Bilder fixieren. Diese Denkgesetze sind uns fast ausnahmslos angeboren, aber sie erleiden doch durch Erziehung, Belehrung, und eigene Erfahrung Modifikationen.

p. 317: "Es ist bekannt, dass die *Darwinsche Lehre* keineswegs bloss die Zweckmässigkeit der Organe des menschlichen und tierischen Körpers erklärt, sondern auch davon Rechenschaft gibt, warum sich oft Unzweckmässiges, rudimentäre Organe, ja geradezu Fehler in der Organisation bilden konnten und mussten."

p. 320: "Vom Standpunkt der *Darwinschen Theorie* ist auch das Verhältonis des Instinktes der Tierwelt zum Verstände des Menschen begreiflich. Je vollkommener ein Tier ist, desto mehr treten bei demselben neben dem Instinkte bereits Spuren von Verstand auf."

p. 339: "Mach hat selbst in so geistreicher Weise ausgeführt, dass keine Theorie absolut wahr, aber auch kaum eine absolut falsch ist, dass vielmehr jede Theorie allmählich vervollkommnet werden muss, wie die Organismen nach der Lehre *Darwins*. Dadurch, dass sie heftig bekämpft wird, fällt das Unzweckmässige allmählich von ihr ab, während dass Zweckmässige bleibt, und so glaube ich, Prof. Mach am besten zu ehren, wenn ich in dieser Weise zur Weiterentwicklung seiner Ideen, soweit es in meinen Kräften steht, das Meinige beitrage."

p. 353. "Wenn wir diese Denkgesetze nennen wollen, so sind sie insofern freilich aprioristisch, als sie vor jeder Erfahrung in unserer Seele oder, wenn wir lieber wollen, in unserem Gehirn vorhanden sind. Allein nichts scheint mir weniger motiviert, als ein Schluss von der Apriorität in diesem Sinne auf absolute Sicherheit, auf Unfehlbarkeit. Diese Denkgesetze haben sich nach den gleichen Gesetzen der Evolution gebildet, wie der optische Apparat des Auges, der akustische des Ohres, die Pumpvorrichtung des Herzens. Im Verlauf der Entwicklung der Menschheit wurde alles unzweckmässige abgestreift, und so entstand jene Einheitlichkeit und Vollendung, Welch leicht Unfehlbarkeit vortäuschen kann."

a methodology which influenced Boltzmann heavily and is not dissimilar from Boltzmann's theory of mental pictures. Maxwell wanted to eliminate the Newtonian hypothetico-deductive method, and to replace it by very general hypotheses which then together with the empirical data would yield the specific theories. (This is in direct opposition to the hypothetico-deductive method where one would start with specific theories and then confront these with empirical data.) The most general hypothesis which Maxwell wanted to underlie science was that all bodies are material systems. Such an hypothesis is not a physical hypothesis but rather a physical analogy, a concept very near to Boltzmann's mental pictures. With all this Boltzmann enthusiastically agreed, but he could not then understand the methodological status of Maxwell's mechanical model on which his theory of electromagnetism was built—he could not understand the fact that under pressure Maxwell abandoned it and left only the mathematical structure. This in Boltzmann's eyes was equivalent to having become a mathematical phenomenologist.⁶⁰

⁶⁰The whole problem of the development of Maxwell's methodology has come nearer to a solution in a recent issue of the *Journal of the History and Philosophy of Science* wholly dedicated to Maxwell's methodology, in the various fields of his creativity. The story of Boltzmann's analysis of Maxwell's needs deserves separate treatment which should center on Boltzmann's two volumes of lectures on Maxwell's theory of electricity, which appeared in 1891 and 1893 respectively.

Here let me only remark that in the above-mentioned issue on Maxwell there is an article by Dorling who views Maxwell as a conventionalist who went through a short realistic stage and then became a kind of sophisticated conventionalist; I see his development differently.

I wish to sketch it here briefly: Maxwell started out as a Newtonian which in England in the 1850's meant adhering to Newtonian metaphysics (that is presupposing the existence of material systems, and moreover that the world is built up of discrete material particles between which central forces are acting at a distance) and methodologically to the hypothetico-deductive method (which meant formulating as specific and non-speculative hypotheses as possible, attributing to them physical reality and checking them against the experimental data). Under the influence of Faraday—Faraday's success, his prestige, his new concepts like the field and his well-known anti-Newtonian attitude (both his anti-Newtonian Physics and his metaphysical anti-Newtonian methodology)—Maxwell decided to abandon the Newtonian methodology while sticking to the Newtonian matter and motions metaphysics. In other words, he rejected the Newtonian attempts to escape speculative theories and he rejected the argument that if a theory is so speculative that it cannot be experimentally tested, then no physical reality should be ascribed to it. On the contrary, as Faraday's disciple, in accepting a physical reality of fundamental non-experimental theories and entities (field!) Maxwell gradually developed that most general metaphysical theory which would serve as a conceptual framework for formulating specific experimentally testable scientific theories. It is in this methodology which is so clearly described in his 1875 lecture to the Chemical Society, "On the dynamical evidence of the molecular constitution of bodies."

One further point to be mentioned when viewing Boltzmann's changing image of science: at one stage Boltzmann tried to deal with the methodological changes as only different kinds of expression; later he abandoned this attitude and realized a depth of the change. The change in what he considers as the task of science can be followed starting in his 1886 Kirchhoff memorial lecture, where he said: "Die Einheit der Naturkräfte überall aufzudecken ist ein Hauptziel der Naturwissenschaft."⁶¹

The comparison and critical dialogue with Hertz is prolonged and interesting. Boltzmann's preoccupation with the Hertzian view of science can be seen in the fact that the dissertation topic of his illustrious student Paul Ehrenfest was Hertz's methodology.⁶²

Besides Maxwell, the greatest influence on Boltzmann, as already mentioned above, were the evolutionary ideas of Darwin. Not only did he oppose rigid adaptation of the mental pictures to the laws of thought, but he also took into account evolutionary development of our mental pictures themselves and developed a methodology of permanent change. In this one point, he was not really followed except perhaps by Schrödinger, and we have yet to see how a SRP looks, the hard-core metaphysics of which is a principle of processes and of total change.

⁶¹L. Boltzmann, "Gustav Robert Kirchhoff" *Pop. Schriften*, p. 57. Here Boltzmann is a direct disciple of Helmholtz—see Helmholtz's introduction to his 1847 paper "Die Erhaltung der Kraft."

⁶²Boltzmann on Hertz in *op. cit.*, note 57, p. 262: "Ein wirklicher Fehler der deduktiven Methode besteht dagegen darin, dass der Weg nicht sichtbar wird, auf welchem man zur Auffindung des betreffenden Bildes gelangte."

p. 266: "Die Prinzipien der Mechanik, welche Hertz aufstellt, sind von ausserordentlicher Einfachheit und Schönheit. Sie sind natürlich nicht vollständig frei von Willkürlichkeit, aber ich möchte sagen, die Willkürlichkeit ist auf ein Minimum beschränkt. Das von Hertz unabhängig von der Erfahrung konstruierte Bild hat eine gewisse innere Vollendung und Evidenz. Es enthält an sich nur wenig willkürliche Elemente. Hingegen steht offenbar mein Bild weit zurück. Letzteres enthält weit mehr Züge, welche den Stempel davon an sich tragen, dass sie nicht durch eine innere Notwendigkeit bestimmt sind, sondern bloss eingefügt wurden, um hinterher dann eben die Übereinstimmung mit der Erfahrung zu ermöglichen."

p. 268: "Mann hat also nur folgende Wahl, entweder man lässt die Natur des Mechanismus, welcher die Gravitation, die elektrischen und magnetischen Erscheinungen erzeugen soll, unbestimmt und willkürlich. Dadurch entsteht eine unerträgliche Unanschaulichkeit, indem man genötigt ist, immer mit Gleichungen zu operieren, von denen man nur einige ganz allgemeine Eigenschaften kennt, deren spezielle Form aber vollständig unbekannt ist, oder man bemüht sich, einen bestimmten Mechanismus zu wählen, wodurch man dann wieder in ebensoviele Willkürlichkeiten als Schwierigkeiten verwickelt wird."

In the years when Boltzmann developed this version of his SRP he was not doing any more original work in physics. And thus, for Boltzmann himself, there is no way of telling whether the last and more fundamental problem-shift was progressive or degenerative. That it was progressive for the greatest physicists of the next generation is beyond doubt.

6. Boltzmann's changing image of science as against a natural background

So far I have followed the changing SRP and attributed the changes to a combination of certain factors relating to the objective body of knowledge and others relating to the changing image of science. But the changing image of science itself is connected with the general cultural atmosphere of Vienna in the 1860s and Vienna at the turn of the century. Vienna in the 1860s, in spite of its waning political influence, was still a cultural crossroads where happy-go-lucky realism ruled and nothing dogmatic was tolerated. In science Vienna had escaped the degenerating influence of the Naturphilosophie which never succeeded in finding an intellectual base for its heavy mystical Teutonic depth. As a result, Vienna also escaped the even more degenerating reaction of arid empiricism which was so typical of the famous scientific laboratories in the Germany of the 1830s and 1840s.

On the other hand, in the Viennese philosophical tradition Baconianism was never taken too seriously. There were no great traditions with past scientific successes which had to be venerated or violently opposed. In other words, Boltzmann, Loschmidt and Stefan never had to face such methodological battles which were Maxwell's daily bread. It was a simple many-sided, creative common-sensical world.

By 1900 all this had changed. Growing disenchantment with the world, and with Austria especially, and cultural disintegration created an atmosphere of decadence. The previous cosmopolitan attitude and commonsensical realism now laid bare its negative side—the lack of commitment, lack of depth in traditions, there was nothing to cling to. Viennese intellectuals were among the first to emigrate in groups to the USA in this early period. Boltzmann lived through this transition period and his abandonment of the primitive atomistic realism and his endorsement

of theoretical and methodological pluralism are, to say the least, not contradictory to the changing spirit of the times.

Very little study has been made of the intellectual atmosphere of Vienna between the 1860s and the period of the Vienna Circle, Wittgenstein, etc. which began after the First World War. It is an open question whether we are justified in talking about a special Viennese spirit in science or whether we may indulge in detecting causal relationships between the early Boltzmann, the statistical mechanics and the ensuing changes influenced by Karl Kraus, the architect Adolf Loos, Freud, and others. Some of this has been at least hinted at in recent publications. Freud's connection with the debates around energetics is worth attention and Wittgenstein philosophy has been traced to his Viennese cultural background by Toulmin.⁶³

The direction of the argument which I am hinting at is that the spirit of synthesis, the attempts to amalgamate different metaphysical world-views faded inevitably by too much proliferation and too little commitment into intellectual meaninglessness. It was almost self-evident that when, on top of this, the First World War ruined physically the remnants of the Viennese cultural center, a violent dogmatic anti-metaphysical positivism occurred there, embodied in the Vienna circle. Even if this description is over-simplified (even on a hint-level) yet one cannot read Karl Kraus in "Die Fackel" or in "Die Letzten Tage der Menschheit," without realizing that it is not senseless. Boltzmann's great creative efforts were accomplished in the times when he still had an image of science as a great synthesizing enterprise in the sense of Helmholtz, Karl Exner and Leibniz.

As a closing example let me trace a complementary story: the problem-shift which occurred in Planck's SRP under the influence of Boltzmann. Planck had spent the first 15 years of his scientific career in an unusually ambitious SRP. It wanted to reduce all physics to the two laws of thermodynamics. At its hard core was the plan to formulate the second law as generally as had been done with the first law, for only then could one eliminate all specific hypotheses as to the nature of matter. He combined an attitude which originated with Helmholtz (as far as Planck was concerned), namely that all science could be derived from

⁶³S. Toulmin, "Ludwig Wittgenstein," *Encounter*, January 1969, 58–71.

a small number of great unitary principles with the Kirchhoffian mathematical phenomenology. This programme itself, even if Planck had not modified it, and had he not reached the conclusion as to the atomic nature of radiation-emission and absorption, would have made him an unusually great 19th century physicist.

The very depth and daring of the programme made him an outsider among the phenomenologists and a worthy successor of Kirchhoff (incidentally, Kirchhoff's chair was offered to Boltzmann and only after Boltzmann's refusal offered to Planck). As is well-known the story developed differently. While working on the series of papers "Über irreversible Strahlungsprozesse,"⁶⁴ also a paper by Boltzmann appeared on the same topic⁶⁵ and as a result of the argument, Planck finally realized that the principles of thermodynamics and classical electrodynamics were not sufficient to account for the processes of emission and absorption of radiation and that the probability principle had to be adduced to the fundamentals as well. As mentioned above, it seems to me that this shift in Planck's research programme was made possible only by the fact that in the dialogue with Boltzmann it became manifest that Boltzmann's ideas on probability had become part of his theory of mental pictures. That is, probability considerations were now presented in a framework which was philosophically, rationally more acceptable to Planck than it had been previously.

Now the whole Zermelo-Boltzmann argument looked different and Planck turned to Boltzmann's work for help. The clearest formulation of the problem-shift that had occurred in Planck's SRP was given in his 1909 Columbia lectures. This was a time before Planck resigned himself to Einstein's views some time during or after the first Solvay Congress —he still tried to recapture honestly the intellectual changes which led him to his discoveries. He was still very worried about the Einsteinian shift and tried to show that his own development did admit problem-shifts and yet they did not commit him to such radical changes as demanded by Einstein: "I intend above all to bring out the peculiar significance of the atomic theory as related to the present general system of theoretical

⁶⁴M. Planck, *Physikalische Abhandlungen und Vorträge*, Braunschweig, 1958, vol. II, pp. 493–600 and 614–667.

⁶⁵L. Boltzmann, "Über irreversible Strahlungsvorgänge, I," *Wissenschaftliche Abhandlungen*, Leipzig, 1909, vol. III, p. 615.

physics, for in this way only, will it be possible to regard the whole system as one containing within itself the essential compact unity . . . it is fundamental to recognize . . . the division of all physical processes into reversible and irreversible processes. Furthermore we shall be convinced that the accomplishment of this division is only possible through the atomic theory of matter or, in other words, that irreversibility leads of necessity to atomistics.⁶⁶

Lest there be any doubt in my interpretation of Planck, let me bring a few quotations from the same lecture: "Now the idea of irreversibility harks back to the idea of entropy. . . . I have completed the emancipation of the entropy idea from the experimental arts of man, and the elevation of the second law thereby to a real principle, was the scientific work of Ludwig Boltzmann. Briefly stated, it consisted in general of referring back the idea of entropy to the idea of probability . . . the new conception of entropy introduces a great number of questions, new requirements. . . . The first requirement is the introduction of the atomic hypothesis into the system of physics." As to my interpretation that Planck wanted to absorb Boltzmann into his old conceptual framework and reject Einstein: "I desire here only to mention that the novelty involved by the introduction of atomistic conceptions into the theory of heat radiation is by no means so revolutionary as, perhaps, might appear at the first glance. For there is, in my opinion at least, nothing which makes necessary the consideration of the heat processes in a complete vacuum as atomic, and it suffices to seek the atomistic features at the source of radiation, i.e. in those processes which have their play in the centres of emission and absorption of radiation."

That formulation of the statistical hypothesis which was acceptable to Planck was clearly stated by Boltzmann in his 1904 retrospective account of statistical mechanics: "Da in den Differentialgleichungen der Mechanik selbst absolut nichts dem zweiten Hauptsatze Analoges existiert, so kann derselbe nur durch Annahmen über die Anfangsbedingungen mechanisch dargestellt werden. Um die hierzu tauglichen Annahmen zu finden, müssen wir bedenken, dass wir behufs Erklärung kontinuierlich scheinender Körper voraussetzen müssen, dass von jeder Gattung von Atomen oder allgemeinen, mechanischen Individuen aus-

⁶⁶M. Planck, the third of his Columbia Lectures 1909, New York, 1915.

serordentlich viele in den mannigfaltigsten Anfangslagen befindliche vorhanden sein müssen. Um diese Annahme mathematisch zu behandeln, wurde eine eigene Wissenschaft erfunden, welche nicht die Aufgabe hat, die Bewegungen eines einzelnen mechanischen Systems, sondern die Eigenschaften eines Komplexes sehr vieler mechanischer Systeme zu finden, die von den mannigfaltigsten Anfangsbedingungen ausgehen. Das Verdienst, diese Wissenschaft in ein System gebracht, in einem grösseren Buch dargestellt und ihr einen charakteristischen Namen gegeben zu haben, gebührt einem der grösssten amerikanischen Gelehrten, was reines abstraktes Denken, rein theoretische Forschung anbelangt, vielleicht dem grösssten, Willard Gibbs, dem kürzlich verstorbenen Professor von Yale College. Er nannte diese Wissenschaft die Statistische Mechanik."⁶⁷

Boltzmann's influence on a whole generation of physicists through the fact that they accepted his whole SRP, again deserves a special discussion. Let me only add here that Paul Langevin considered his work on magnetism as a direct continuation of that of Boltzmann. Schrödinger said:

"Atomicity has the additional task by whose accomplishments alone its superiority over the phenomenological theories can be established. It has to discover and predict conditions under which the differential equations, based on conceptions of continuity, would lead —because of the really atomistic structure of matter—to evidently false conclusions. . . . His [Boltzmann's] line of thought may be called my first love in science. No other has ever thus enraptured me or will ever do so again."⁶⁸

⁶⁷L. Boltzmann, "Statistische Mechanik," *Pop. Schriften*, pp. 360-361.

⁶⁸E. Schrödinger, *Science, Theory and Man*, Dover, 1957, pp. xii-xviii.

DISCUSSION

On papers by I. LAKATOS and Y. ELKANA

BECHLER: There is a sort of inductivist's myth about Newton and Kepler, but I am convinced of that less from Duhem's argument than from two recent papers that were written, one by Wilson and one by Whiteside, about the relation between Kepler's Laws and Newton's derivation of his inverse square law. On the other hand the argument of Duhem is completely invalid and this I want to show now.

First let us accept that it is true that from Newton's theory, it follows that Kepler's laws are invalid—this is true—I accept it. Now this is a theoretical fact, but it does not follow from this theoretical fact that Newton did not derive his inverse square law from Kepler's laws—this is a historical fact in principle completely unrelated to the theoretical or logical fact. In fact, he did derive it and made several mistakes in the process of derivation.

LAKATOS: So he did not succeed in deriving it, he *thought* he derived it. BECHLER: To derive means for me to arrive at the last line of the derivation and the conclusion (in this case the inverse square law). Whether the derivation is valid or invalid, is quite another problem—it is a logical problem. The historical fact is that Newton writes that he derived it and shows the derivation, but the derivation is false—completely false, and the falsity incidentally does not inhere in the fact that there are perturbations—here Duhem was mistaken. This is not the crucial fact. The crucial fact is that he assumes the trajectories of the planets to be firstly circular, which is false, and secondly concentric, which is also false. And from these two assumptions, plus Kepler's third law (by the way he uses only Kepler's third law, neither the first nor the second), plus Huygens' law of centripetal force—he derives the inverse square law. So the whole thing, the whole derivation is valid, but baseless. That is to say: if you assume concentric and circular trajectories the conclusion follows validly, but these assumptions simply happen not to be true of the planetary motions.

ROSENFELD: I think you put the thing too sharply. You are perfectly right in defining Newton's procedure. Then you say that Newton's derivation is false—you used that word—because he assumed a circular orbit.

BECHLER: He made false assumptions.

ROSENFELD: Well, you must be careful in calling the assumption of circular orbits false—it is not what a physicist would call a false assumption. He would say it is an approximation with a high degree of validity.

BECHLER: No! It has not even a high degree of validity.

ROSENFELD: But of course it has—I mean the moon's orbit is almost circular.

BECHLER: The moon's yes, but not the planets. The planets are off-circle.

ROSENFELD: Most of the planets are not. The physicist never bothers with inessential complications when he tries to find something. One first tries to simplify the problem as much as possible. Now that is a dangerous step, of course, because one may throw away something which one thinks is unimportant and which then turns out to be important—well, one has to learn.

Now turning to Lakatos' paper, I think in your enumeration of methods you have forgotten a last one which is the right one—the only one that scientists use, namely, trial and error.

LAKATOS: But this is exactly *falsificationism* by conjecture and refutation: trial is the conjecture and error is the refutation, and my paper refutes falsificationism!

ROSENFELD: I am afraid that if one starts by criticizing Newton for having assumed the orbits to be circular, then one will get into a Byzantine discussion which will lead nowhere.

BECHLER: Yes, but let us clarify: If I talk about a valid but false derivation—do you Lakatos accept my terminology?

LAKATOS: No.

BECHLER: All right, use your own terminology.

LAKATOS: I am using the terminology of any elementary logic text.

ROSENFELD: It is not a point of logic—you could call it a point of morality.

TOULMIN: May I put in a point of order here? To bring in elementary logic texts as directly relevant here reminds me of G. E. Moore's attempt to prove that John Stuart Mill committed the naturalistic fallacy.

(G. E. Moore invented a definition such that, by reading it back anachronistically into Mill, he could poke fun at Mill.) The fact of the matter is that, in current English, the word "deduce" means the same as the word "infer," and it covers a large variety of intellectual procedures by which people, as you say, arrive at conclusions. So there is no doubt that, in what Berkeley would have called the "vulgar" acceptation of the word "deduce," he *deduced* the empirical relevance of his inverse square law from the experimental evidence of Kepler's laws.

You may say that, by standards of 20th century symbolic logic, this does not count as an example of what is now technically defined as a "deduction," but this is an anachronistic objection.

BECHLER: Let me go on with this. I don't think it is an essential point what terms we use. The essential point is this, that Newton showed how to make the steps from the motions to the conclusion of the inverse square law in his *System of the World*. Now, for the inductivist the most important question is not whether the derivation is sound, that is whether or not its factual assumptions are absolutely true, but only whether it is logically valid, that is, assuming its assumptions to be approximately true whether its conclusion follows or not. In this case the derivation is valid since if you assume Kepler's laws it follows that the central acceleration of a planet is proportional only to its distance from the sun and hence is independent of the planet. This was shown by Newton, and Born only put it nicely into a convenient analytic form, which I never heard anyone has proved to be logically faulty.

Now, you contend that this derivation is faulty for the reverse cannot be done, that is, you cannot derive Kepler's laws from the inverse square law, and hence you contend that Newton's derivation must be faulty since it is inconsistent. My point is that even if this logical fact is true, it is historically unimportant. I for one shall want to be shown in what exact step did Newton, or Born, make the invalid move. In the absence of this, and I am not aware that this was ever done—it was surely not done by Duhem—I would be ready to abandon the term derive and concede that though Newton did not perhaps derive his law, he certainly "derived" it, put in quotes.

LAKATOS: I never saw a logic text-book in which there was your "derives" in quotes. Let us really discuss this one point. My point was that —let us get these elementary things out of the way—my point was that

it is very difficult (in whatever way or sense we use the word "derive") to say that I derived *not-p* from *p* unless *p* is logically false. But Kepler's and Newton's laws are inconsistent, so that if Kepler's laws are *p* then *not-p* follows from Newton's laws, and if you now say that Newton's laws can be derived from Kepler's laws you are saying that *not-p* can be derived from *p* even when *p* (Kepler's laws) is not logically false. It is just a matter of elementary logic.

MENDELSON: What is very clear—I think the point you are making is very good—is that the difficulty with the scientists in the past is that they did not write history the way it should have been done, they did it wrong. There is little doubt that the history of science is filled with what we would have to say are illogical derivations and findings of a conclusion from facts which just do not bear out the conclusion.

LAKATOS: You are now making suddenly two points which confuse me, because what you say implies that in the present it is not so and this only happened in the past!

MENDELSON: Where I was granting you a point is—in contemporary logic you are right, but what I am going on to say is that scientists have not necessarily used that because they don't recognize the correlation between *p* and *not-p* as being such.

ROSENFIELD: If I can be allowed to say one word more about Newton—and then we can leave him alone—I maintain that there is a perfect logic in the whole argument, and that this perfect logic has been completely respected by Newton. He started by considering a circular motion of a mass point around a centre of attraction—this is a perfectly possible phenomenon, whether it is a planetary motion is irrelevant. Then from the law of centripetal force and so on you deduce that the attraction must be as 1 over r^2 . Having this law, Newton proceeded somewhat later—as a matter of fact under Hooke's provocation—to ask the more general question what are the possible trajectories that can be derived from this law. This was an enormous feat of mathematical invention because you cannot do that without the calculus of fluxions—and without a differential procedure and a correct law of motion. This problem he solved in 1680. He then found that the most general closed orbit was an ellipse—he also knew that it could be a parabola or an hyperbola according to the initial conditions.

There is a perfect logic in the whole procedure. He first deduced

from a special case what the general law could be and then he investigated by a purely logical, mathematical procedure what the consequences of this law are.

AGASSI: Mr. Chairman, we had in the two talks here this morning a number of examples, each of which can easily take the rest of the morning to debate. And I confess that even though two of the topics which occurred were ones on which I have published myself some attempts at reconstruction, I was not able to understand what the speakers offered well enough to feel qualified to make a detailed discussion like the one that has been attempted here so far. So I will confine myself to generalities.

From Dr. Elkana's discussion of Boltzmann I was only able to work out that Boltzmann's research programme at some stage had two aspects, a methodological aspect and a metaphysical aspect. As for the other examples, all of which (unlike Boltzmann) I am familiar with, I did not—as I have said—understand enough to be able to comment. While Lakatos has convinced me that I must outlive him, because of the way in which he has willfully misrepresented my statements on the historiography of science imputing to me the renunciation of the rationality of science.

Now I cannot really blame the speakers for their failure, since they undertook much too much to be able to execute their programme in any measure of adequacy. We may render that I. B. Cohen published in 1950 a whole book on Franklin and Newton in which he expounds one thesis, which is that the word "Newtonianism" has two meanings—one to refer to an accomplished scientific programme, one to a research programme of extending it further. That was entirely a matter of the metaphysical side. On the methodological side Cohen, it seems to me, tried to do no more than dispel certain popular opinions which might get in his way. Basically he did not take up the methodological issues—but only the metaphysical issue of one metaphysical research programme. And to work out just this required a very long thick book.

In my own work I admittedly presented inductivism and conventionalism as inadequate views, which may be the source of Lakatos' misrepresentation of me, but I at least started with problems—problems relating to truth, problems relating to distortions in books on the history of science, and so on. Now in the whole of today's discussions I have not heard anything about problems or about truth, there was no Prob-

lemstellung of any sort—there was no mention of any problem today. There was one scientific criticism mentioned—Loschmidt's criticism of Boltzmann. And there were a lot of philosophical and epistemological criticisms, which I somehow don't know where they come from and where they go to.

As I see it, the idea of *metaphysical* research programmes is old and venerable and one can discuss the history of views on it. The idea of *methodological* research programmes has a completely different history and how the two go together I for one do not know. When the two are thrown together without any "Problemstellung," it is very hard to follow.

LAKATOS: As far as *problems* are concerned in my papers and in the talk, I think that several problems were discussed in detail and I can mention some which I think Prof. Agassi used to solve poorly and I wonder whether he has changed his mind now. For instance one problem is—how should we learn from experience? This you discussed in your recent paper "Popper on Learning from Experience."

AGASSI: I never discuss questions of "should." And my paper doesn't discuss this problem—I discussed in it the problem how *do* we learn from experience.

LAKATOS: Oh—you are right. They *are* two different problems. But I never thought that you were so empirically minded! Fine, now, according to you, and according to all the papers which you have published recently, we learn only from disconfirming instances. You said that confirming instances and neutral instances are irrelevant—the instances from which we learn are the falsifying disconfirming instances. Now according to my methodology of research programmes we learn only from a few confirming instances. I think here there is a problem and two different solutions. I argued against your solution at quite some length. And this is just one of the problems which we have both discussed and where we seem to have different views.

To take up another point, you say that the difference between metaphysical research programmes and what I call scientific research programmes has not been made clear. Well, metaphysical research programmes is something which comes primarily from Popper. In his *Logic of Scientific Discovery* he says that while we learn from falsifiable hypotheses and counterinstances, the *choice* of our problems may be determined by religious, metaphysical and I don't know what considerations, which

for him are external factors. However, what I say is something very different. According to Popper what we can scientifically investigate is an isolated hypothesis, by confronting it dramatically with a fact. Now I have given detailed arguments why this is impossible. On the other hand if we have two scientific research programmes (and I have explained what I mean by this), then we can say which progresses faster in terms of more excess confirming instances.

In my approach, refutations play no role whatsoever because all our theories are always refuted. That means that for instance if you take Newton's theory and name me any year between 1686 and say 1905, I shall give you the most notable known anomalies of that year. The same applies to special relativity theory—Kaufman's refutation appeared in the same issue as the special relativity itself. These are commonly known facts.

So we have an important problem here. I said that the crucial units of scientific learning are research programmes and we learn from confirming instances—you said that the crucial units are hypotheses and we learn from disconfirming instances.

AGASSI: The allegations about my views are false. I even published before you did the statement that two research programmes can compete and the more fruitful is the one which wins. So don't present this as if it is a view of yours which I disagree with.

LAKATOS: What is 'fruitful'?

AGASSI: I explained what is fruitful in detail—here I will just make a brief comment. You distort my view by confusing my view of *learning from experience* with my view of *learning*, forgetting that in my opinion learning from experience is a rare phenomenon in science.

You misrepresent also Popper's view. Popper says in the *Logic of Scientific Discovery* that we choose the most testable hypotheses and not ones based on metaphysical presuppositions and in my paper on his view I challenged this, saying that since Faraday had metaphysical research programmes he chose testable hypotheses rather than *the most* testable ones. So that Popper's criterion is false.

Now you confuse his view with mine, or you confuse your view with mine!

ELKANA: To answer you directly: I think the problem we are dealing with today, (you may oppose it—but this is what is up for discussion) is *how knowledge grows*.

To this we had an answer, a clearcut answer: I mean the interesting and well-argued thesis of Lakatos, that knowledge grows, according to him, with competing research programmes and a dynamics which he describes. As part of his historical description of how in his opinion knowledge grows, not only was Lakatos claiming that science grows by violent debates between two whole research programmes (and not two theories only)—but moreover he is ready to see in the hard core of every programme a metaphysical view. This is an enormous advance in relation to previous opinions, where the hard core was only a specific theory about this or that. He is ready to look in every case for the hard core which is being defended and he goes on from this to draw a conclusion of how one could write a history of science. He also shows that there are a whole lot of problems which he can explain by this type of explanation and whole lot of problems which, in his opinion, are irrelevant, external, secondary or whatever you wish to call it. And on this basis he separates off completely, at one stage, rational reconstruction of history, which in his opinion should include an improved story as against an historical story.

In my paper and in my example I accepted that knowledge grows by rational discussions between scientific research programmes, but claimed further that at the hard core of these scientific research programmes there is something more than just the hard core metaphysics about how the world is constructed. This further thing is an *image of science*—what people think about science, which influences very deeply what problems should be taken up as problems. Of what Lakatos calls external issues there then remain just some problems which can be called external in a trivial sense, but these are not very interesting any more.

I think this is the issue we are dealing with—this was the clearcut problem stated both by Lakatos and myself, and these were our somewhat different answers.

TOULMIN: I think the way Yehuda Elkana put his point is very good and helpful, and gives us something to debate, but I would like to suggest one amendment to his agenda. For he frames it in terms of the question "How does knowledge grow?," and this only perpetuates a major source of this polemic that we are trying to avoid. The real question which we should have been debating, but have instead been discussing in an inappropriate terminology, is not the question *how knowledge grows* but the question *how understanding improves*.

Let me tell you exactly why I say this. If you frame this whole debate in terms of the question, "How does knowledge grow?", you get trapped in a whole lot of conundrums about "universal propositions," and about whether or not particular experiences are "confirming or refuting instances" of these "universal propositions." That is not, as I understand it, the central issue. The prior issue is the question what *concepts* we use to make sense of our experience, and whether or not these concepts are adequate for the purposes of making sense of the phenomena, the problems, the difficulties, the anomalies and the rest which are now facing us in science. If we only take care to realize that what is there to be talked about is concepts, and improvement of concepts—rather than propositions, and the verification, falsification, probabilification, confirmation and whatnotification of propositions—we shall be able to discuss this matter more fruitfully. In fact, the whole research programme story can be just as well told as a story about concepts and the improvement of our understanding through the modification of concepts.

ROSENFELD: I am afraid that logicians tend to overlook the fact that formal logic has nothing to do with improving understanding of nature, or improving our knowledge. Formal logic is a handy method to check whether we have not made trivial mistakes in a chain of argument. I think Toulmin has an excellent point in insisting on concepts. "Concept" should not be understood as meant here in the formal logical sense—concepts are code words for concrete *representations* of natural phenomena and this property is irreducible to formal logic. It cannot be formalized. And so Newton's procedure cannot be formalized—it is an inference from an analysis of natural phenomena.

LAKATOS: In answer to this talk on 'concepts', I would even say that there are *three* traditions in evaluating scientific results. The difference is in the choice of the unit of appraisal—the choice of what kind of thing is to be good or bad—namely whether this unit of appraisal is to be the concept, the proposition or the cluster of propositions.

These three approaches are clearly different. For instance Goodman belongs to the *first* tradition because Goodman talks about good and bad *predicates*—projectile predicates being good and non-projectile predicates being bad. The non-projectile predicates are not scientific according to Goodman. An example of the *second* tradition is falsifiability or verifiability, since these apply to single propositions. While simplicity

and coherence apply to clusters of propositions and so are the kind of criteria used by people belonging to the *third* tradition, such as Duhem.

Now I certainly think that the third is better than the second and the second better than the first for logical reasons that I can state in a sentence, namely, the definition of a concept is always given either in an explicit definition or in an implicit definition, and therefore by a proposition in the former case or by a cluster of propositions in the latter case. Therefore concepts are secondary to propositions, and propositions are secondary to clusters of propositions inasmuch as explicit definitions use terms that are defined only implicitly.

TOULMIN: Do let us discuss Yehuda's question about science, instead of dragging in Frege's treatment of concepts which was developed to deal with pure mathematics. What Lakatos says may be true in mathematics, but we are not talking about pure mathematics; we are talking about the improvement of scientific understanding, and this is the matter which we must go on talking about rather than getting distracted in this way.

MENDELSON: I think today's papers especially bring up one issue that has been interesting lately, and that is: in what way does philosophy of science inform the writing of the history of science? It is important because to my mind the historian has too often accepted the definition and the bounds of science given to him by scientists, indeed often those given by scientists contemporary with him. He has been stuck with what these scientists give him, and here the philosophy of science can give new leads.

Starting from here I can see two clusters of questions that are important. Firstly—what did the scientist do? We look at his notebooks and start analyzing—is it logically consistent or did it introduce errors which he did not perceive? This opens up all the questions that Lakatos' philosophy of research programmes leads to.

Secondly—what did contemporaries think a man had done? This may be different from what he did and also from what he thought he did. Here I would agree with Elkana in disliking the dichotomy between internal and external. It is non-functional, it may provide somewhat neater analysis but it excludes important questions. For example, we are interested in knowing what approaches in research a scientist takes, and here Elkana gives us a good direction. We are also interested in knowing what is involved in the manner in which a scientist reports what he is

saying and the manner in which he publishes it, because we know fully that almost every scientist involves himself in retroactive falsification when he writes.

Then there are questions about the constancy or change in what is going on. Take the working environment of a man of science—truly this changes. The 17th century amateur is in a vastly different environment than that of a modern scientist integrated into an active research community and say financed heavily by the government, before which he has to justify his activities.

ELKANA: To stress one point, which I put in by the way, but which I would like to come to much more forcefully now: there are degrees in this whole story of internal and external. Take the example of Mendelian genetics—it is certainly self-evident that if you take all the Mendelian geneticists in Russia and kill them off then there won't be scientists to do Mendelian genetics. In a very trivial sense this is external but you can get by degrees to a different evaluation.

Even with the content of the image of science it is not very clear where it all belongs, if you insist on a dichotomy. You can talk for example about ideologies and the personal moral code of a given scientist, and somebody who insists on the dichotomy could say that these are clearly external. But you can get slowly nearer to something that is supposed to be internal—for example, let us say that somebody does not accept experiments which are not repeatable; is it because of his moral code about the truth, or is it because he has some very rational theory on the behavior of Nature? Here it already gets fuzzy whether this is external or internal.

But what I want to stress in drawing attention to the image of science is its rational part—which on no account should we lose sight of—that is, what do people in some time at some place think very rationally that it belongs to science to do? And this is a changing thing—they often simply eliminate even brilliant ideas systematically, because they no longer think that this is legitimate. If Boltzmann at the time when he thought the task of science was to develop competing mental pictures, as many as possible and independent of what is seen in the outside world, had suddenly developed a mental picture dependent on the visible in the style of his older materialistic image, i.e. in the realm of realistic atomism—he would simply have eliminated it. He in fact *had* such ideas and did not allow them

to develop. This was a rational decision about what he thought science was supposed to do and is on any account part of the rational history, and therefore of the internal history if we are to maintain Lakatos' equation of rational and internal.

I stress this because I do accept this philosopher's problem that we are groping after, namely the theory of rationality, which is a highly important problem irrespective of whether we are historians of science or not.

AGASSI: I want to say Yehuda Elkana did answer now, in the discussion, my challenge and he did now present a problem, both in his claim that he is after a theory of the growth of knowledge (i.e. how does knowledge grow?) and in his further claim that the scientific act is a rational act of that growth (i.e. is scientific growth the growth of rationality?). Growth, he says, is done rationally and the question then, I understand him to say, is how is it done rationally. Moreover, I understand Yehuda to say that he criticizes Lakatos, because his and Lakatos' ideas of what is the hard core of any such programme are different. However, since I don't think Lakatos actually said what is the hard core, I do not see how Yehuda can justly claim to have criticized him in this respect—and this I can say even without claiming to have understood him.

I apologize that I lost my temper and was outraged by the willful and obvious misrepresentations by Lakatos of my view. I think his misrepresentations are his business and not mine, and as I think they are willful I should simply withdraw from the discussion which I entered by mistake. Again, I apologize.

TOULMIN: Everett Mendelsohn's helpful exposition brings up one important point: the fact that scientists do go through the motions of saying things about what they do in science, which are not necessarily to be taken at their face value. Einstein himself, you remember, said, "If you want to find out what methods theoretical physicists use, don't listen to their words, fix your attention on their deeds."

I was very happy that Prof. Rosenfeld mentioned the notion of "representation," which has unaccountably been missing in these discussions and which seems to me to play a crucial part, if you are going to understand what physical concepts are, as opposed to concepts in pure mathematics. What it did bring out for me was the source of a confusion I still find in Lakatos' whole research programme account: namely, over what

he means by saying that inductivism, falsificationism, conventionalism and all the rest are accounts of the scientist's code of morality—by which I understand him to mean "rational morality," i.e. the code by which a scientist actually performs or talks about, his argumentative procedures. As I see it, inductivism, falsificationism, conventionalism and so on are slogans which the scientist picks up from the philosophy of science, and feels it incumbent on him to talk about as having a bearing on what he is doing: that they are, indeed, part of his code of "morality," in precisely the sense in which Kierkegaard rightly condemned morality—that is, they are an expression of the "morality" to which the scientist pays conventional lip-service.

LAKATOS: I just would like to say in answer to this point that I do indeed distinguish between the false awareness of rationality in scientists and the actual rationality they pursue. Of course the most interesting historical questions are the interactions of the two—the actual standards and the believed standards.

LOWE: There is an interesting comparison to be made between the variety of philosophies of science outlined by Lakatos and the situation in modern formal logic. The interest of the comparison is that it seems that Lakatos has given us the beginning of a satisfactory solution to an old problem in the philosophy of science, namely, when are we entitled to describe the activities of a given scientist in terms of *our* philosophy of science if in fact the scientist concerned explicitly adopts a philosophy of science that differs from ours? The comments of Bechler in this discussion were of course partly prompted by this general question.

There are in modern logic actually rival theories of logic. Besides the classical logic used by most mathematicians, there is also intuitionist logic, used by a small but nonetheless quite widely respected school of mathematicians, and one might also mention the negationless logic of Griss. Now it happens that in *metalogic*—the study of the foundations of logic—it has been found fruitful to study all kinds of logic using classical logic. For instance, many logicians who are interested in intuitionist mathematics will use a classically logical metalanguage in their study of it.

Turning to the philosophy of science: according to Professor Lakatos, Popper has formulated a version of conventionalism which cannot be questioned logically or epistemologically. It is thus conceivable that one

might have rival schools of science—Lakatosian scientists and conventionalist scientists—both pursuing perfectly coherent, although different, kinds of enquiry. We can even imagine that some follower of Carnap, or say Dorling (to mention a different approach), may yet construct a coherent version of inductivism, so that there could be yet another kind of science.

Thus far the situation in science and its philosophy is similar to that in mathematics and metalogic. The obvious question is: is it possible that there is some global philosophy of science that can play a role comparable to classical logic in metalogic?

Now the principle underlying the global role of classical logic appears to be that the other kinds of logic are, in a certain sense, weakened versions of it. By adding certain extra assumptions to negationless logic we can get intuitionist logic, and by adding further assumptions to intuitionist logic we can get classical logic. What analogous principle could there be in the philosophy of science? It seems to me that Professor Lakatos' analysis of external and internal problems provides one. For he claims that all of the internal issues of inductivism are also internal issues of conventionalism, but that also some of the issues that are external to inductivism are internal to conventionalism. Also he claims that the internal issues of conventionalism are internal to his own philosophy of science, together with some issues external to conventionalism. These relations even represent, in Lakatos' account, a historically developing shift of the boundary between internal and external issues.

Thus we may propose the following rule: we are permitted to apply to a scientist a philosophy of science that differs from his own explicitly adopted one, provided that in our philosophy of science all of *his* internal issues are internal, perhaps together with some of the issues external to his philosophy of science. (Or, inasmuch as we are dealing with scientists' beliefs rather than mathematical systems, we may weaken the requirement to demanding that *virtually* all of his internal issues remain internal.)

Looking somewhat further, if we do not assume that Lakatos' philosophy of science is the last word on the subject, it is possible to foresee three logically (or algebraically) distinct eventual situations. We may succeed in eliminating external issues altogether. Or we may reach some unique general barrier, such that although certain issues are still external,

it is impossible to find a way of transforming them into internal issues of a more comprehensive philosophy of science. (In either of these first two cases, we eventually construct a unique global philosophy of science.) Or we may discover that for every philosophy of science there exists a yet more comprehensive one, or at least that there is no unique maximal philosophy of science.

If I understand Yehuda Elkana's comments correctly, he claims in effect that with Lakatos we have already reached the second of the three possibilities that I have described. For he claims that although some external issues remain for philosophy of science, they are not such as could be made internal to a more comprehensive philosophy of science than that offered by Lakatos.

LAKATOS: I do agree roughly with you although perhaps not in the details, but of course you seem to agree with me as against Yehuda, that there is a point to this internal-external demarcation.

LOWE: I think, as indicated, that his own comments on your paper presuppose the making of the internal-external distinction. However, he may be nonetheless right in claiming that it will not be important any longer, now that we have reached the position of having the philosophy of science that you advocate—it is a question of whether the boundary can be shifted any further.

LAKATOS: We shift the boundary as we get a better methodology.

LOWE: And even when people say that Newton himself follows a different methodology of science, it may still be relevant to apply a much wider one—provided that the rule I suggested is satisfied.

LAKATOS: I only would like to say that I think that there is always a possibility of creative expansion of any given rationality series through the rationality of the individual physicist. This is where I would give the primacy to the physicist's creativity.

I wonder Mr. Chairman, could I come back to Yehuda Elkana's central criticism about internal-external? All that I meant by the internal-external demarcation is something very simple. For instance, suppose someone accepts falsificationism and further says that the Michelson-Morley experiment refuted the ether theory. If he then looks at Lorentz and finds him still working on the ether theory, the falsificationist has to say that this is not a rational activity—so he needs to explain this by some other kind of factor. It can't be explained by the rational standards

of falsificationism, but it can be perhaps explained by psychopathology, by the fact that he was senile, or by claiming that that is the way the establishment behaves. This is what I call giving an external explanation, as opposed to explanations supplied by the given theory of rationality, which I term internal.

Now, all that I say is that any methodology introduces the same distinction into the description of the history of science. This follows at once if we agree that there is no wholly neutral observation of scientific activity, but that somehow the theoretical terms of historiography peter down to the descriptive level so that the methodological ideas appear as descriptive terms in the skeleton of history, such as when historians talk about crucial experiments or about falsifications. Now behind these "descriptive" terms there are concealed methodological theories and this is what I really would like to stress: each methodology determines a different skeleton of historical description.

ELKANA: I think it is a very nice, sharp, clearcut disagreement, which is the nice point about it. Your examples prove exactly what I don't want to take. Certainly there can be cases of psychopathology, and if it is indeed a case of psychopathology—a clearcut, proven, historical case, then you can call it as external as you wish but then already I am not interested in it in the history of science—then I couldn't care less. But in most cases what is happening is that it is not—it is a rational decision on the part of these people, for some or other legitimate reason coming from their hard-core metaphysics, or from their image of science (which is at least as legitimate as the other), to stick to their own problem and continue it.

But by making your kind of dichotomy, we are just encouraging the same mistakes which just now you wanted to attack in the most instrumentalist-minded physicists. That is, to disclaim programmes, not to pay any attention, for example, to what great scientists in a previous age said, because we can't incorporate it in today's science. This is exactly what has been happening with Bohr's suggestion, that people who understand complementarity will do something with his idea (which is pooh-poohed nowadays) both in psychology and in biology—it is not taken care of because he was "proved" wrong by exactly this kind of argument.

For the same reason Kant's posthumous unfinished work was con-

sidered a book of senility while it is a very rational and beautiful example of a very systematic problem-shift by which Kant actually leaves behind the Newtonian frame in which he was thinking and tries to switch over to a somewhat different framework.

LAKATOS: May I ask you—are you denying that where we make the demarcation between rational and senile depends on whether we are inductivist or falsificationist, or belong to the research programme school, or whatever you want?

ELKANA: Yes, I am. Either somebody is sick or not, and if he is sick we are not interested any more in this context.

MURDOCH: Can we come back to the general level? At the beginning of his remarks, Everett directed us to find out, as historians, how the philosophy of science can inform us. Perhaps this is too elegant a way to put it—let us ask instead what use can be made of it. Because I think that this is what is really involved.

In listening this morning, it seemed to me that something like the following has happened (and I want to add one further possible direction to it): Yehuda is very pleased because Lakatos has expanded the base or core of research programs from specific theories to include the metaphysical as well. Then Everett takes over and expands the whole even further. I don't want to disagree with Everett, but what he seems to be implying is that as historians we should have some framework or other, some pattern or other, some model or other, that is broad enough to encompass all of the variables we shall encounter. This is one direction we might move in looking for a marriage with the philosophy of science. But there is another as well, one that can be followed simultaneously with that advocated by Everett.

That is: as historians, our use of the philosophy of science might also be to allow ourselves to make the core of the research program or models *not* broad and all-encompassing, but as narrow, as particular, as restricted, as idiosyncratic as possible, while still trying to use them in doing history. Plug them into history and observe what happens. The real value of them, it seems to me, is not that they work, but that they almost never work *entirely*. In other words, if a given model or research program doesn't fit the historical sources exactly, then use the ill-fittingness of it to tell you something about the particular history you are undertaking. For instance, take model X—try it out on Einstein, try it

out on Copernicus. In order to make it fit Copernicus, you may have to fudge it this way and make these qualifications; in order to make it fit Einstein, you may have to qualify or change it in another way. What is important to me then is not the model X itself (although I would grant that such might be the concern of the philosopher of science), but the list of qualifications one has made for Copernicus and the list of qualifications one has for Einstein. It is from just such a roster of accommodations made that we may learn things of historical value. And it is, therefore, this kind of use of the philosophy of science that needs also to be kept in mind.

ELKANA: As to this, John,—allow me to add a psychological remark which has never been proven, but my whole attitude to these problems stems from it: I don't think that any contribution to science was ever done by a scientist who at the time of his contribution was not a realist. In other words, I don't believe that of an instrumentalistic attitude any contribution to science can come.

The same applies, in my opinion, to historiographic methodology. I cannot imagine doing good history of science by this kind of completely instrumentalistic attitude to history or methodology. I think the result will be a mosaic without any meaning.

MURDOCH: Yehuda, one could, I think, take some exception to your faith in the necessity of realism for contributions in science and be even more skeptical over your easy move from this to a similar stance for history and historical methodology; but let these points pass. What is more pertinent to what I was trying to urge is that your point still has to do with the nature of some all-embracing philosophical model that is to be used by historians, while mine has rather to do with a quite different use of such models, or even isolated conceptions, as tools in particular instances. The use you are implying naturally does have a great concern for the fittingness of the model with how science may actually have progressed; mine need not. That is to say, what is important to the additional use I would advocate is that, regardless of fit, such and such a tool furnished by the philosopher of science (or by anyone else, for that matter) does illuminate, does bring to the fore, an historical point of value that would not otherwise have surfaced. The fact that history *has* seen this happen from time to time in its writing would imply, then, at least *this* merit for an "instrumentalistic" point of view.

NEWTON'S THEORY VS. KEPLER'S THEORY
AND GALILEO'S THEORY:

An example of a difference between a philosophical
and a historical analysis of science*

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— 1 —

The philosopher's analysis of science often leads him to conclusions that are somewhat at variance with historical reconstructions based on a full and careful examination of documentary evidence. This difference between the methods of philosophers and historians is particularly striking in a study of the relationship of Newton's principles of dynamics and celestial mechanics to the theories and laws of Galileo and Kepler. In this case, however, philosophers of science have directed our attention to the uncritically accepted supposition of a dependence of Newton's principles of celestial mechanics on Kepler's law of planetary motion, and—at least by implication—they have challenged an over-simplified generalization about Newton's principles having been a 'synthesis' of Kepler's and Galileo's laws.

The issue can be stated quite simply in the following terms. It is often alleged by historians and scientists that Newton's law of planetary force was 'derived', or 'deduced', or 'inferred', from Kepler's laws, and in particular from Kepler's third (or harmonic) law of planetary motion. On analysis, however, such a 'derivation', 'deduction', or 'inference' appears to be logically impossible, since Kepler's laws (notably the third law) and Newton's principles are mutually inconsistent.

In the major philosophical discussions of this issue (by Duhem, by

*[Editor's remark: Unfortunately Prof. Cohen was prevented from participation in the symposium, and I am indeed very grateful for this contribution to the volume. In view of the lively discussion of this very issue provoked by Prof. Lakatos' paper, it was felt that the significance of Prof. Cohen's paper would be most appreciated if it immediately followed that discussion.]

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Popper, and by Feyerabend), no information is given as to the original source of the criticized statement of an alleged logical dependence of Newton's principles on Kepler's laws.¹ In my view this is a serious omission, because the declaration concerning the inverse-square law of planetary force having been 'deduced' from Kepler's third law proves not to have been an invention of post-Newtonian historians or scientists who did not have a clear grasp of logical or of physical principles; rather, it was Newton himself who stated (and thus evidently believed) that his own work had—to an important degree—grown logically out of Kepler's.

Whatever general philosophical principles concerning science may be generated by the philosophers' analysis of Newton's principles in relation to Kepler's laws, the historian must deal with a set of quite different inquiries. First, he must ask himself whether or not Newton was being careless in referring to his alleged deduction. If we believe that Newton meant what he said, and that he said what he meant, then we are led to ask whether Newton failed to understand the logical structure of his own system of celestial dynamics, and so was misled into thinking his law of planetary force could be logically 'deduced' from Kepler's third law.² A related question is whether Newton's historical recollections con-

¹Such a statement may be found, e.g., in Stephen F. Mason: *Main Currents of Scientific Thought: a history of the sciences*, Henry Schuman, New York, 1953 (and later printings in English and in translation), p. 157: "It seems that he [Newton] deduced the law of centripetal force and with it derived from Kepler's third law of planetary motion the inverse square law of gravitational force." Mary B. Hesse, whose full text shows that she appreciates how Kepler's laws cannot be accepted without modification in Newtonian physics, makes statements that appear to indicate that in the "theory of gravitation . . . the phenomena of Kepler's laws . . . can be deduced"; see *Forces and Fields*, Thomas Nelson and Sons, London, Edinburgh, . . . 1961, p. 147. Again, pp. 147-148: ". . . the law [of universal gravitation—?] was certainly known to or guessed by Hooke and Newton before its deduction from Kepler's laws. . . ."

²Possibly, it is the word 'deduced' which should occupy our attention. But whatever form of deducing or inferring Newton (or his latter commentators) may have had in mind, neither process can be valid if the premise and conclusion are logically mutually inconsistent. See, on this point, my presentation in Chapter III of the Wiles Lectures, *Transformations of Scientific Ideas*, Cambridge University Press [*in preparation*].

William Kneale: *Probability and Induction*, Clarendon Press, Oxford, 1949 (reprinted 1952, 1963), pp. 98 ff., discusses Newton's "curious way of deducing propositions from phenomena." On this topic, also, I. B. Cohen: "History and the Philosopher of Science", to appear in *The Structure of Scientific Theories*, Frederick Suppe, ed. (scheduled for publication by the University of Illinois Press in 1973).

cerning his discoveries may possibly indicate only that he had made the great leap forward by a temporary lapse in the rigour of logic in deduction or inference; are we possibly to assume, therefore, that a discovery so profound as Newton's could have been the result of a by-passing of ordinary logic, or even the result of a logical error? Clearly, we are at the brink of the psychology of discovery or invention, and cannot avoid the question of the 'non-logic' of discovery. But the issue at hand goes even beyond such questions about Newton himself, and the process of scientific innovation. The philosophical analysis calls into doubt the historical phenomenon known as the 'Newtonian synthesis', for how could there have been a synthesis which contradicts the principal ingredients being synthesized?³

Let Newton speak for himself. In an often-quoted autobiographical statement, Isaac Newton recounted how he had made his discoveries:

And the same year [i.e., the year 1666] I began to think of gravity extending to the orb of the Moon, & [having deduced *del.*] having found out how to estimate the force with which *< a >* globe revolving within a sphere presses the surface of the sphere: from Keplers Rule of the periodical times of the Planets being in sesquialterate [i.e., 3/2] proportion of their distances from the centers of their Orbs, I deduced that the forces which keep the Planets in their Orbs must *< be >* reciprocally as the squares of their distances from the centers about which they revolve: & thereby compared the force requisite to keep the Moon in her Orb with the force of gravity at the surface of the earth, and found them answer pretty nearly.

According to the biographical information assembled by John Conduitt,

In 1684 Dr [Edmond] Halley came to visit him at Cambridge. After they had been some time together, the Doctor asked him what he thought the Curve would be that would be described by the Planets, supposing the force of attraction towards the Sun to be reciprocal to the square of their distance from it. Sir Isaac replied immediately that it would be

³I do not know who first used the expression 'Newtonian synthesis'. In 1922, however, A. N. Whitehead referred to the age of Newton under the rubric of "The first physical synthesis".

an Ellipsis. The Doctor struck with joy & amazement asked him how he knew it. Why, saith he, I have calculated it. . . .

Newton had, furthermore, in 1673,

. . . laid down this proposition, that the areas described in equal times were equal, which though assumed by Kepler was not by him demonstrated, of which demonstration the first glory is due to Sir Isaac.⁴

These quotations give the unambiguous impression that Newton believed: (1) that he had "deduced" the inverse-square law from Kepler's third law plus the rule for centripetal forces; (2) that he had "demonstrated" the law of areas; (3) that he had "calculated" the elliptical shape of the planetary orbits from the inverse-square law of force (in fact, using the law of areas).

The issue under discussion here is how we may possibly reconcile such unambiguous statements of Newton's with the interpretation given by Karl Popper in a celebrated paper on "The aim of science". According to Popper:

It is often said that Newton's dynamics can be induced from Galileo's and Kepler's laws, and it has even been asserted that it can be strictly deduced from them. But this is not so; from a logical point of view, Newton's theory, strictly speaking, contradicts both Galileo's and Kepler's. . . .

Thus Popper concludes,

It is therefore impossible that Newton's theory can be ob-

⁴The first of these statements comes from a manuscript in the University Library, Cambridge (England), MS Add. 3968, § 41, f. 85; it has often been published in one form or another. In this transcription I have spelled out Newton's abbreviations (such as *y^e* for 'the' and *w^{ch}* for 'which'), I have left out all but one of Newton's cancelled expressions, and I have ignored his repetition of the phrase "from Keplers rule". The second two extracts are taken from John Conduitt's "Memorandum relating to Sr Isaac Newton given me by M^r Abraham Demoivre in Nov^r 1727", a manuscript now in private hands. In my transcription, I have again spelled out abbreviations, and in this one I have divided the text into sentences for clarity. Both of these documents, together with other similar ones, may be found in Supplement I to I. B. Cohen: *Introduction to Newton's 'Principia'*, Cambridge University Press, Cambridge (Eng.), and Harvard University Press, Cambridge (Mass.), 1971.

tained from these theories by a process of either deduction or of induction. For neither a deductive nor an inductive inference can ever lead from consistent premises to a conclusion which formally contradicts these premises.⁵

Accordingly, if Popper's analysis is correct, Newton must have erred in assuming that he could "deduce"⁶ his own "theory" (to use Popper's expression) from Kepler's.⁷

At the outset, let me make it absolutely clear that my presentation is a supplement to Popper's, and that it even reinforces the value of Popper's profound insights into the nature of science. If, therefore, I conclude that certain aspects of his presentation of Newton in relation to Galileo and Kepler do not go deep enough and hence appear misleading, this should not be interpreted to mean that I esteem any the less the remainder of his great paper on "The aim of science". At the end of the present study, I shall indicate why I consider Popper's discussion extremely important for every historian of scientific ideas, even though I find that Popper has not completely fulfilled the expectations of a critical historian of science in his treatment of Newton and Galileo, and of Newton and Kepler.⁸

⁵Karl Popper: "The Aim of Science", *Ratio*, 1 (1957), 24–35, esp. pp. 29–30.

⁶Newton tended to use the word 'deduce' for ordinary logical deduction, for the process of mathematical derivation, and also—in a rather loose and general fashion—for any series of logical or logical-mathematical steps which we might call today 'deduction', 'inference', or even 'induction'. Further information on Newton's usage may be found in I. B. Cohen: *Transformations of Scientific Ideas* (cited in note 2 *supra*), ch. 3.

⁷Popper makes a similar charge concerning Newton and Galileo's "theory"; but I shall deal almost exclusively with the problem of Newton and Kepler's "theory". On Galileo, see note 38 below.

⁸This aspect of Popper's paper illustrates a difference which often occurs between the treatment of scientific ideas by the philosopher and the historian. Obviously, we should not expect that Popper would do the job of the historian for him. The historian should therefore be grateful that Popper has given him important insights, which may help to make him a better historian. On the other hand, the historian has a right to believe that if a certain issue between Galileo or Kepler and Newton is central to the discussion by a philosopher of science, then that philosopher might reasonably be expected to find out what Galileo, Newton, and Kepler actually had to say on the question or questions being discussed, and especially if the text in which this information may be found should be readily available. This point of view is merely another way of saying that a historian of science expects philosophers and other scholars to treat historical materials with the same respect that he does.

— 2 —

In dealing with "Newton's theory" and "Kepler's laws", Popper does not concern himself with the many laws about planets and their satellites which were announced by Kepler, and which are to be found in Kepler's *Epitome astronomiae Copernicanae*,⁹ nor even with all three of the laws which astronomers, physicists, and mathematicians have selected from this collection to designate as Kepler's 'three' laws of planetary motion. Nor, despite his references to "Kepler's theory", does Popper even mention Kepler's own 'theory' of motion. Popper limits his discussion to one law, Kepler's third law, or harmonic law, which we have seen Newton refer to as Kepler's "Rule of the periodical times of the Planets being in a sesquialterate proportion of their distances from the centers of their Orbs".

"Kepler's laws", according to Popper, are "strictly invalid" (since they are "only approximately valid") "in Newton's theory", once we take cognizance of the "mutual attraction between the planets". But the "contradictions between the two theories" are "more fundamental" than anything so "obvious". For even if "we neglect the mutual attraction between the planets", he writes,

Kepler's third law, considered from the point of view of Newton's dynamics, cannot be more than an approximation which is applicable to a very special case: to planets whose masses are equal or, if unequal, negligible as compared with the sun. Since it does not even approximately hold for two planets one of which is very light while the other is very heavy, it is clear that Kepler's third law contradicts Newton's theory in precisely the same sense as Galileo's.¹⁰

To show the contradiction, Popper displays the familiar "law" which "Newton's theory yields for a two-body system", and which "astronomers often call 'Kepler's law' since it is closely related to Kepler's third law."

⁹In this work, summarizing all of his achievements, and stating the principles in which he still believed, Kepler set forth not only what we call today the 'three' laws of planetary motion, but also the law of the regular solids (according to which the planets' orbits are constructed on spheres, nested one within the other, in a special way: each sphere is inscribed in and circumscribes one of the five regular solids). There are also other laws of planetary speed, and so on.

¹⁰*Op. cit.* above (notes), p. 32.

Let m_0 be the mass of one body (say, the Sun) and m_1 the mass of the other body (a planet). Then, "choosing appropriate units of measurement, we can derive from Newton's theory

$$a^3/T^2 = m_0 + m_1 \quad (1)$$

where a is the mean distance between the two bodies, and T the time of a full revolution." This law (1) is then contrasted with Kepler's own law:

$$a^3/T^2 = \text{constant} \quad (2)$$

where we have "the same constant for *all* planets of the solar system." Relation (2) can therefore be valid only if all planets have the same mass (m_1), which is false, or if all masses are so small with respect to the mass of the Sun that they may be taken as zero. The latter may be "quite a good approximation from the point of view of Newton's theory", Popper concludes, but it "is not only strictly speaking false, but unrealizable from the point of view of Newton's theory", since a "body with zero mass would no longer obey Newton's laws of motion." Thus, wholly apart from the actions of one planet upon another, "Kepler's third law (2) contradicts Newton's theory which yields (1)."

— 3 —

There can be no quarrel with Popper's analysis of the bare fact that in the system of celestial mechanics which we call 'Newtonian', the simple relation

$$a^3/T^2 = \text{constant} \quad (2)$$

cannot hold.¹¹ Obviously, then, if Newton ever thought otherwise, he must have been mistaken. But did he? How shall we interpret such statements of Newton's as those quoted above (in § 1)?

¹¹Others have discussed the relation of Kepler's third law to Newton's dynamics, notably Pierre Duhem: *The Aim and Structure of Physical Theory*, Philip Wiener tr., Princeton University Press, Princeton, 1954; reprinted by the Atheneum Press, New York, 1962, pt. II, ch. 6, § 4, pp. 190-195. Duhem's analysis, to which Popper refers, deals only with Kepler, and not with Galileo. Duhem proceeds differently, and draws different conclusions from his own study of this question. It is of interest to the historian of scientific ideas to observe that Duhem, at this point writing as a philosopher of science, rather than in his other capacity as a historian of science, has failed to make a thorough reading of Newton's *Principia*, and so presents an erroneous impression of Newton.

In his article, Popper uses the expression, "Newton's theory", which seems to imply that he has in mind a particular 'theory' which was expounded by Isaac Newton in his *Principia* or other writings, and not some post-Newtonian formulation that may happen to incorporate some of Newton's discoveries: for instance, the law of universal gravitation. Moreover, Popper distinctly gives the impression that he has in mind the 'theory' of the historical Isaac Newton as expounded by Newton in the *Principia*, since in a footnote he refers to the *Principia* by chapter and verse.¹² That Popper is presenting what historians would designate as 'Newton's theory', and not a more general 'Newtonian theory',¹³ seems to be further indicated by his careful differentiation of a genuine law stated by a scientist and a later law named after him. We have just seen how Popper contrasts what he calls the "law which astronomers often call 'Kepler's law'" (or a "so-called 'Kepler's law'") and "Kepler's own third law" (the one which has proved to be inconsistent with "Newton's theory"). In such a context, the reader cannot but suppose that, similarly, in the case of "Newton's theory", Popper must mean what I may call 'Newton's own theory' rather than a 'so-called' form of 'Newton's theory'. Furthermore, only a page or so earlier, in a discussion of "Galileo's theory" and "Newton's theory", there has been reproduced in facsimile an illustration taken from an early edition of a treatise of Newton's, the so-called "System of the world".¹⁴

¹²The propositions to which Popper refers seem to be picked somewhat by chance, since they do not include the early propositions in which Newton shows the use he has made of Kepler's laws. Popper concludes, in the note mentioning these propositions by number, that his own analysis may even be "implicit" in Newton's *Principia*. As we shall see, however, it is not implicit at all, but clearly and absolutely explicit.

¹³The eponymous assignment of names in scientific practice is hardly the result of careful historical inquiry and critical historical judgment. The concept underlying what is called 'Galileo's principle of inertia' in the theory of relativity turns out to be not only post-Galilean, but post-Newtonian, and even may have post-Einsteinian overtones. Similarly, the phrase 'Newtonian theory' tends to be used to denote the whole body of classical rational dynamics, and may include such post-Newtonian doctrines as the conservation of energy. But I believe that few readers would interpret 'Newton's theory' as anything other than the theory expounded by Isaac Newton himself.

¹⁴This is a work published posthumously, first in an English translation, and second in an altered form of the Latin original. A critical evaluation of these texts may be found in my Introduction to the facsimile reprint, Isaac Newton: *A Treatise of the System of the World*, Dawsons of Pall Mall, London, 1969, and in my article, "Newton's *System of the World*: some textual and bibliographical notes",

However valuable Popper's analysis may be for the working historian of scientific ideas, the potentialities of his insights are somewhat masked by his presentation, which overly concentrates on the logical relations (such as consistency, and hence the *possibilities* of deduction or induction) between Newton's dynamics and both Galileo's laws of free fall and Kepler's third law of planetary motion. In the light of Popper's explicit statement that the "task of science" is "to find *satisfactory explanations*", and his ancillary expression of belief that to understand an explanation requires "the idea of discovery",¹⁵ the reader might have supposed that attention could more profitably have been directed to the stages whereby Newton himself dealt first with the validity of Kepler's laws and then with their invalidity: thus exhibiting a progression from a level of dynamics on which Kepler's laws may hold to a more advanced or complex level on which these laws must be modified.

The following discussion may, in a sense, be read as an attempt to illustrate further and to complete the doctrine Popper has advocated. In his article Popper has explored what Whitehead once referred to as the "logic of the discovered", but he has not really given any attention—save by implication—to the "logic of discovery".¹⁶ As a result, a reader is apt to be misled by the lack of distinction between the logical and historical points of view. In discussing the examples given by Popper, therefore, I shall attempt to make explicit the differences between logical and historical analysis.

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In presenting Newton's own treatment of Kepler's law (in contrast to a 'Newtonian' confrontation)¹⁷ I shall first direct attention to the ex-

Physis, 11 (1969), 152–166. See also Stanislaus J. Dundon: "Newton's 'mathematical way'" in the *De Mundi Systemate*, *Physis*, 11 (1969), 195–204.

The diagram, showing various paths of long-range projectiles fired from a mountain, is famous because it illustrates Newton's understanding of the way in which a projectile could become a satellite if given a sufficient initial velocity. The artist has made this illustration far more realistic than the original, adding continents to Newton's own representation of the 'mathematical' outline of a sphere.

¹⁵*Op. cit.*, pp. 24, 34.

¹⁶These expressions, the "logic of the discovered" and "logic of discovery", are used in an effective way, perhaps an entirely original way, in Alfred North Whitehead: *The Aims of Education, and other essays*, The Macmillan Co., New York, 1929, p. 80.

¹⁷See § 3 *supra*.

position found in the *Principia*.¹⁸ Newton begins with Definitions, followed by Axioms or Laws of Motion, and certain ancillary corollaries and scholiums. Then, Book I has an opening mathematical section on limits, on what Newton calls the method of "first and last ratios". Next there is an introduction of 'Keplerian motion' (although Newton does not use this name¹⁹), positing (Sections II and III) a center of force and a point-mass. Newton does not use the expression 'point-mass' (or 'mass-point'), but writes of a "body". This "body", however, has no physical dimensions or shape, which is the reason I have called it a 'point-mass'. Under these limiting circumstances, Newton does not have to concern himself about the meaning of 'distance' from the "body" to the center of force.

First of all, Newton shows that a force directed toward such an immovable center is both a necessary and a sufficient condition for the law of areas (Props. I and II). In Prop. III, Newton deals with the case of the area-law applying to "the centre of another body howsoever moved".²⁰

¹⁸I shall concern myself here exclusively with the final statement that occurred in the third authorized edition of Newton's *Principia* (1726), and I shall not discuss the evolution of this text through the two earlier editions (1687, 1713). All of the differences among the several editions, and the manuscript sent to the printer, may be found in the edition of the *Principia* with variant readings, assembled and edited by Alexandre Koyré, I. B. Cohen, and Anne Whitman, 2 vols., Cambridge University Press, Cambridge (Eng.), and Harvard University Press, Cambridge (Mass.), 1972.

¹⁹In the *Principia*, Newton refers to Kepler as author of the third law of planetary motion, not being aware that Kepler had found a similar law for the system of Jupiter's satellites (which may be found in Kepler's *Epitome Astronomiae Copernicanae*). Nowhere in the *Principia* does Newton mention Kepler's name in relation to either the law of areas or the law of elliptical orbits. We shall see a possible reason for this below. It may be observed, however, that in some earlier tracts, written just before Newton composed the *Principia*, and in the autobiographical statements which he wrote about the time of the second edition (1713), he shows himself to have been perfectly aware of Kepler's contribution to the first and second laws of planetary motion. In the *Principia*, Kepler also appears as an observer of astronomical phenomena, notably comets.

The problem discussed in Section VI of Book I is to find the place of a planet at some given time after it has been in any given place in its orbit. This problem, as Newton observes, cannot be "universally" solved "by means of equations of any number of finite terms"; and, therefore, some method of approximation must be used. Today this is known as Kepler's problem.

²⁰Quotations from Newton's *Principia* are based on Andrew Motte's English version, published in two volumes in London in 1729, and reprinted in facsimile by Dawsons of Pall Mall, London, 1968. I have, for the ease of the reader, spelled out certain words which were abbreviated, and I have notably put in the *e* for the apostrophe in such examples of the past tense as 'discover'd' and used some more modern expressions (as '3/2 power').

Prop. IV is devoted to the centripetal forces of bodies that move uniformly in circles, an obvious and elementary example of area conservation. Corol. 6 (of this Prop. IV) states that if "the periodic times are as the 3/2th power of the radii", then "the centripetal forces will be inversely as the squares of the radii; and conversely". This situation is said (in a scholium) to obtain "in the celestial bodies".²¹ Some theorems later²²

²¹This scholium would make it appear that Newton *did* believe that Kepler's third law is a valid and verifiable statement, and we might conclude that he was not aware of the contradiction between his own system of dynamics and Kepler's third law, which has been brought to our attention by Karl Popper. But Newton is here speaking in a general manner, and we are no more to think that he did believe the third law to be the true description than we would assume that he equally thought the orbits of the planets to be circles, which is the context of this Prop. IV. In Book III where the three laws of Kepler appear under the rubric of "The phenomena", it is clear that Newton believes the laws of Kepler to hold in the world of observation only approximately, or to the degree of accuracy of observations. In the first edition, the laws of Kepler had been called "Hypotheses"; they became "Phenomena" in the second edition of the *Principia* (1713).

²²Between the first and the second editions, Newton altered the end of section II, in which he discusses a variety of questions, including the way to find a planetary orbit (ellipse) if one is given three positions and velocities. He also discusses the question of motion along an ellipse if there is a force directed to the center.

The "spirit of the process by which Newton has resolved this interesting problem [of motion according to Kepler's first two laws]" has been displayed in an admirable and succinct manner by John F. W. Herschel, *Outlines of Astronomy*, new edition, Longmans, Green, and Co., London, 1875, §§ 490–493: "In the case before us, of an ellipse described by the action of a force directed to the focus, the steps of the investigation of the law of force are these: 1st, The law of the areas determines the actual *velocity* of the revolving body at every point, or the space really run over by it in a given minute portion of time; 2dly, The law of curvature of the ellipse determines the linear amount of deflection from the tangent *in the direction of the focus*, which corresponds to that space so run over; 3dly, and lastly, The laws of accelerated motion declare that the intensity of the acting force causing such deflection *in its own direction*, is measured by or proportional to the amount of that deflection, and may therefore be calculated in any particular position, or generally expressed by geometrical or algebraic symbols, *as a law* independent of particular positions, when that deflection is so calculated or expressed."

Like Newton himself, and all others interested in celestial mechanics who have read Newton's *Principia*, Herschel pointed out that this third law of Kepler, in its strict sense, as Newton has proved in his 59th proposition, "is applicable only to the case of planets whose proportion to the central body is absolutely inappreciable. When this is not the case, the periodic time is shortened in the proportion of the square root of the number expressing the sun's mass or inertia, to that of the sum of the numbers expressing the masses of the sun and planet; and in general, whatever be the masses of two bodies revolving round each other under the influence of the Newtonian law of gravity, the square of their periodic time will be expressed by a fraction whose numerator is the cube of their mean distance,

(Section III, Prop. XI), Newton proves that if a point-mass²³ "revolves in an ellipsis", then "the law of the centripetal force tending to the focus of the ellipsis" is "inversely as the square of the distance". There follow similar propositions on motion in a hyperbola (Prop. XII), and in a parabola (Prop. XIII). Then, in Prop. XIV, he considers that "several bodies revolve about one common centre, and [that] the centripetal force is inversely as the square of the distance of places from the centre"; in this case "the periodic times in ellipses are as the 3/2th power of their greater axes", as stated in Prop. XV.

All of these theorems are based on two assumptions. First, that the mass of the orbiting "body" is not a variable parameter (so that the law of centripetal force in each proposition is stated in relation to distance only).²⁴ Second, that the center of force is a mere point, and not a body with a mass that would have an interaction with the mass of the orbiting body. Even when (as Prop. XIV) Newton's condition is that "several bodies revolve about one common centre," these are not 'bodies' which attract (and therefore interact with, or affect the motions of) the other bodies. These are, therefore, mass-points, with an orbital motion determined entirely by an initial 'inertial' component and

i.e. the greater semi-axis of their elliptic orbit, and whose denominator is the sum of their masses. When one of the masses is incomparably greater than the other, this resolves into Kepler's law; but when this is not the case, the proposition thus generalized stands in lieu of that law. In the system of the sun and planets, however, the numerical correction thus introduced into the results of Kepler's law is too small to be of any importance, the mass of the largest of the planets (Jupiter) being much less than a thousandth part of that of the sun."

²³It will become clear, below, why I use the expression 'point-mass'.

²⁴By this I mean only that Newton does not deal with the question of whether or not two different bodies, having very different 'masses' or 'quantities of matter', would have the same or different orbits, or even move with the same speeds, if placed successively and separately at a particular distance from the Sun. Even in dealing with Kepler's third law, under the restricted conditions of the early part of the *Principia* (as Prop. XV), he is not concerned with a 'system' of various bodies or point-masses or planets moving simultaneously around the center of force, but rather with the more restricted situation in which one and the same body, say the Earth (or really a point-mass), might be taken from its own orbit and carried to some other orbit (say, corresponding to the orbit of Mars); if it were so placed as to have the direction and velocity of that planet, it would then—according to Kepler's third law—describe the very same orbit in the same period. Later on, when Newton found the proper correction for Kepler's third law, in terms of his own system of dynamics, he was, of course, aware that in the foregoing situation a small correction would have to be made for the fact that the mass of the Earth is different from the mass of Mars.

a 'force' directed to a mathematical 'centre'. Under these conditions Kepler's three laws are proved by Newton to be valid. Newton has not only determined the conditions of validity of these laws; he has also discovered *for the first time* the significance of each of Kepler's three laws of planetary motion. Admittedly, the system in which these laws hold is rather restricted, and differs greatly from any system in nature.²⁵

Additionally, Newton's 'theory' showed for the first time the universality of Kepler's first two laws: areas and elliptical orbits. Kepler himself had dealt with the actual shape of the orbit of only one planet, Mars; he had, furthermore, really investigated the orbit only in the neighborhood of the apsides. This may have been the basis of Newton's stand, that Kepler had no rightful claim to the 'discovery' of the elliptical orbits of the planets,²⁶ nor of the law of areas. In a memorandum,

²⁵By this I mean that Newton showed for the very first time that Kepler's law is both a necessary and sufficient condition that a point-mass moving in a curved path according to the law of areas must be subjected to a force constantly directed toward a given point: that the elliptical shape of orbits implies that this centrally-directed (or 'centripetal' force) must vary inversely as the square of the distance from the focus from which the equal areas are reckoned. Of course, there is an assumption of a force acting; this assumption follows from the Galileo-Descartes-Gassendi-Newton law of inertia, since a point-mass (or any other body) moves in a straight line and not in a curve in the absence of any external force. Newton showed, furthermore, that the third law indicates the 'universality' of the force, as may be seen by the consideration of the motion of the point-mass (approximated by a planet) shifted from one orbit to another. This last relation enabled Newton to set up also an identity between the planetary force, the force of a planet on a satellite (such as the Earth upon the Moon), and the force a planet exerts on a body falling down to it. Newton not only showed, therefore, the 'physical' meaning of each of Kepler's laws, but—as I shall stress below—the *artificial* conditions under which the law holds exactly, rather than approximately (as in the real world of planets and satellites).

²⁶As we shall see below, Newton may very well have been overly fussy as to the distinction between a 'guess' or a mere 'assumption' and a 'demonstration'; he was trying as hard as he could to mark off the true character of his own discovery of the inverse-square law from 'guesses' made by Hooke, or—for that matter—by many others. We would agree that it is one thing to 'guess' that there is a planetary force (even one which varies inversely as the square of the distance), and another thing to understand the relation of that law to the other laws of planetary motion, and the nature of perturbations, orbits, and planetary motions. For instance, a true 'demonstration' which would be part of a valid claim to the 'discovering' of the inverse-square law would imply that the discoverer appreciated the fact that while the elliptical orbit implies an inverse-square law, the inverse-square law does not necessarily imply an ellipse (the parabola and the hyperbola are also possible). Furthermore, the discovery would certainly imply an understanding of the irreconcilability of this law with a false law of orbital motion, such as the one saying

based on A. De Moivre's discussions with Newton, it appears that in 1673, when Hooke and Newton were corresponding about the path of falling bodies on a moving Earth,

Dr [Hooke] writt to him that the Curve would be an Ellipsis & that the body would move according to Kepler's notion [*sic*: evidently meaning according to the law of areas], which gave him an occasion to examine the thing thoroughly & for the foundation of the Calculus he intended laid down this proposition that the areas described in equal times were equal, which though assumed by Kepler was not by him demonstrated, of which demonstration the first glory is due to Sir Isaac.²⁷

Newton thus made an important distinction between an 'assumption' or guess and an actual demonstration. Newton refused to give Hooke credit for any share in the 'discovery' of the inverse-square law of gravitation, on the grounds that Hooke had only 'guessed' that this force would diminish in the ratio of the square of the distance, and could not 'prove' it.²⁸ Hooke, indeed, was wholly unable to apply this relation in any major circumstances, say in the motion of planets or the falling of the Moon. In a letter to Halley, concerning Hooke's claims to a share in the discovery of the inverse-square relation, Newton said:

But grant I received it afterwards from Mr Hooke, yet have I as great a right to it as to the ellipsis. For as Kepler knew the orb to be not circular but oval, and guessed it to be

that the speed of a planet at any place in the orbit is inversely proportional to its distance from the Sun. Kepler had advanced the latter point of view, even though it is in conflict with the law of areas (this topic is discussed by E. J. Aiton: "Kepler's second law of planetary motion", *Isis*, 60 (1969), 75–90). Newton showed that this law never holds exactly, but is approximated to some degree in the neighborhood of the apsides; which might account for Kepler's mistake. Since Hooke's particular claim to the invention of the inverse-square law was embodied in a statement in which he expressed his belief that the inverse-square law implies (or can even be derived from) the above-mentioned incorrect law of speed, Newton was fully aware that Hooke was a mere guesser, and that Hooke thus had no true claim to the inverse-square law on the profound level that he himself did.

²⁷See note 4 *supra*: John Conduitt's memorandum, based on De Moivre's recollections.

²⁸It is well known that on the very eve of publishing the *Principia*, Halley wrote to Newton that Hooke expected to have his priority mentioned somewhere; Newton refused, at first even threatening to withdraw Book III, in which he had expounded the system of the world. See I. B. Cohen: *Introduction* (cited in note 4 *supra*), ch. 5, §1.

elliptical, so Mr Hooke, without knowing what I have found out since his letters to me, can know no more, but that the proportion was duplicate *quam proxime* at great distances from the center, and only guessed it to be so accurately, and guessed amiss in extending that proportion down to the very centre, whereas Kepler guessed right at the ellipsis. And so Mr Hooke found less of the proportion than Kepler of the ellipsis. There is so strong an objection against the accuracy of this proportion, that without my demonstrations, to which Mr Hooke is yet a stranger, it cannot be believed by a judicious philosopher to be anywhere accurate. And so, in stating this business, I do pretend to have done as much for the proportion as for the ellipsis, and to have as much right to the one from Mr Hooke and all men, as to the other from Kepler; and therefore on this account also he must at least moderate his pretenses.²⁹

These sentiments leave no doubt that Newton was firmly convinced that he was entitled to the credit for the law of elliptical orbits as much as for the inverse-square law of attraction. And so we may understand why Newton, in the *Principia*, gave credit specifically to Kepler for the third law, and not for the first two laws, which are presented without the name of their discoverer.³⁰

²⁹Newton to Halley, 20 June 1686; published in Newton, *Correspondence*, vol. 2, pp. 435–437; presented in a modern (but not modernized) version. See note 4 *supra*.

³⁰Had Newton not gone beyond Section III in the *Principia*, he would not have founded modern celestial mechanics, since he would not have advanced to the stage where he would have been able to deal with the real world: as found in the solar system as a whole, or even the Jovian system. Let us, however, arbitrarily suppose that the first three Sections did not represent a mere stage on Newton's way to the fully developed system of celestial mechanics of the *Principia*. Under these circumstances, he would not have so advanced the subject as to be able to take cognizance of the mutual interaction of the planets, or the phenomena and theory of perturbations (obviously most consequential when planets come close to one another, and most marked in the effects of Jupiter on the other planets, since Jupiter has so much greater a mass). Essentially, he would have produced the dynamics of point-masses moving about a center of force (where there is no body); even this situation is highly artificial, once we have two such point-masses. For we would have to assume that each of the two-point-masses is acted upon by the centripetal force directed toward a focus of their elliptical orbits (which means they must have some 'mass'), and yet neither of these point-masses will have any gravitational attraction for the other.

Additionally, under the foregoing condition, Newton would not have reached

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In the *Principia*, as stated above, Sections II and III of Book I are devoted to the motion of a point-mass acted on solely by a force directed toward a point or center, and thus called by Newton a "centripetal" force. Sections IV and V are geometrical, and Section VI deals with the problem of finding the position of a body in its orbit at any given time, under the same conditions as hold for Sections II and III, concluding in Newton's contributions to 'Kepler's problem'.³¹ Section VII deals with yet further aspects of motion under the same specified conditions, as—for instance—the descent of a body toward a point, under the action of a force that varies inversely as the square of the distance from that point. Then, in Section VIII Newton discusses "orbits wherein bodies will revolve, being acted upon by any sort of centripetal force". Here there is presented the great Prop. XLI, neglected by most analysts of the *Principia* today,³² but widely acknowledged in Newton's time as the major example of his ability to deal with Kepler-motion in general, and especially cited by Leibnizians.³³ It reads:

the concept of the center of force being located in a real body, rather than a mathematical point. The importance of having the center of force in a real body, we have observed, is that there will be a resulting mutual attraction. Now the planet will attract the Sun just as the Sun attracts the planet. The result is that *not one* of the three of Kepler's laws is *followed exactly*.

What if Newton had not known how to proceed from Section III, and if he had not introduced a central attracting body (to say nothing of a gravitational interaction between every pair of bodies)? Or, what if, when he had completed Section III, he had been diverted from the enterprise of working out the principles of celestial mechanics? He and his readers, I submit, would, nonetheless, have seen wherein Kepler's laws would need correction, even though he did not yet have a proper 'theory' in Popper's sense. In short, the particular contradiction which Popper has presented to us, arising from a confrontation of Kepler's third law and Newton's 'theory' (even ignoring perturbations) would have become apparent as a contradiction between the conditions Newton had displayed in Sections I-III (under which Kepler's third law is valid) and the conditions that obtain in the universe, or in any central body and its satellite (or satellites), or in any two-body system (such as a double star). We shall return to this point in a later section.

³¹On 'Kepler's problem', see note 19 above.

³²Two scholars who have been singularly appreciative of Prop. XLI are E. J. Aiton (who has discussed this proposition and the reactions to it, in several articles in *Annals of Science*, and also in his recent book on the history of vortex theories of celestial mechanics) and D. T. Whiteside, notably in his: "The Mathematical Principles underlying Newton's *Principia Mathematica*", University of Glasgow (*Glasgow University Publications*, 138), 1970.

³³For instance, by J. Hermann, in his *Phoronomia . . .* (Amsterdam: apud Rod. & Gerh. Wetstenios H. FF., 1716), p. 73 and elsewhere.

Supposing a centripetal force of any kind, and granting the quadratures of curvilinear figures, it is required to find as well the trajectories in which bodies will move, as the times of their motions in the trajectories found.

In this part of the *Principia* there are many propositions which have the conditional phrase "granting the quadratures of curvilinear figures", which refers to the possibility of finding the areas ("quadratures") of figures bounded by a curve or by curves. Newton had devised a method for finding such "quadratures", or what we would call a general method of integration, but had not published it. The particular theorems which he had in mind, together with their proofs, remained in manuscript until they were published *in extenso* only a couple of years ago in D.T. Whiteside's edition of *The Mathematical Papers of Isaac Newton*.³⁴

In Section XI of the *Principia*, Newton advances to a stage beyond the simple model of a point-mass acted on by a force directed to a specific mathematical point. In the introduction to Section XI, he makes it clear that hitherto he has "been treating of the attractions of bodies towards an immovable centre" and he makes the admission that "very probably there is no such thing existent in nature"; surely not in the system of Sun and planets, nor even in a system composed of a single planet and its satellites. For, wholly apart from the perturbations of one attracting body (planet or satellite) on another, "attractions", as Newton declares, "are made toward bodies". Hence (by the reciprocal equal action of one body on another, according to Law III) "neither the attracted nor the attracting body is truly at rest," but both will "revolve about a common centre of gravity". In this 'real world', as opposed to the hypothesized world presented in the earlier sections of the *Principia*, Kepler's laws will no longer hold exactly. Thus in Prop. LXV, Newton states:

Bodies, whose forces decrease in a duplicate ratio of their distance from their centres, may move among themselves in ellipses; and by radii drawn to the foci may describe areas proportional to the times very nearly.

Not only are areas described in a proportion determined only "quam

³⁴Vol. 3, Cambridge University Press, Cambridge, 1969, pp. 373 sqq.

proxime", but even the ellipses prove to be not truly ellipses. In Prop. LXIV, Newton supposed the "forces with which bodies mutually attract each other to increase in a simple ratio of their distances from the centres", but now in Prop. LXV he contrasts the two situations, observing that in Prop. LXIV he had "demonstrated that case in which the motions will be performed exactly in ellipses"; but "according to the law supposed" in this Proposition LXV, bodies will *not* "move exactly in ellipses unless by keeping a certain proportion of distances from each other". He then deals with two special cases in which "the orbits will not much differ from ellipses". There can be no doubt, therefore, that Newton was aware that after he added the true conditions of nature—specifically, (1) that forces originate in real bodies (such as the Sun or Jupiter), and (2) the effects of real bodies (that is, bodies with a definite mass, such as planets moving around the Sun, or satellites moving around planets)—the simple law of elliptical orbits and the simple law of areas no longer were consistent with his own 'theory'. In short, Newton discovered that the first two laws of planetary motion, as announced by Kepler, were not valid in the world of Newtonian physics. But what of Kepler's third law?

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Let me now turn to Newton's treatment of Kepler's third law in Section XI of Book I of the *Principia*. In Section XI, as opposed to the earlier parts of Book I, we have seen that Newton assumes that forces arise from real bodies (not mathematical points); and that in a system of two (or more) attracting bodies, each one will mutually and equally act upon the other.

In Prop. LX (Section XI, Book I), Newton deals with two bodies *S* and *P* that "revolve round their common centre of gravity", while "attracting each other [mutually] with forces reciprocally proportional to the squares of their distance". Under these conditions, he proves that

... the principal axis of the ellipsis which either of the bodies as *P* describes by this motion about the other *S*, will be to the principal axis of the ellipsis, which the same body *P* may describe in the same periodical time about the other body *S* quiescent, as the sum of the two bodies *S* + *P* to the first of

two mean proportionals³⁵ between that sum and the other body *S*.

Newton is thus saying that in a system of planets of masses *P*₁, *P*₂, ... in which these masses are not negligible with respect to the mass of the Sun *S*, we can no longer assume a simple harmonic law in which the respective semi-axes major (*a*₁, *a*₂, ...) will be related to the periodic times (*T*₁, *T*₂, ...) in the proportion

$$\frac{a_1^3}{T_1^2} = \frac{a_2^3}{T_2^2} = \dots$$

Once we take account of the fact that the Sun and planets are moving about a common centre of gravity, we will find a new ellipse for each Sun-and-planet system, one with semi-axis major *ā*₁ (or *ā*₂, ...), which for the same periodic times (*T*₁, *T*₂, ...) will be to *a*₁ (or *a*₂, ...) in the ratio of (*S* + *P*₁) to the first of two mean proportionals of that quantity³⁶ and *S*. That is,

$$\frac{\bar{a}_1}{a_1} = \frac{S + P_1}{\sqrt[3]{(S + P_1)^2 S}}, \text{ etc.}$$

³⁵See note 36 *infra* for a definition of the "first of two mean proportionals".

³⁶In general, if

$$\frac{X}{x} = \frac{x}{y} = \frac{y}{Y}$$

then *x* is the *first of the two mean proportionals* between *X* and *Y*, and

$$x = \sqrt[3]{X^2 Y}.$$

In the present instance, the first mean proportional is

$$x = \sqrt[3]{(S + P)^2 S}.$$

Hence, the principal axis of *P*'s ellipse, when *S* moves, is to the principal axis of *P*'s ellipse when *S* is stationary (the periodic time of *P* remaining the same) as

$$(S + P) : \sqrt[3]{(S + P)^2 S}.$$

so that

$$a_1^3 = \bar{a}_1^3 \frac{(S + P_1)^2 S}{(S + P_1)^3} = \bar{a}_1^3 S / (S + P_1)$$

and

$$a_2^3 = \bar{a}_2^3 S / (S + P_2), \dots$$

Hence, the primitive or simple harmonic law

$$\frac{a_1^3}{T_1^2} = \frac{a_2^3}{T_2^2}$$

becomes, in a world of non-negligible gravitating masses,

$$\frac{\bar{a}_1^3 S / (S + P_1)}{T_1^2} = \frac{\bar{a}_2^3 S / (S + P_2)}{T_2^2}$$

Or, universally, we may write Kepler's third law, for any two-body system, in the form

$$\frac{\bar{a}_1^3}{T_1^2 (S + P_1)} = \frac{\bar{a}_2^3}{T_2^2 (S + P_2)}$$

or

$$\frac{\bar{a}_1^3 (S + P_2)}{T_1^2} = \frac{\bar{a}_2^3 (S + P_1)}{T_2^2}$$

Therefore, for the system in which the planets and Sun move about their common center, Newton's relation is

$$\frac{\bar{a}_1^3 / T_1^2}{\bar{a}_2^3 / T_2^2} = \frac{S + P_1}{S + P_2}$$

To see the actual difference between this law and Kepler's law, we may divide through by S , and transform the equation so as to get

$$\frac{\bar{a}_1^3 / T_1^2}{\bar{a}_2^3 / T_2^2} = \frac{1 + (P_1/S)}{1 + (P_2/S)}$$

the form in which this law is commonly found in contemporary works

on celestial mechanics. This law can be extended readily to any number of planets.

For most problems in the solar system, the planetary masses P_1, P_2, \dots are so small that they may be neglected; that is, the quantities $P_1/S, P_2/S, \dots$, may be discarded; and the general equation then corresponds to the original formulation of Kepler's law.

Instead of expressing this result in a close algebraic version of Newton's own verbal statements, we could have expressed Newton's result in the form

$$\frac{\bar{a}_1^3}{T_1^2 (S + P_1)} = \text{constant}$$

where for suitable units; the value of the constant is one. Under these conditions the law is

$$\frac{\bar{a}_1^3}{T_1^2} = S + P_1$$

which is the way in which it is given by Popper.³⁷

Newton's amended form of Kepler's third law differs from the conventional one, or the one presented by Popper, only to the extent that Newton bases his statement on the "first mean proportional" and does not use literal equations. If by Newton's 'theory' we mean the development of dynamics in general, and of celestial mechanics in particular, as expounded in the *Principia*, then it is at best misleading to say that Newton's 'theory' contradicts Kepler's third law. Rather, Newton's 'theory' first displays the hypothetical circumstances under which this and the other two of Kepler's laws are valid, and then shows the modified form in which all three of these laws occur in Newtonian dynamics.

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Let me now turn to Popper's conclusions. These relate to both Galileo's laws and Kepler's but the two situations are similar enough for us to continue this analysis without introducing the Galilean example in full detail.³⁸

³⁷See §2 *supra*.

³⁸Popper's discussion of Galileo follows the same lines as his analysis of Kepler. There, too, although he is allegedly dealing with historical events, Popper

First, Popper states that "from Galileo's or Kepler's theories we do not obtain even the slightest hint how these theories would have to be adjusted . . . in order to proceed from these theories to another and more generally valid one such as Newton's."³⁹ I suppose that what we are to understand by Popper's statement is that if their results (laws or "theories") had contained such a "hint", or if this "hint" had been more manifest, we might have expected them or their immediate successors to go on to the next stage: Newtonian celestial mechanics. Popper assumes that the way "these theories would have to be adjusted" depends on discerning "what false premises would have to be adopted, or what conditions stipulated". In short,

Only after we are in possession of Newton's theory can we find out in which sense the older theories were approximations to it. We may express this fact briefly by saying that

is only contrasting Galilean principles of motion with the Newtonian dynamics, and not really concerning himself with either the historical Galileo or the historical Newton.

Popper shows that two of Galileo's assertions (". . . that a thrown stone or a projectile moves in a parabola, except in the case of a free vertical fall when it moves, with constant acceleration, in a straight line") are false from "the point of view of Newton's theory . . . for two distinct reasons." The first is that the path "of a long-range projectile . . . will be not even approximately parabolic: it is elliptic." While the parabolic trajectory is "an excellent approximation" for "sufficiently short throws," the "parabolic track is not strictly deducible from Newton's theory unless we add to the latter a factually *false* initial condition (and one which, incidentally, is unrealizable in Newton's theory) to the effect that the radius of the earth is infinite." As in the case of Newton and Kepler, we are given no hint as to whether Newton himself was aware of this contradiction, or whether even Galileo may have been.

It turns out that Newton was fully cognizant of the contradiction presented by Popper. Hence, as was the case for Kepler's laws and Newton's physics, the historian is challenged to find the steps whereby Newton may have proceeded from Galileo's laws to his own dynamics. Furthermore, even Galileo was aware of the fact (without fully knowing Newton's dynamics) that "conclusions proved in the abstract" must be modified when they are "applied in the concrete". Thus, in the motion of a projectile, "neither will the horizontal motion be uniform nor the natural acceleration be in the ratio assumed, nor the path of the projectile a parabola. . ." Hence, even though Galileo did *not* know about elliptical paths, he was aware that the trajectory could not be a true parabola, and compared the situation to the fact that "in all their discussions, Archimedes and the others considered themselves as located at an infinite distance from the center of the earth".

See "The Aim of Science" (cited in n. 5 *supra*), p. 30; Galileo's *Dialogues Concerning Two New Sciences*, Henry Crew and Alfonso de Salvio, trans., The Macmillan Company, New York, 1914, pp. 240-241.

³⁹"The Aim of Science" (cited in n. 5 *supra*), p. 33.

although from the point of view of Newton's theory, Galileo's and Kepler's are excellent approximations to certain special Newtonian results, Newton's theory cannot be said, from the point of view of the other two theories, to be an approximation to their results. All this shows that logic, whether deductive or inductive, cannot possibly make the step from these theories to Newton's dynamics. It is only ingenuity which can make this step. Once it has been made, Galileo's and Kepler's results may be interpreted as corroborating the new theory.⁴⁰

The final portion of this paragraph sets forth grounds for research in the history of scientific ideas (or the conceptual history of science) in a striking way. This declaration reveals the great importance of Popper's general philosophy of science for the practicing historian. The only alteration I might wish to make would be to substitute for Popper's word "ingenuity" a more active expression such as 'insight' or 'exercise of the creative imagination'.

It is not surprising that Karl Popper should have such wise counsel for the historian of scientific ideas. As a long-time critic of the "widely accepted view" that "the empirical sciences can be characterized by the fact that they use '*inductive methods*', as they are called",⁴¹ Popper has challengingly called upon us to seek out the imaginative content or structure of scientific discovery, the genesis of new ideas—concepts, laws, relations, theories, or modes of inquiry (including new ways of experimenting)—or their alteration on being used in new ways or in new contexts or in circumstances so different from those conceived by their first proponents that they are given a wholly new life.⁴² The 'inductivist' approach, on the other hand, would have us seek out the exercise of skill in applying 'the method' rather than the role of the creative imagination in making a great leap forward. The opening paragraph of Popper's *The Logic of Scientific Discovery* declares:

A scientist, whether theorist or experimenter, puts forward statements, or systems of statements, and tests them step

⁴⁰*Ibid.*, p. 33.

⁴¹Karl R. Popper: *The Logic of Scientific Discovery*, Basic Books, New York, 1959, ch. I, p. 27.

⁴²See his essay, "Truth, Rationality, and the Growth of Scientific Knowledge," pp. 215 ff. of *Conjectures & Refutations: the growth of scientific knowledge*, Basic Books, New York and London, 1962.

by step. In the field of the empirical sciences, more particularly, he constructs hypotheses, or systems of theories, and tests them against experience by observation and experiment.

Here the historian of scientific ideas may find yet another bold statement of the guiding principle of his endeavor. Popper goes on:

I suggest that it is the task of the logic of scientific discovery, or the logic of knowledge, to give a logical analysis of this procedure; that is, to analyse the method of the empirical sciences.

Here, however, the historian no longer finds Popper expressing fully the goal of historical research. The historian's primary concern is to study and analyze the genesis and growth of concepts and statements, and if necessary to find and display the 'illogic' without restricting himself to the 'logic' of scientific discovery; but Popper as philosopher is rightly more concerned with analysis of method in the sciences than with historical investigations in depth. The historian must also perform a task the philosopher need not take on, to study the way concepts, theories, or methods are related to the scientific and general background of the age in which they were conceived. A historian must use logic to illuminate the degree to which such innovations may reflect the idiosyncratic qualities (or even the unique personality) of their progenitor.

In short, the target of the philosopher, as expressed by Popper, is "the growth of scientific knowledge"—by which he means not the mere "accumulation of observations", but rather "the repeated overthrow of scientific theories and their replacement by better or more satisfactory ones."⁴³ From this point of view there can be no objection to his making a logical confrontation of Kepler's 'theory' (as expressed in the three laws of planetary motion, but primarily in the third law) and Newton's 'theory'. If I insist, therefore, that Popper is wrong to state, without qualifications, that Newton's 'theory' contradicts 'Kepler's theory', the reason is not merely that I wish to assign to Newton himself the credit for the logical or technical portion of Popper's analysis. Rather, I believe that an awareness of the full extent of Newton's own involvement may provide a key to the creative scientific process, to the degree that it is

⁴³*Ibid.*, p. 215.

illustrated by Newton. The difference between Popper's presentation and mine is, therefore, that he is concerned with the logical analysis of science and the logical relations (or lack thereof) between successive theories, and thus necessarily omits (and so masks) the stages of development of each theory. I believe his approach characterizes many philosophers of science who use historical examples.

The contrast between the method of the historian and of the philosopher of science can be illustrated by the phrase 'Newton's theory', which is said to contradict 'Kepler's theory'. Is this a reference to Newton's fully developed theory of celestial mechanics in the *Principia*, including the consequences of the mutuality of the action of a force between the Sun and a planet, plus the theory of perturbations in which each planet may have its motion affected by the gravitating force of other planets? Or was the contradiction already apparent in a more primitive version of the theory, or possibly discernible at some early stage in the development of the final theory? As mentioned earlier,⁴⁴ the *Principia* displays the dynamical significance of each of Kepler's three laws of planetary motion *well* before developing Newton's own "more generally valid" later "theory", to use Popper's own words. In Sections II and III of Book I, Newton shows (Props. I, II, III) that the law of areas is a necessary and sufficient condition for pure linear inertial motion, so long as it is stated explicitly that the moving point-mass (*corpus*) sweeps out equal areas in equal times with respect to *any* point not on the line of motion; and that if a force acts (or if there be no pure linear inertial motion), then the law of areas is a necessary and sufficient condition for the force to be directed to a point, the very point with respect to which the equal areas are computed. Then, for the case of uniform circular motion, the third or harmonic law is seen (in Prop. IV and its corollaries) to imply that the central force must vary as the inverse-square of the distance; shortly, Newton will show that this law implies, further, that one and the same force must be acting on all the bodies moving in orbits according to the law. In Prop. IX, Section III, Book I, it is demonstrated that the elliptical orbit, taken with the area law, is to imply an inverse-square centripetal force directed toward a focus.

Had Newton stopped at this stage of his explorations, he would have

⁴⁴Chiefly in note 30 *supra*.

found "what false premises" were needed for Kepler's laws (or 'theory') to be valid. These conditions, as we have seen, are that there is a point-mass (or, if there are more than one point-masses, that they do not attract one another, or do not alter or affect each other's motions) which is attracted toward a point (not a real body; at least, if it is a body, it is not attracted by and so moved or altered in its position or motion by the point-mass). We must, therefore, as historians, ask a question that Popper does not raise: Even if Newton had never gone ahead to develop his 'theory' of universal gravitation, would he have seen what 'true' conditions would have to be introduced in place of these 'false' or, at best, mathematical conditions?⁴⁵

This question cannot be answered without establishing how Newton actually developed his 'theory', which shall be the burden of the following section (§ 8). But here we may observe that in the early stages, Newton accepted Kepler's third law and explored its consequences without assuming that a planet would act gravitationally on the Sun or that a satellite would similarly act on the planet it encircled. Then, apparently, he progressed to the law of areas and proved that the elliptical orbits of planets are caused by an inverse-square central force directed to the Sun at a focus—but still without taking cognizance of a possible mutual action, the planet producing a motion of the Sun, just as the Sun produces motion (or change in motion) in the planet. Then, still before writing the *Principia*, Newton recognized that the physical conditions under which Kepler's laws hold are not those of the real world. For instance, he became aware that the center of the sun does not lie in the common center of gravity of the sun and planets, and that the difficulties in computing planetary motions are further complicated by the fact that "each orbit is dependent on the combined motions of all the planets, not to mention their actions upon each other." But he did not know how possibly to deal with "so many causes of motion at the same time".⁴⁶

Are we not, therefore, justified in assuming that there are at least three distinct stages in the development of Newton's thought? One is the

⁴⁵This has already been discussed in note 30 *supra*.

⁴⁶Quoted from Newton's tract, *De Motu*, written in 1684, edited and translated in A. R. Hall and Marie Boas Hall, eds.: *Unpublished Scientific Papers of Isaac Newton. A selection from the Portsmouth collection in the University Library, Cambridge*, Cambridge University Press, Cambridge, 1962. See, further, note 72 *infra*.

recognition of the physical significance of Kepler's laws. The second is related, and is the awareness that the conditions of validity of these laws are not those of the real world, and hence that some corrections or alterations must be made. Third comes the full 'theory', in which the consequences of correcting or altering the conditions are worked out and yield a very different set of planetary laws.⁴⁷

The foregoing remarks apply primarily to Book I of the *Principia*. In Book III, called the "System of the World," Newton redevelops the main principles in their application to the external world. In the first edition (1687), Newton listed the three laws of Kepler as "Hypotheses". What else could he have called them? After all, he had just shown from dynamical principles that—without emendation—they are false, unless his own system were false. In short, whatever may have lain behind Newton's designation, these Keplerian statements about the motion of planets or their satellites were *in fact* hypothetical at best, without major modification.

I believe that Newton's use of the term 'hypotheses' was misunderstood by some readers, who gained the impression that the Newtonian system of the world was, in some general philosophical sense, 'hypothetical'. This was a natural enough conclusion, since a very careful reading was required to reveal that only certain statements were "Hypotheses", not the whole system! Indeed, in the general introduction to Book III, Newton has said that he would "now demonstrate the frame of the System of the World" and not of some supposed world. In the second edition (1713) and in the third (1726), these laws of Kepler appear as "Phaenomena". I suggest that a careful reading shows that Newton was saying that Kepler's laws⁴⁸ are 'phenomenologically true', that is, they are 'true' within the limits of observation; but they are then only 'true' as 'phenomena' and are not absolutely 'true' as stated without modification.⁴⁹

⁴⁷I refrain from generalizing as to whether or not these stages may ordinarily characterize conceptual advances in science.

⁴⁸Of course, Newton—as has been mentioned—does not use the expression 'Kepler's laws' and gives Kepler credit (in the *Principia*) only for the third law, and that in relation to the planetary system and not the system of Jovian satellites.

⁴⁹In the beginning of Book III of the *Principia*, in the second and third editions, Newton sets forth a series of "Rules of Reasoning in Philosophy" and "Phaenomena" which are to be used in presenting his "System of the World"—or in the application of the "mathematical principles" (developed in Books I & II) to the

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I have put off, until now, the discussion of a question of major importance, which Karl Popper's article poses to the historian of scientific ideas: how a scientist comes to make his discoveries. Have we any evidence that Newton, in the example under consideration, did indeed start out with Kepler's 'three' laws of planetary motion and determine the dynamical restrictions under which they are valid? In this sense, can we have any assurance that the presentation of this subject in the *Principia* is cast in a somewhat autobiographical mold? Or did Newton work out a fully developed system of celestial mechanics and only then look back to find out to what degree Kepler's laws are approximations to 'reality'? Or, possibly, did he in fact make a logical error in 'deducing', or in having thought he had 'deduced', his principles from Kepler's laws? Or even, with a great psychological leap of insight, did he transcend all common steps of deduction and inference—as Whiston said was his wont⁵⁰—

real world of experience and observation. The "Rules" are general and have what we could call today 'philosophical' character, whereas the "Phaenomena" deal with generalizations based upon experience. They include Kepler's law of areas, of the elliptical orbits, and the third or harmonic law applied to the motion of the planets around the Sun and of satellites around planets. It seems odd that Newton would present under the rubric of "Phaenomena" a set of propositions which he had proved mathematically could not exist in the real Newtonian world. But it must be stressed that in discussing each of these statements, he makes it clear as to what the evidence is of observation and calculation, to support the generalization in question. The reader is under no illusions as to the fact that these relations hold only to varying degrees of approximation, or limits of accuracy of observation, and are not absolutely true statements.

Nevertheless, Newton does use these statements in what may appear to be a misleading fashion. That is, he makes use of them in Book III in support of supposedly real conditions of planets and satellites. Thus he shows that there must be a center of force (planets attracted by the Sun, and satellites attracted by planets) because of the law of areas; and that there must be an inverse-square force because of the elliptical orbits; and that it is one and the same inverse-square force which acts upon each of the planets, because of the third or harmonic law. And then, following the procedures of Book I, he goes on to show that Kepler's laws cannot be valid without modification.

See, further, I. B. Cohen: "Hypotheses in Newton's Philosophy", *Physis*, 8 (1966), 163–184.

I would not wish the reader to assume that I believe Newton always (and consistently) interpreted the word 'phenomenon' in this particular way.

⁵⁰William Whiston: *Memoirs . . .* (London: printed for the author, and sold by Mr. Whiston . . . and Mr. Bishop, 1749), p. 39:

"Sir Isaac, in Mathematics, could sometimes see almost by Intuition, even without Demonstration; as was the Case in that famous Proposition in his *Principia*,

and 'divine' the principles of Newtonian celestial mechanics while contemplating Kepler's laws?

In trying to answer such questions, even without any real degree of positiveness, the historian of scientific ideas is on very uncertain ground. In attempting to determine how any given scientist may have made a discovery, the historian must be especially wary of later recollections of the scientist, whether in the form of autobiographical writings or interviews. For instance, as an old man, Newton told William Stukeley that his discovery of universal gravitation was made on seeing an apple fall; but I doubt if we are to take this anecdote too seriously as revealing a cause (or even the conscious 'occasion') of a discovery he had made some sixty years earlier.⁵¹ At the height of the priority dispute with Leibniz (c. 1712 sqq.), Newton wanted to show that he had had the calculus fully in hand when writing the *Principia* in the 1680's, and so he put forth the view "that the arguments used in the central portions of the *Principia* are", as D. T. Whiteside has observed, "effectively fluxional analyses clothed in the heavy dress of traditional synthetic geometry."⁵² Newton, on one occasion, even alleged that he had found some major propositions in the *Principia* by the use of fluxions and had then rewritten them for presentation in the style of good Greek geometry:

By the help of the new *Analysis* Mr. Newton found out most of the Propositions in his *Principia Philosophiae*: but because the Ancients for making things certain admitted nothing into Geometry before it was demonstrated synthetically, he demonstrated the Propositions synthetically, that the Systeme of the Heavens might be founded upon good Geometry. And this makes it now difficult for unskilful Men to see the Analysis by which those Propositions were found out.⁵³

that *All Parallelograms circumscribed about the Conjugate Diameters of an Ellipsis are equal*; which he told Mr. Cotes he used before it had ever been demonstrated by any one, as it was afterward. And when he did but propose Conjectures in Natural Philosophy, he almost always knew them to be true at the same time. . . ."

⁵¹See William Stukeley: *Memoirs of Sir Isaac Newton's Life . . .*, Taylor & Francis, London, 1936, pp. 19–21; see, further, Gavin de Beer & Douglas McKie: "Newton's apple", *Notes and Records of the Royal Society of London*, 9 (1951), 46–54.

⁵²D. T. Whiteside: "The Mathematical Principles" (cited in note 32 *supra*).

⁵³Quoted from the anonymous book review (written by Newton himself) of the *Commercium epistolicum*, and published in *Philosophical Transactions*, 29 (1715), 206.

Despite such assertions of Newton's, however, we may agree with the conclusions of D. T. Whiteside, that:

It is . . . futile to plough laboriously through the voluminous mass of Newton's extant papers (containing 10–15 million words at a conservative estimate) in search of manuscripts bearing dotted fluxional arguments which reappear, suitably recast in geometrical mould, in the pages of the first edition of the *Principia*. As Newton himself never forgot (though he tried cleverly to conceal it by various sly turns of phrase from his contemporaries) the standard dot-notation for fluxions was invented by him only in mid-December 1691, almost four and a half years after the *Principia* first appeared in London's bookshops.⁵⁴

Another example of a Newtonian pseudo-history occurs in the Scholium to the Laws of Motion, in the beginning of the *Principia*, where Newton says:

By the first two Laws and the first two Corollaries, Galileo discovered that the descent of bodies varied as the square of the time [*in duplicata ratione temporis*] and that the motion of projectiles was in the curve of a parabola; experience agreeing with both, unless so far as these motions are a little retarded by the resistance of the air.⁵⁵

Wholly apart from the fact that Galileo found the law of falling bodies in a wholly different way, and that he certainly didn't have any idea of the Newtonian concept of force in the second law of motion, does this statement imply that Newton was inspired by Galileo to formulate the first two laws of motion? I think not. Newton seems not to have read Galileo's *Two New Sciences* (in which these results concerning falling bodies and projectiles appear) until years after writing and publishing the *Principia*.⁵⁶ If he found these laws in a rudimentary form anywhere as a young man, it was rather in Descartes' *Principia*.

⁵⁴D. T. Whiteside: "The Mathematical Principles" (cited in note 23 *supra*), p. 9.

⁵⁵Scholium following Corollary VI, Laws of Motion.

⁵⁶See I. B. Cohen: "Newton's attribution of the first two laws of motion to Galileo", *Atti del Simposio su «Galileo Galilei nella storia e nella filosofia della scienza»* (Firenze, Sept. 1964), pp. XXIII–XLII.

Newton, of course, may have (possibly later on) encountered Galileo's work in a secondary source such as the writings of Charleton, Digby, or Grassendi. Another possible source might have been Barrow.

Newton tells us, in an autobiographical fragment quoted at the beginning of this article, how in 1666 he had begun to think of "gravity extending to . . . the Moon"; he had independently found the law of central acceleration (v^2/a); and making use of Kepler's third law ($a^3/T^2 = k$), he had then found the forces which keep the planets in their "Orbs" to be "reciprocally as the squares of their distances from the centers about which they revolve". If the force (F) is as the acceleration (A), and A is as v^2/a , then by the simplest algebra (C being a constant),

$$\begin{aligned} F &= C \cdot A = C \cdot \frac{v^2}{a} = C \cdot \frac{(2\pi a/T)^2}{a} \\ &= C \cdot \frac{4\pi^2 a^2}{a T^2} = \frac{4\pi^2 C}{a^2} \cdot \frac{a^3}{T^2} \\ &= \frac{4\pi^2 C k}{a^2} \end{aligned}$$

or

$$F \propto \frac{1}{a^2}.$$

But this calculation is based on circular orbits, and so could not be expected to do better than "answer pretty nearly"⁵⁷ to the observed properties of the Moon's motion.

In Newton's case the foregoing calculation of the 'falling' of the Moon, or the central acceleration of the Moon as it moves in its orbit around the Earth, is a significant achievement. It requires a not generally accepted concept of force and a mode of analysis in which circular motion is resolved into two components: a linear or tangential inertial motion, and an accelerated motion of descent toward the center to keep the Moon in its orbit ('falling' away from the tangent).⁵⁸ These could have occurred to anyone of real talent; what is significant is that Newton had to discover the law of centripetal (or 'centrifugal') acceleration or

⁵⁷See the first quotation from Newton in § 1 *supra*.

⁵⁸Possibly the most difficult of these ingredients is the concept of force. For example, Huygens could not conceive of a gravitational attractive or grasping force spreading out to planetary distances. He was thus denied the concept of one planet exerting a perturbing force on another.

force.⁵⁹ After 1673, when Huygens published the law of centrifugal force in his *Horologium Oscillatorium*, it was—as Newton pointed out⁶⁰—no great feat to combine $F \propto v^2/a$ with $a^3/T^2 = \text{constant}$, to obtain $F \propto 1/a^2$ for circles!

But what about ellipses? Let us allow Newton to testify:

In the end of the year 1679 in answer to a Letter from Dr Hook then Secretary of the R.S. I wrote that whereas it had been objected against the diurnal motion of the earth that it would cause bodies to fall to the west, the contrary was true. For bodies in falling would keep the motion which they had from west to east before they began to fall. . . . Dr Hook replied soon after that they would do so under the Equator but in our latitude they would fall not exactly to the east but decline from the east a little to the south. . . . And he added that they would not fall down to the center of the earth but rise up again & describe an Oval as the Planets do in their orbs. Whereupon I computed what would be the Orb described by the Planets. For I had found before by the sesquialterate proportion of the tempora periodica of the Planets with respect to their distances from the Sun, that the forces which kept them in their Orbs about the Sun were as the squares of their mean distances from the Sun reciprocally: & I found now that whatsoever was the law of the forces which kept the Planets in their Orbs, the areas described by a Radius drawn from them to the Sun would be proportional to the times in which they were described. And by the help of these two propositions I found that their Orbs would be such Ellipses [i.e. with the sun as force-centre set in the lower focus] as Kepler had described.⁶¹

⁵⁹Newton's own independent discovery of ' v^2/r ' is recorded in the "Waste Book" (University Library, Cambridge, MS Add. 4004), and described in the final paragraph of the Scholium to Prop. IV, Book I, of the *Principia*. See, further, John Herivel: *Background to Newton's Principia*, Clarendon Press, Oxford, 1965.

⁶⁰This point of view is expressed clearly in Newton's letter to Halley of 20 June 1686, post-script, where Newton says that after "Hugenius had told how to find the force in all cases of circular motion, he had told 'em how to do it in this as well as all others. And so the honour of doing it in this is due to Hugenius."

⁶¹University Library, Cambridge, MS Add. 3968, § 9, fol. 101, printed in D. T. Whiteside: "Newton's Early Thoughts on Planetary Motion: a fresh look", *British Journal for the History of Science*, 2 (1964), 117–18.

In appreciating this statement, we must keep in mind that while Kepler's third law was rather generally accepted and used in the mid-seventeenth century, the law of elliptical orbits was less commonly part of astronomical practice, and the law of areas was very rarely even mentioned. For instance, in Thomas Streete's *Astronomia carolina* (1661, 1663), from which Newton learned both about elliptical orbits and the third law, the second law does not appear. Astronomers then used one or another variant of a speed law for planets, based on an 'equant': the planet would be supposed to move along the ellipse so that a radius-vector from the 'empty focus' to the planet would sweep out equal angles in any equal times. A correction factor was often added to make the results more accurate.⁶²

In autumn 1679, Newton received a letter from Hooke, in which a philosophical correspondence was proposed, so that Newton might thus renew his contributions to the Royal Society.⁶³ Would Newton let Hooke know his "thoughts" concerning a "hypothesis or opinion of mine" of "compounding the celestall motions of the planetts of a direct motion by the tangent & an attractive motion towards the centrall body. . . ." Newton replied by discussing what appeared to be a quite different problem, namely, the falling of a body on a rotating Earth.⁶⁴ In the ensuing discussion, Newton evidently tried to avoid having to grapple with the planetary force, either not being convinced that "the problem was of scientific importance",⁶⁵ as Hooke had supposed,⁶⁶ or not yet being pre-

⁶²The simple and most commonly used 'corrected' method of computing planetary positions by means of equal angular motion about the empty focus are associated with the names of Ward and Bullialdus, although the uncorrected method had been discovered (and rejected) by Kepler; another correction was introduced by Mercator. I have been studying the accuracy of these approximations (making use of the computer), in conjunction with Prof. Owen Gingerich.

⁶³Hooke to Newton (24 Nov. 1679); Newton, *Correspondence*, vol. 2, p. 297. Hooke had just been made Secretary. Newton and he had had an acrid dispute on the nature of light only a few years earlier.

⁶⁴On the Newton-Hooke letters, and their significance in the development of Newton's thoughts on celestial physics, see Alexandre Koyré: "An unpublished letter of Robert Hooke to Isaac Newton", *Isis*, 43 (1952), 312–337; reprinted in A. Koyré, *Newtonian Studies*, Harvard University Press, Cambridge (Mass.), and Chapman & Hall, London, 1965. A most valuable study is J. A. Lohne: "Hooke versus Newton. An analysis of the documents in the case of free fall and planetary motion", *Centaurus*, 7 (1960), 6–52.

⁶⁵Quoted from D. T. Whiteside: *op. cit.* (note 61 *supra*), p. 134.

⁶⁶Hooke, in a letter to Newton, 6 January 1679/80, specifically referred to the "great Concerne to Mankind" in the problem, "because the Invention of the Longitude by the Heavens is a necessary Consequence of it . . .", Newton, *Correspondence*, vol. 2, p. 309.

pared "to accept that an ellipse might be traversed under some suitable law of gravitational decrease with distance".⁶⁷

Hooke pressed on, writing to Newton that his "supposition is that the Attraction always is in a duplicate proportion to the Distance from the Center Reciprocall, and Consequently that the Velocity will be in a subduplicate proportion to the Attraction, and Consequently as Kepler Supposes Reciprocall to the Distance".⁶⁸ Then, challenging Newton directly, "I doubt not but that by your excellent method you will easily find out what that Curve must be, and its proprietys, and suggest a physicall Reason of this proportion. . . ."⁶⁹

Newton did not reply, but he went to work with such an alacrity and intellectual zeal as perhaps only a challenge from Hooke could have produced. The results of his enquiries are stated clearly in the quotation given two paragraphs above: "I found now that whatsoever was the law of the forces which kept the Planets in their Orbs, the areas described by a Radius drawn from them to the Sun would be proportional to the times in which they were described." Secondly, "that their Orbs would be such Ellipses as Kepler had described" on the supposition that "the forces which kept them in their Orbs about the Sun were as the squares of their . . . distances from the Sun reciprocally. . . ."

In short, the presentation in the *Principia* more or less follows the line of discovery, and remains (in this respect) unaltered in all three editions of the *Principia*.⁷⁰ It is the same, also, in the versions of the tract *De motu . . .*,⁷¹ which he wrote out just before composing the *Principia*. The key is the area law, which Hooke either did not know, or of which he did not appreciate the significance; there was also required

⁶⁷Whiteside: *op. cit.*, p. 134.

⁶⁸Hooke to Newton, 6 Jan. 1679/80; Newton, *Correspondence*, vol. 2, p. 309. This statement shows that Hooke was not a truly competent mathematician, since this law of motion does not lead to a law of force inversely proportional to the square of the distance, but to a wholly different law. See Whiteside: "Newton's early thoughts . . ." (note 61 *supra*), p. 135, n. 56; Koyré: "An unpublished letter . . ." (note 64 *supra*), p. 336, n. 118.

⁶⁹Hooke to Newton, 17 Jan. 1679/80; Newton, *Correspondence*, vol. 2, p. 313.

⁷⁰But Newton did rework the opening of Section II, Book I, so that the falsity of the speed law stated by Hooke would occur among the corollaries to Prop. I, Theor. I, rather than later on.

⁷¹These have been published by A. R. Hall and M. B. Hall, *Unpublished Scientific Papers* (cited in note 46 *supra*), and also in John Herivel, *Background* (cited in note 59 *supra*).

a high degree of mathematical insight and skill (which Hooke lacked).

The role of Kepler's laws in the development of Newton's celestial mechanics thus becomes clear, and we must accordingly be wary lest we throw away the keys to discovery on the grounds of an apparent logical inconsistency. But we must, of course, be careful to note that Newton had not really dealt with "the Planets in their Orbs . . . [about the] Sun", as he implies in his autobiographical statement, but rather had dealt with a single point-mass in a central gravitational field. Newton was rather careful about this point in the *Principia*, and in the tract *De motu*, there clearly distinguishing between "mathematical principles" and "natural philosophy". And, even after having proved that under certain restricted mathematical conditions Kepler's laws may be valid, he still had before him the enormous task of developing the mathematico-physical principles and demonstrations of the motions in the real world.⁷²

We may thus see the difference between the requirements of logical analysis, however historically based, and historical inquiry. Each sets problems for the other discipline, and each is illuminated by the findings

⁷²I have referred earlier to the unsolved historical problem as to when Newton became concerned about (1) the attraction of the Sun by the Earth and (2) the perturbing force of one planet upon another. By the time of the *Principia*, Newton was well aware of both factors and the result that not one of Kepler's laws could be a true statement concerning planetary motions in the real world. By 1684, when Newton wrote the tract *De Motu*, he must have been aware of some aspects of the lack of correspondence between Kepler's laws and the observed facts of planetary motion, since he stated unequivocally:

"By the displacement of the Sun from the centre of gravity it may happen that the centripetal force does not always tend to that immobile centre, and thence that the planets neither revolve exactly in ellipses nor revolve twice in the same orbit. Each time a planet revolves it traces a fresh orbit, as happens also with the motion of the Moon, and each orbit is dependent upon the combined motions of all the planets, not to mention their actions upon each other. Unless I am much mistaken, it would exceed the force of human wit to consider so many causes of motion at the same time, and to define the motions by exact laws which would allow of an easy calculation. Leaving aside these fine points, the simple orbit that is the mean btween all vagaries will be the ellipse that I have discussed already." Quoted from from A. R. Hall and Marie Boas Hall, *Unpublished Scientific Papers* (cited in note 46 *supra*), p. 281.

I believe, however, that he did not recognize the full implication of the mutuality of attraction of the Earth by the Sun and of the Sun by the Earth, until he actually got to work soon after, and wrote out the *Principia*. In any event, however, the correction or alteration of Kepler's laws must have followed the recognition of the general consequences of (1) having a gravitating real body, the Sun, at the focus of planetary orbits, and (2) considering the planets as true bodies with different masses, which attract one another.

and conclusions of the other. But even an apparently simple problem, such as the logical consistency (and hence inter-deductibility) of Kepler's laws and Newton's 'theory' may lead philosophers and historians to quite different conclusions, perhaps thus illustrating a degree of complementarity of the 'logic of discovery' and the 'logic of the discovered'.

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I have tried above to show the difference between a philosophical analysis such as Popper's and a historical inquiry. The results may be summed up in the observation that the philosopher examines the possibility that Kepler's laws of planetary motion may be deduced from (or even be valid in) Newtonian dynamics or celestial mechanics, whereas the historian studies how Newton actually made use of Kepler's laws⁷³ to develop a system in which these same laws are invalid without modification.⁷⁴ This difference in method does not denigrate the great value

⁷³And other laws and principles, too. As we have seen, furthermore, Newton no doubt encountered the law of areas some time after he had learned Kepler's other two laws of planetary motion; hence the order in which Newton became acquainted with Kepler's laws is important too.

⁷⁴In discussing the relation of Kepler's third law to Newton's celestial mechanics, Popper (*op. cit. note 5 supra*), suggests (pp. 32-33) that we can obtain the law

$$\text{from } \frac{a^3}{T^2} = m_0 + m_1$$

$$\frac{a^3}{T^2} = \text{constant}$$

"only under the assumption that m_1 is the same for all planets; or, if this is factually false (as is indeed the case, since Jupiter is some million times larger than the smallest planets), that the masses of the planets are *all zero as compared with that of the sun*, so that we may put $m_1 = 0$, for all planets."

He then observes: "This is quite a good approximation from the point of view of Newton's theory; but at the same time, putting $m_1 = 0$ is not only strictly speaking false, but unrealizable from the point of view of Newton's theory." The reason is: "A body with zero mass would no longer obey Newton's laws of motion."

But Popper does not at all explore even the possibility of the reverse process, in which Newton begins with

$$\frac{a^3}{T^2} = \text{constant}$$

and in which $m_0 = 0$ and $m_1 = m_2 = m_3 = \dots$; that is, in which the planets are equal non-interacting point-masses and the center of orbits is not a body but a mathematical point. Into the equation

$$\frac{a^3}{T^2} = \text{constant}$$

Newton introduced assumptions by which (1) $m_0 \neq 0$ and (2) $m_1 \neq m_2 \neq m_3 = \dots$ so that the 'constant' was constant no longer but had a value depending on both the mass of the Sun m_0 and the mass of each planet m_1 .

(to which I have referred again and again in this paper) which Popper's special analysis has for historians of scientific thought.⁷⁵

I have already mentioned the way in which Popper's work alerts the historian to the dangers inherent in an overly facile view of Newton's celestial dynamics being 'deduced' (in the current sense of the word) from Kepler's. I believe my own presentation of Newton's steps reinforces Popper's view of the supreme importance of creative ideas in the advance of science.⁷⁶

In one sense the sub-title of Popper's book, *Conjectures and Refutations*, may be misleading: "The growth of scientific knowledge" must not be taken to imply that Popper is presenting case histories of such growth. Rather he has made an analysis of science by setting forth standards for "the progress of scientific knowledge" and by comparing various states of knowledge with respect to conjectures and hypotheses and their rejection or refinement by critical experimental tests. This differs greatly from the historian's quest to learn the stages by which science as a whole (or the mastery of a part of science by an individual scientist) may have advanced from one level of knowledge to another.⁷⁷

Nevertheless, the historian of scientific ideas can be grateful to Karl Popper for reminding him of the importance of a critical approach to his subject. All too many historians (and even philosophers and scientists) have spoken too glibly about some kind of Newtonian 'synthesis', as if Newton had produced his revolutionary system by a sort of logical glue by means of which Galileo's laws of falling bodies and laws of projectile motion, Kepler's laws of planetary motion, Descartes's concept of motion as a 'state', and much else, were fused to yield a new dynamics and celestial mechanics, in which—somehow or other—the resulting whole became greater than the mere sum of the parts. Historians, as well as historically minded philosophers, who read Popper's analysis carefully will be on the alert to avoid such easy generalizations,

⁷⁵It is therefore to be regretted that so few practicing historians of scientific ideas are familiar enough with Popper's writings.

⁷⁶In *The Logic of Scientific Discovery* (cited in note 41 *supra*), p. 314, Popper even refers to "metaphysics (which from a historical point of view can be seen to be the source from which the theories of the empirical sciences spring)."

⁷⁷I alert the reader to the fact that in this article I have discussed only a few aspects of Popper's philosophy. For instance, I have not said a word about such major topics in his philosophy as falsification or testability.

for surely no historian or philosopher would propose a synthesis which contradicts its component parts.⁷⁸

No historian would seriously wish to propose that Newton had made a synthesis which contradicted (or was logically inconsistent with) the laws and principles he had allegedly synthesized. Popper's analysis thus demands that historians no longer discuss the 'Newtonian synthesis' (or any other alleged 'synthesis') without making qualifications. In this instance, Karl Popper's analysis not only corrects an over-simplified and misleading view of a major historical event, but focuses attention on the creative aspects of true history, in which logic alone could not have produced Newton's ideas out of Galileo's and Kepler's. All historians of scientific ideas are indebted to Karl Popper for this lesson.⁷⁹

⁷⁸A contradiction of this sort is very different from the Hegelian-Marxian dialectic of thesis and antithesis producing synthesis: since in that case the synthesis is a resolution of contradiction by rejection. But the proponents of the alleged Newtonian synthesis assume that the Keplerian elements of the synthesis are not rejected but remain valid.

⁷⁹I do not wish to enter here into the domain of philosophical controversy, but two examples may show how Popper's analysis of Kepler and Newton has been treated by philosophers. First, Imre Lakatos has given a summary of the events as follows:

"Newton first worked out his programme for a planetary system with a fixed point-like sun and one single point-like planet. It was in this model that he derived his inverse square law for Kepler's ellipse. But this model was forbidden by Newton's own third law of dynamics, therefore the model had to be replaced by one in which both sun and planet revolved round their common centre of gravity. This change was not motivated by any observation (the data did not suggest an 'anomaly' here) but by a theoretical difficulty in developing the programme. Then he worked out the programme for more planets as if there were only heliocentric but no interplanetary forces. Then he worked out the case where the sun and planets were not mass-points but mass-balls. Again, for this change he did not need the observation of an anomaly; infinite density was forbidden by an (inarticulated) touchstone theory, therefore planets *had* to be extended. This change involved considerable mathematical difficulties, held up Newton's work—and delayed the publication of the *Principia* by more than a decade. Having solved this 'puzzle', he started work on *spinning balls* and their wobbles. Then he admitted interplanetary forces and started work on *perturbations*. At this point he started to look more anxiously at the facts. Many of them were beautifully explained (qualitatively) by this model, many were not. It was then that he started to work on *bulging planets*, rather than round planets, etc." Quoted from pp. 135–136 of his contribution, "Methodology of scientific research programmes", in the volume *Criticism and the Growth of Knowledge*, Imre Lakatos and Alan Musgrave, eds., Cambridge University Press, Cambridge, 1970.

The first two sentences are certainly correct, and the remainder plainly conveys the idea of Newton advancing stage by stage; but it is far from certain that each of the steps mentioned by Lakatos was as separate and distinct as he would have us believe. Nor can the order he proposes be guaranteed by documentary evidence,

With regard to the reference to Imre Lakatos in my note 79, the editor of this volume informs me that there is a contribution by Lakatos, together with the discussion it aroused, elsewhere in the present volume.

The article, "The Aim of Science," which is discussed by me, has just been reprinted with some emendations in Karl R. Popper: *Objective Knowledge, An Evolutionary Approach*, Clarendon Press, Oxford, 1972, pp. 191–205. A comparison of the two versions shows that the later one differs from the first printing only in matters of stylistic detail and expression, and that there is no substantive change. It is certainly to be hoped that the availability of this essay as part of a book may give it the wider currency it deserves.

In the same volume, pp. 341–361, an English translation is given of a lecture which Popper gave in August 1948, and which was published

especially the events that allegedly "delayed the *Principia* by more than a decade." Observe that Lakatos does not even refer to the logical incompatibility of Newtonian dynamics and Kepler's laws. (In the above-mentioned volume, John Watkins, on pp. 30–31, refers to Popper's article and concludes—despite the evidence of the *Principia*, which he in fact cites—that "Newton . . . seems to have been far [!] from regarding the Keplerian system as having failed in any way.")

In another recent discussion of this topic, Paul Feyerabend has written: "Not only does Newton's theory transcend the domain of observation; it also contradicts the observational laws that were available when the theory was first suggested. It is therefore quite impossible to obtain it by inductive generalization, which leaves the 'facts' unchanged; and if it *did* seem possible to obtain it in this fashion, then this was due to the omission from the argument of some essential premises."

In context, Feyerabend is using this example to show that Newton's "law of gravitation", though introduced "in a manner which suggests that it was a direct consequence of his own inductivist rules of procedure", illustrates rather "a procedure of suppression and unwitting concealment" that enables "the gulf [to] be bridged that existed between the theory and the philosophy allegedly responsible for its invention." I shall refrain from any discussion of Feyerabend's critical presentation of either the alleged method of induction or the alleged empiricism of Newton's science, or—for that matter—of the science of the seventeenth century and of later centuries, including ours. See p. 155 (and n. 44) of his article, "Problems of empiricism", in Robert G. Colodny, ed.: *Beyond the Edge of Certainty*, Prentice-Hall, Englewood Cliffs, New Jersey, 1965 (vol. 2 of University of Pittsburgh Series in the Philosophy of Science). In another article, in Herbert Feigl and Grover Maxwell, eds.: *Minnesota Studies in the Philosophy of Science*, vol. 3 (1962), Feyerabend admitted (p. 46) that he was "aware that, from a historical point of view, the discussion . . . is not adequate." However, he was "here interested in the systematic aspect," and he therefore allowed himself "what could only be regarded as great liberties if the main interest were historical." Despite this disclaimer, this article, and others by Feyerabend, are of great value for the historian of ideas, not for historical information but rather for the philosophical insights into the nature of science which help to cast light on problems in the formation and development of scientific ideas.

in German in 1949. Entitled "The Bucket and the Searchlight: Two Theories of Knowledge", this paper had as its purpose "to criticize a widely held view about the aims and methods of the natural sciences, and to put toward an alternative view." Section X (pp. 357-358) states briefly the major points concerning Kepler's laws and Galileo's law of free-falling bodies in relation to Newton's theory.

MACH'S CONCEPTION OF THOUGHT EXPERIMENTS IN THE NATURAL SCIENCES*

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Experimentation in thought is an indispensable precondition for the execution of any physical experiment. All the same, a close analysis of the expression "experimentation in thought" proves to be singularly difficult to circumscribe and clarify in an orthodox and concise manner. The untidiness associated with "thought experiment" gives rise to questions that are less manageable for the historian and philosopher of science than the untidiness that surrounds "theory" as such and "experiment" as such. One of the principal reasons for the problematic status of the "thought experiment" is that written scientific records typically furnish much richer and more detailed contextual evidence about physical experiments and theories, than about what goes on either in the mind or in the external environment of the scientist who is engaged in "thought experiments" that relate to the physical experiments and scientific theories. Seen as a record of positive accomplishment it is understandable, at least in retrospect, that a science, *qua* science, will be analyzed and appraised as end-result primarily from the perspective of a theory-experiment language.

Along the restricted axis of bias of a theory-experiment analysis, top priority generally is given to logical consistency, phenomenological relevance, inclusiveness, in addition to other less tangible attributes such as predictability, simplicity and formal elegance. But the historian, philosopher, psychologist and sociologist of science are attentive, as well, to the merits of examining science as an intellectual and social process that teaches something important about the behavioral patterns of individual scientists and communities of scientists. Conceptions and suppositions about prevalent, acceptable and effective methodological atti-

*Author's Note: It is a pleasure to acknowledge the support of the National Science Foundation for research on the life and work of Ernst Mach.

tudes and techniques for scientific discovery, in fact, hinge not merely on what ultimately gets boxed off as "science," "theory," or "positive experimental evidence"; for at another level of inquiry, more or less removed from the theory-experiment syndrome, and considerably more bothersome to ferret out, exists the rich spectrum of productive stratagems and personal dispositions—inferential, heuristic, dialectical—that inevitably enter into all creative scientific effort. The vocabulary used to characterize this latter outlook on scientific investigation includes such equivocal and insecure denominators as analogy, association, memory, phantasy, volition, instinct, perception, contemplation, conjecture, symbolization, visualizability.

Although such psycho-physiological external features of scientific activity can be ignored and mostly eliminated (rightly so) from rationally reconstructed scientific theories, they cannot, in general, be ignored or eliminated in any investigation that focuses upon methodological questions about *how* scientific knowledge is acquired and *how* scientific theories are generated. To blur the distinction between the rational reconstruction of science and the historical analysis of the methods of science is overt historical treason and covert philosophical deception. To camouflage the two programs and lump them together within the same department is tantamount to obfuscation, and—what is worse—to throwing away an opportunity to explore and elucidate a cardinal historico-philosophical question; *videlicet*: What does the scientist do and how does he behave when he is doing science, or when he says that he is doing science or that he is behaving in a scientific manner?

That is a question that is easy to pose but incredibly difficult, even presumptuous, to try to answer. So let us be more modest in our goals and merely assume that an examination of the role of the intellect in planned experimentation can be, for the history of the philosophy of science, something more than an engaging intellectual exercise. If undertaken in a systematic way (as cannot be done in this short paper) such an historical examination might serve in a preliminary way as a constructive counter-irritant to recent discussions on the function of the history of science in the rational reconstruction of science. Whatever else one might wish to say about scientific experiments *a propos* rational reconstruction they cannot merely be served up as disjunct preludes and/or codas to garnish scientific theories. We note, then, that the

emphasis invariably falls too narrowly on examining the role that physical experiments perform in helping to generate a theory or that physical experiments play as predictions made from the theory.

It is meaningful, it seems, to examine, *exempli gratia*, such external methodological aspects of scientific investigation as "thought experiments," since they are woven into the theory at both ends: i.e., they are woven into the *process* of coming (in genesis) and going (in prediction), and do not necessarily leave any permanent traces. It is not alone the product of scientific investigation but also the *process*, that attracts the attention of the historian of science and the philosopher of science.

That was in fact Mach's chief preoccupation as a historian-philosopher of science. For one of Mach's prime objectives was to demonstrate historically that the conceptual products of science are always incomplete. At any one time they take on forms that reflect the particular historical circumstances, the mode of investigation, and the focus of attention of the individual scientific investigator. The historical analysis of scientific constructs that engaged Mach's attention, therefore, was one, hopefully, that would disclose something significant about the process of science and the mode of cognitive organization of experience—even where the process and the mode of organization have contributed little or nothing new to what is known.

Mach, of course, recognized that scientists normally place considerable emphasis on internal logic and consistency, maximum comprehensiveness, simplicity, and so on. Nevertheless, when examined within the historical context, scientific concepts, laws, and theories—he discovered—do not exhibit the neat logical features that are supposed to represent the traditional bench marks of science. Rather, what stands out predominantly is a strong element of historical fortuitousness. With the passage of time, what is accidentally (historically) acquired is converted into the philosophically argued. For this reason, Mach felt that it was imperative for the scientist to recognize and counteract the insidious gradual practice by which scientific constructs come to be regarded as philosophically necessary rather than historically contingent. Philosophical necessity, here is paired with rational reconstruction; historical contingency with the analysis of process and method.

As far as I know, Mach's writings contain the first explicit and significant statement of the status of thought experiments in the natural

sciences. We want to examine some of his ideas briefly.¹ In general, Mach's expression "Gedankenexperiment" appears within the context of his historical and philosophical reflections on how mental deliberations can suggest, initiate, and foster, for the practicing scientist, innovative tactics for experimental investigation and experiment-suggestive theoretical investigation. Although provocative, his arguments taken together are presented in an unorganized and sometimes ambiguous way. In this paper it is my intention partly to clarify Mach's general conception of Gedankenexperimente, but primarily to focus on his views concerning the didactic value of methodically encouraging students of science to engage in thought experiments as a normal part of a scientific investigation. At the same time we shall see, within a broader context, how Mach's treatment of the Gedankenexperiment squares with his overall perspective upon, and interpretation of, the sorts of problems the scientist encounters as soon as he begins to puzzle about the nature, method and limits of his own branch of knowledge. Thus, Mach's position can serve as a point of departure for raising some significant epistemological problems for discussions of the mode of interaction of experiment and theory in science; but such issues will barely be touched upon in this paper.

Early in his teaching career Mach gave serious thought to questions about man's inborn propensity to engage in problem-solving activities. I have attempted elsewhere to characterize Mach as a scientist who, when deeply perplexed philosophically about the nature of his own scientific discipline and deliberations, turned to the study of the history of science for an explication of the puzzles that he encountered.²

Scientist-historian-philosopher that he was, Mach's life can be viewed as a progressive struggle for the mental clarification of scientific puzzles. Indeed, an examination of his writings (both reflective and scientific), as well as his personal correspondence, reveals that Mach devoted an extraordinary amount of effort to solving problems of one sort and another. He was notably fond of formulating and attacking tough, knotty problems that could be resolved, if at all, only by a variety of unorthodox maneuvers (like invoking historical precedent) that require a

¹A more thorough examination of the role of the Gedankenexperiment in Mach's philosophy of scientific investigation is reserved for another paper.

²"Mach's Philosophical Use of the History of Science," in *Historical and Philosophical Perspectives in Science*, edited by Roger Stuewer, Minneapolis, 1970, pp. 184-202.

great deal of personal mental input on the part of the investigator. Mach would have indorsed with gusto Piet Hein's saying: "Problems worthy of attack prove their worth by hitting back."³

In 1872 Mach wrote: "There are only two ways of reconciling oneself with actuality: either one grows accustomed to the puzzles and they trouble one no more, or one learns to understand them by the help of history and to consider them calmly from that point of view."⁴ Examining the role of problem-solving in the learning process, from the perspective of a physics teacher, Mach concluded that students should not be expected, without the study of history, to understand propositions that had cost several thousand years' labor of thought [Gedankenarbeit]. "There is only one way to [scientific] enlightenment: historical studies."⁵

From 1867 to 1895 Mach occupied the chair for experimental physics at the Charles University in Prague. In 1887, together with G. B. Schwalbe from Berlin, he launched a new pedagogically oriented journal —*Zeitschrift für den physikalischen und chemischen Unterricht*—that was intended as a forum for discussing the goals and methods of instruction in the physical sciences. The *Zeitschrift* contained articles, notes and book reviews dealing with new experimental methods and apparatus, lecture-room and laboratory demonstrations, news of the profession of interest to physics and chemistry teachers, and historical materials considered to be of didactic value.

As an additional feature each issue of this journal carried a section with a list of problems under headings such as *Physikalische Aufgaben* or *Physikalische Denkaufgaben oder Denkfragen*. These "thought problems" and "thought questions" were deliberately formulated with the intent of furnishing the student with an opportunity to exercise his creative potential in a way that was not prescriptive within the rigorously organized educational framework of the German and Austrian secondary schools and universities. This was because Mach, and his scientific colleagues, took a rather dim view of the prevalent use of cook-book style

³Piet Hein, *Grooks*, Copenhagen, 1969, p. 2.

⁴"[est gibt] aber zwei Wege, sich mit der Wirklichkeit auszusöhnen. Man gewöhnt sich an die Rätsel und sie belästigen uns nicht weiter. Oder man lernt sie an der Hand der Geschichte verstehen, um sie von da aus ohne Hass zu betrachten." *Die Geschichte und die Wurzel des Satzes von der Erhaltung der Arbeit*, Prague, 1872, p. 1.

⁵*Ibid.* pp. 1-2.

laboratory experiments that had been contrived so as to enable the student to arrive unequivocally at the expected end result. It also was felt, that little or no value was to be gained by having students work the unimaginative numerical problems that customarily were appended to the chapters of textbooks conceived along more or less dogmatic guidelines. An examination of the *Denkaufgaben* in the *Zeitschrift*, many of which were formulated by Mach himself, reveals close parallels with the types of examples later given by Mach to illustrate his conception of Gedankenexperimente, or of what he on occasion referred to as "das Experimentieren mit Gedanken."

Mach first devoted an essay to the subject of thought experiments in the *Zeitschrift* in 1897.⁶ Central to his analysis of this topic in the essay is the resolute conviction that an instinctive and unrehearsed inclination to physical experimentation is native to man—so much so, that it permeates all activities, scientific and otherwise. Mach notably sought his evidence for this opinion in the study of the behavior of young children and young animals. To them he attributed an unpremeditated and inborn mode of experimentation that he designated as the "method of variation." By this he understood the innate ability of living organisms to progress from the experienced to the experimentally unknown by varying the conditions of the act of behavior. The inclination to experiment and the method of variation Mach took to be inborn, whereas the resultant of this spontaneous unlearned behavior is, of course, something that is learned.⁷ Adults, Mach argued, are obliged to "rediscover" many matters of information because they frequently have been brought up within the narrow circle of interests of a given society and, being thus confined, acquire the habit of adopting a multitude of ready-made views that are assumed to lie beyond the need for further verification. Not infrequently, they discover what they thought they already knew.

Apart from the properly so-called physical experiments referred to here, Mach sees "another [experiment] that is practiced largely at a higher

⁶"Über Gedankenexperimente," *Zeitschrift für den physikalischen und chemischen Unterricht*, 10 (1897), 1-5. A considerably expanded version of this essay, under the same title, was reprinted in Mach's *Erkenntnis und Irrtum*, Leipzig, 1905, pp. 183-200.

⁷Mach's *Erkenntnis und Irrtum*, and his *Die Analyse der Empfindungen*, besides the personal correspondence, are notably rich sources of information about experiments carried out by Mach on himself, his children, and the animals around the Mach household.

intellectual level—the thought experiment." There is no doubt, says Mach, "that the thought experiment initiates the greatest transformations in our thinking and opens up the most consequential methods of research."⁸ But this faculty of experimentation in thought is no birth-right of the intellectual or the scientist. It is universally recognizable at all levels in the analysis of man's thought patterns. The schemer, the builder of castles in the air, and the architect of social and technical utopias is engaged in thought experiences (*Gedankenerfahrungen*). This experimenter will envision some contextual circumstances and connect these with representations, expectations, and the conjecture of particular consequences. The ingredients of thought may be juxtaposed in fantasy in ways not known to accord with reality. Inconsistent inferences may be drawn.

On the other hand the respectable merchant, the inventor, and the researcher likewise engage in thought experiments, but may acquire conceptions that are a good likeness of the facts (*gute Abbilder der That-sachen*) and thus remain close to reality in thought. Mach writes: "Indeed, the possibility of [making] thought experiments rests upon the more or less accurate involuntary illustration (*Abbildung*) of the facts in our conceptions (*Vorstellungen*). Just as we are able in our recollection to count the strokes of the clock where this was neglected during the striking, and just as we can perceive particulars in the after-image of a lamp that escaped our attention while directly viewing it, and just as we discover in recollection a trait that suddenly discloses the hitherto misunderstood character of a person, so also in our recollection we can retrieve novel hitherto unnoticed properties of physical facts, or make discoveries."⁹ Like the physical experiment, the thought experiment thrives on the method of variation. By altering the circumstances (continuously wherever possible) the sphere of applicability of the mental representations or anticipations are modified, specialized and extended.

If experimentation in thought is of such immense importance for the enhancement of the cognitive process, then how, asks Mach, is this to be inculcated into the learning process? A positive step in the right direction is taken by systematically encouraging students to predict (guess) the likely outcome of experimental situations (*Versuchsanordnungen*).

⁸*Op. cit.* (*Zeitschrift*) above (note 6), p. 2.

⁹*Ibid.*, p. 1.

Most students, of course, will predict what commonly lies nearest in associative thought; but some will entertain amazingly singular solutions. "As the slave in Plato's Menon believes that doubling the sides of a square also doubles the area of the square, so one easily hears from the elementary student that doubling the length of a pendulum will double the period, whereas the more advanced student will make analogous but less obvious mistakes. Precisely these mistakes eventually will sharpen the feeling for differences between the logical, the physical, and what is associatively determined or nearest in thought. The student will learn to separate what can be guessed (*das Erratbare*) from what cannot be guessed."¹⁰

The habit of experimenting in thought, almost as an unrestrained behavioral reflex action, can be promoted in the student of science, thinks Mach, by demonstrating and analysing paradoxes. Such paradoxes are illustrated by numerous examples in the Denkaufgaben of the *Zeitschrift*. Not only does the student learn to sense the nature of a scientific problem through an analysis of its paradoxical elements (indeed, it is the paradoxical content that constitutes the problem), but the contradictory character of the problem furnishes the incentive for engaging in Gedankenexperimente. It is the paradox, the puzzle, the perplexity of the problem, that keeps the thoughts from coming to rest. A recognition of the expectations associated in thought with the given circumstances of the problem contributes to a state of unresolved disquietude, that nevertheless provides the driving force for working through to some measure of clarification or partial solution of the problem.

But why so much emphasis in science upon experimentation "in thought"? The principle of economy of thought (Denkökonomie) lies close to the surface of Mach's deliberations about Gedankenexperimente. He indicates that our mental conceptions are more conveniently at hand than the physical facts. It costs so much less effort to experiment in thought than in fact, that thought experiments necessarily precede physical experimentation. And the more the experimentalist thinks before he leaps to act, the more will his physical experiments be "thought-experiment"—laden. In some cases, the outcome of the chain of thought experiments may be so definite, decisive and continuous that the author

¹⁰Ibid., p. 4.

—right or wrong—may find it unnecessary to carry out a physical experiment to test his conclusions. More commonly, however, the outcome of the thought experiment is so indefinite, even precarious, that it becomes evident that a physical experiment must be undertaken in order to plot the strategy of any further experiment, whether in thought or in act.

Hence it is clear, from Mach's illustration of the rich see-saw reciprocity between "experiment in thought" and "experiment so-called," that he encourages the scientist to explore and exploit the potential world of Gedankenexperimente as a precondition for undertaking any physical experiment at all. At least, such an approach, with the Gedankenexperiment as the *terminus a quo* for a scientific investigation, will stand out as the preferred move at certain nodal points in science. We see here that vulgar experimentalism or crude empiricism are conceived to be just as sterile, in Mach's opinion, as fruitless metaphysical speculation born of thought experiments gone wild. It is clear that while science for Mach was something of a game constituted of puzzles seen to be worth resolving, it was, nevertheless, a game the requisite finesse for which was acquired mainly through superior, first-hand experience in carrying out Gedankenexperimente, in conjunction with physical experiments informed by Gedankenexperimente.

The division of mental and experimental labor that enters into Mach's formula for optimum overall economy in scientific investigation is, of course, not spelled out. For obvious reasons: first, although the methods that characterize scientific investigation are not seen to differ, in principle, from the methods employed in any other human endeavor, there nevertheless are enormous differences in the way in which, and the extent to which, mental and experimental activities enter in. Second, science is taken by Mach to be such a many-sided—infinitely versatile—quest for truth, that there can be no rigid and unambiguous methodological prescriptions for the acquisition of new knowledge. *A priori*, that is, there is no discernible rationale for scientific discovery in physics, and certainly no recipe for the mix of mental and experimental labor that will give rise to scientific novelty, or that will add an increment of understanding to a problem under investigation. What turns out to be discoverable can always somehow be rationalized into the existing framework, but mostly in retrospect.

Finally, since Mach views science primarily as an eminently intellectual and humanizing enterprise, his attention is at least as much focussed on the science of the past and present as upon future discovery. This being so, it is inevitable that Mach's conception of the Gedanken-experiment is given first rank importance. Always in the background, for Mach, is the expectation that the history of science—the Gedankenarbeit of many centuries—will help the investigator most to understand the scientific puzzles of the here and now. If you cannot simply become accustomed to the enigmas of science—as Mach could not—then there is no other option to achieving a modicum of mental comfort than to analyze the enigmas with the help of history. If Mach had a formula, as philosopher of science, then it was this: to make use of history in order to illuminate the philosophical puzzles associated with the growth of science. He was determined, in his teaching, to convey this technique to his students at all costs.

FINDING FAVOR WITH THE ANGEL OF THE LORD:
NOTES TOWARD THE PSYCHOBIOGRAPHICAL
STUDY OF SCIENTIFIC GENIUS*

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I. INTRODUCTION: IN THE "TEMPLE OF SCIENCE"

Historians of science return again and again to the epochal contributions of the Newtons and Niels Bohrs, the Darwins and Freuds. Although the history of science is not primarily the study of the work of "genius," historians cannot avoid encountering at every turn the primary or secondary effects of a few extraordinary, transforming works. At the same time, men and women at that level of achievement are the most puzzling ones.

What is meant by genius in science? What are its characteristics? What may be the sources of that awesome strength which usually is dealt with merely by applying the label "genius"? Can one understand it, or is that a contradiction in terms? I am not speaking merely of "creative" people, nor of men of "high attainment." I am aware of the large amount of literature on creativity, and some fine studies of men of genius in the arts or in political affairs. But I do not find them very helpful for understanding the life or the work of a Fermi or an Einstein, and even less for discerning how his personality and his scientific achievements interact.

Einstein himself pointed to one difficulty with such a study: it may be hard to find commonalities among many cases from which to gain

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more understanding about a specific case. In a remarkable address, given in 1918 in honor of Max Planck (entitled "Motiv des Forschens" and later mistranslated as "Principles of Research"), Einstein gave us what is perhaps the best autobiographical insight into his own view of the motivation for doing research in science. Later, the analyses in the following pages may help in interpreting these charming but intensely charged passages, and in turn will receive illumination from them; to begin with, however, let us take careful note of Einstein's own view through this summary of his essay (translation of key passages given in quotes):¹

"The Temple of Science," Einstein begins, "is a multi-faceted building." In it, many engage in science out of joy in flexing their intellectual muscles, or for utilitarian ends. These are useful persons, to be sure, although external circumstances could easily have made them into engineers, officers, etc. If only such scientists existed, "the Temple would not have arisen."

"If there now came an Angel of the Lord to drive these persons out of the Temple," few scientists would be left in it. But one of them would be Planck, "and that is why we love him."

Now let us turn to those "who found favor with the Angel." They are mostly "rather odd, uncommunicative, solitary fellows, who despite these common characteristics resemble one another really less than the host of the banished."

"What led them into the Temple? The answer is not easy to give, and can certainly not apply uniformly. To begin with, I believe with Schopenhauer that one of the strongest motives that lead men to art and science is flight from the everyday life with its painful harshness and wretched dreariness, and from the fetters of one's own shifting desires. One who is more finely tempered is driven to escape from personal existence and to the world of objective observing [Schauen] and understanding. This motive can be compared with the longing that irresistibly pulls the town-dweller away from his noisy, cramped quarters and toward the silent, high mountains, where the eye ranges freely through the still, pure air and traces the calm contours that seem to be made for eternity.

¹Based on the essay published in English translation in *Ideas and Opinions* by Albert Einstein, Crown Publishing Co., New York, N.Y., 1954, pp. 224-227.

"With this negative motive there goes a positive one. Man seeks to form for himself, in whatever manner is suitable for him, a simplified and lucid image of the world [*Bild der Welt*], and so to overcome the world of experience by striving to replace it to some extent by this image. This is what the painter does, and the poet, the speculative philosopher, the natural scientist, each in his own way. Into this image and its formation he places the center of gravity of his emotional life, in order to attain the peace and serenity that he cannot find within the narrow confines of swirling, personal experience."

The theoretical physicists' picture of the world [*Weltbild*] is one among all the possible pictures. It demands vigorous precision in the description of relationships. Therefore the physicist must content himself from the point of view of subject matter with "portraying the simplest occurrences which can be made accessible to our experience"; all more complex occurrences cannot be reconstructed with the necessary degree of subtle accuracy and logical perfection. "Supreme purity, clarity, and certainty, at the cost of completeness."

Once such a valid world image has been achieved, it turns out to apply after all to every natural phenomenon, including all its complexity and its completeness. From the general laws on which the structure of theoretical physics rests, "it should be possible to attain by pure deduction the description, that is to say, the theory of every natural process, including those of life, if such a process of deduction were not far beyond the capacity of human thinking. To these elementary laws there leads no logical path, but only intuition, supported by being sympathetically in touch with experience [*Einfühlung in die Erfahrung*]." It is true that this uncertain methodology may in principle give rise to many systems of theoretical physics with equal claim, but in fact it has turned out that at any time just one such system is generally accepted to be decidedly superior. "Though there is no logical bridge from experience to the basic principles of theory," in practice it is agreed that "the world of experience does define the theoretical system uniquely. . . . This is what Leibnitz termed happily 'pre-established harmony.' Physicists accuse many an epistemologist of not giving sufficient weight to this circumstance."

"The longing to behold that pre-established harmony is the source of the inexhaustible perseverance and patience with which Planck [or,

we may add here, as throughout, Einstein] has given himself over to the most general problems of our science, not letting himself be diverted to more profitable and more easily attained ends. I have often heard colleagues trying to trace this attitude to extraordinary will power and discipline—in my opinion, wrongly. The state of feeling [Gefühlszustand] which makes one capable of such achievements is akin to that of the religious worshipper or of one who is in love; his daily striving arises from no deliberate decision or program, but out of immediate necessity. . . .”

II. SINGULARITIES

It seems clear from these metaphoric passages that Einstein would not have been sanguine about a more detailed analysis of the nature of scientific genius. Leopold Infeld, who worked for many years closely with Einstein, dismissed entirely the possibility of giving a definition of genius, for “it is characterized just by the fact that it escapes classification.”² Most scientists today would probably agree with him.

Yet, I do not think the matter is altogether hopeless. On the contrary, it is precisely the attempt to seek some clues through the study of scientific publications, letters, and biographies of such a scientist that has given me the ambitious topic. But I should warn at the outset that we shall not aim for final answers. Rather, we shall be satisfied if we can open the topic wider, and find interesting questions spread over several specialty disciplines. Moreover, what I have to say may or may not be applicable to other scientists—here I shall discuss only one person.

The first temptation is to proceed reductionistically, and to analyze the man of genius into externally visible, singular elements of his work and character. Those of us who have worked with such a person will have caught glimpses of such elements. The first is undoubtedly his insight into the phenomena of science in a way that amounts almost to a special perception of a kind that can hardly be communicated to others, or a tactile coexistence with natural phenomena: sometimes the mind seems to move into the problem of nature as if it were a hand slipping into a glove. Another element may be his clarity of thought as shown by the penetration of his questions, and by the simplicity and ingenuity of his

²L. Infeld, *Albert Einstein*, Charles Scribner's Sons, New York, 1950, p. 118.

Gedankenexperimente—experiments carried out in thought in just the idealized *milieu* that turns out to be needed. Third, one may be startled by the intensity and wide scope of his alertness, for example to small signals in the large “noise” of any experimental situation or of its description. One is likely also to be constantly impressed by his extraordinary energy and persistent dedication—in manipulation of equipment, in the making of apparatus or tools, in computing or writing. There is a marvellous overabundance evident in such a person, in a Kepler or a Gauss no less than in a Mozart. Connected with it is surely the ability to lend oneself—no, to give one's whole life over—to the development of a field or an area of thought, usually to the near exclusion of satisfactions or drives other men find irresistible.

And lastly, one is likely to perceive an aura or atmosphere surrounding such a person's actions and expressions, which sets him apart in a way difficult to define. It is not merely the sometimes unreasonable degree of optimism about his own mission, the self-confidence and self-reliance that appear to others at times to be egocentric obstinacy. Rather, I speak of a basic feeling that such a man has, and that may be shared by those who know him well: that he is, in some sense, one of the “chosen” ones.

These and other characteristics may be more or less adequate earmarks of genius in a particular case. They do apply to Einstein, for example. But I have no illusion that such a list of singularities explains anything. Quite apart from the question of whether any reductionistic approach of this kind can succeed, each of the elements themselves, except possibly the last one, seems to be continuous also to second- and third-raters. Moreover, these elements appear neither to be exhaustive nor to have convincing and necessary connections.

If we now ascend to the next more serious level, one may well be expected to turn to the methods of psychoanalysis or psychohistory. Some successful studies of men not in the sciences exist, although Frank Manuel has given us the one example of such a study of a physical scientist, in *A Portrait of Newton*. At least for the particular case I wish to discuss here, psychobiographical analysis will, in my opinion, be most fruitful when used in conjunction with all the other tools of the historian of science, rather than being made the central method. While the personality and work of the genius appear to me to be qualitatively different

from those of other scientists, I am not prepared, at least not yet, to think that our methodology and techniques of study must be significantly different from those used in more ordinary cases. Indeed, the most promising road seems to me this: first to identify some special puzzle or problem characterizing our understanding of the man of genius, and then to bring the whole range of the historian's professional tools to bear on this particular puzzle—with the hope that the special character of genius may be reflected in the special aspects of the solutions.

III. THE DIMENSIONS OF MODERN HISTORICAL SCHOLARSHIP

For our purposes, the distinction often made between the "externalistic" and "internalistic" approach to historical cases will be too simplistic, too blunt to reveal the necessary fine structure. Although many individual historians of science in most cases are largely, and quite correctly, still occupied predominantly along the chief traditional directions, a general awareness and tolerance has arisen that recognizes the existence of about nine somewhat overlapping but still sufficiently separable directions that most historical work can now take in principle. In my view we may go further, and say that the full potential of a major case is not likely to be exhausted until some attention has been paid to each of these components. For any event E in the history of science at a time t (for example, the announcement of a novel conception), the systematic list of chief elements that may sooner or later fruitfully engage the attention of historians of science would run somewhat as follows:

1. First stands, of course, the awareness of the state of *public* scientific knowledge at that point in time concerning the scientific "facts," data, techniques, technical lore concerning event E —both in the published work of a particular scientist who is being studied, and in the work of others in his field whom he may or may not have known about.

2. The establishment of the *time trajectory* of the state of public scientific knowledge, both leading up to and going beyond the point in time chosen above. This is, as it were, the tracing of the World Line of an idea, a line on which the previously cited element E is merely a point. Under this heading we are dealing with antecedents, parallel developments, and tracing of the acceptance or rejection of an idea. This is our stock-in-trade: telling the story of a conceptual development—though

perhaps too often at the exclusion of all other concerns, making it therefore appear as if scientific ideas had an independent life of their own.

3. The reconstruction of the less institutional, more ephemeral, *personal* scientific activity. (Let us call it S_1 , in contrast to the public and institutional state of science S_2 , cited above as the first element or component.) We are now looking at the same event captured in the letters, drafts, abandoned equipment, reminiscences, interviews, and perhaps in photographs and films. Here we deal with an activity that may be largely tacit, and not necessarily appreciated or well understood by the agent himself; hence our task is that of an archeologist working with scant clues.

4. The establishment of the *time trajectory* of this largely private scientific activity under study—the personal continuities and discontinuities in development. What I am pointing to here is the kind of development we shall discuss which brought Einstein from his preoccupation with magnetism as a young boy to the establishment of special relativity in 1905, then to the period of work on elaborating quantum theory followed by the elaboration of general relativity theory. Now event E at time t is seen and begins to be understood in terms of the intersection of two trajectories, two World Lines, one for public science (S_2), and one for private science (S_1).

5. Coupled to the trajectory of S_1 is the tracing of another line, the *psychobiographical development* of the scientist whose work is being studied. For all its growing vogue it is merely an aspect of historical studies that in some form has been urged since at least the days of Wilhelm Dilthey. No doubt this is among the more difficult but rewarding new areas, allowing one to seek correlations between the S_1 trajectory and the historico-psychological development. Whether one wishes to cast one's lot with the school of Freud or Erikson, Piaget, or others, or with several of them in part, this area will give rise to that most precious of commodities, new and interesting questions. For example, at least in the case of high achievement, it is worth pursuing the hypothesis that a person's public scientific work is an expression of this intimate style of thought and life, with contrasting, contradictory and seemingly non-rational elements not far below the surface.

6. A similar line may be traced that relates the *trajectory of the political and literary events of the time to the trajectories of S_1 and S_2* . We need merely mention the fact that Einstein (to his own astonish-

ment) became a charismatic figure, with influence far beyond physics, as a result of the 1919 experimental confirmation of his general relativity prediction concerning the deflection of starlight passing near the sun's disc. It is not unlikely that this mass response to what was perceived as a "revolution" of the old order in physics, seemingly certified by nothing less than the stars themselves, was to a degree prepared by the existence of political revolutionary situations in many parts of the world at the time. Similarly it has been argued recently that the widespread interest among German scientists in abandoning the principle of causality in the early 1920's (despite Einstein's opposition) is closely related to developments in the German intellectual environment, including the huge vogue then enjoyed by Oswald Spengler's pessimistic and intuitionistic book, *The Decline of the West*. On the whole I agree with the implication that in retrospect some of the seeds of destruction of the Weimar Republic may be discerned in studying this episode in the history of quantum physics. Conversely, the influence of the progress of physics upon political history, can also be documented, for example in the role of the nuclear reactor.

7. A seventh component of understanding fully an event *E* refers unavoidably to the *sociological setting, conditions, influences*—arising from colleagueship or the dynamics of teamwork, the state of professionalization at the time, the link between science and public policy or between science and industry, or from institutional channels for the generating, funding, evaluation and acceptance of scientific work. One partial expression of this seventh component may be found in the current attempts to construct empirical and quantitative measures for the progress of the sciences. But the qualitative aspects are at least as important. One could not, for example, study the great differences in the rates of acceptance of relativity theory or quantum theory in different countries, as two of my students have done, without studying the differences in the educational systems. Similarly, it would not be possible to understand fully the philosophical pilgrimage of Einstein from his early, declared devotion to Mach's philosophy, and toward his later acceptance of rational realism—unless one understands and makes provision for the influence of such colleagues as Max Planck.

8. The same story reminds us of course of another aspect, namely of the need for an *analysis of the epistemological and logical structure*

of the work under study. Here has been our chief contact point with the large variety of philosophers of science whose contributions, as we are all aware, have on occasion been illuminating.

9. Last but not least, one must bring out the individual scientist's *thematic presuppositions*. They motivate the research program, and they surface as quasi-aesthetic commitments that sometimes fly in the face of contrary experimental evidence itself. Since these thematic elements will play a major role in our study of the junction of scientific genius, at least a cursory survey of their role in current science is in order—selecting examples almost at random from a number of recent issues of physics review journals that happen to be piled on my desk.

IV. THE PERSISTENCE OF THEMATIC ELEMENTS

At the top of the pile of journals is a report of deep inelastic scattering, observed at the Stanford University linear accelerator, which has drawn comment from theorists such as Victor Weisskopf and Richard P. Feynman. The problem is: what is the structure of the proton—which to an historian of science sounds like a contradiction of terms. The current candidates are point-like hypothetical constituents, be they quarks, dions, partons, or stratoms. Two things are clear. One is the antiquity of this quest for an elementary, although perhaps protean, constituent of all matter, a quest that has made sense to scientists since Thales. It is nothing less than an *a priori* commitment which I have called "thematic," a scientist's individual commitment built into the very way he asks and answers questions in science. The other striking point is the heuristic approach, so typical in 20th-century science, that is being taken in this case. There are many doubts whether the hypothetical quark, for example, does exist. Perhaps it does not. But by assuming it, one can explain well some puzzling results; and so Weisskopf, skeptical but in a relaxed mood, is quoted as having told the story of Niels Bohr who visited a friend's house, noticed a horseshoe nailed over the door, and asked what it meant. His friend told him, "That brings luck." Bohr was astonished and said, "Do you really believe in this?" To which his friend replied, "Oh, I don't believe in it. But I am told it works even if you don't believe in it."

Almost invariably, for every thematically informed theory used in physics (or any science) there may also be found a theory using at least

one opposing thema, or antithema. Currently fashionable theories believe all hadrons to be dynamical constructs, satisfying self-consistency conditions. But there are also opposing publications insisting, for example, that nature exists in an infinite number of strata with different qualities among them, each of these strata being governed by its own laws of physics, and each always in the middle of creation and annihilation.³ Still another view of the matter is hinted at in publications such as G. F. Chew's, who has speculated that the way to enlarge current ideas in elementary particle physics is to break out in entirely new directions. (To quote him: "Such a future step would be immensely more profound than anything comprising the hadron bootstrap [approach]; we would be obliged to confront the elusive concept of observation, and, possibly, even that of consciousness. Our current struggle . . . may thus be only a foretaste of a completely new form of human intellectual endeavor, one that will not only lie outside physics but will not even be describable as 'scientific'.")

What seems certain to me is that regardless of temporary victories for one side or another, the dialectic process of this sort between a thema and its anti-themata, and hence between the adherents of two or more theories embodying them respectively, is almost inevitable, and perhaps among the most powerful energizers of research and direct influences on its evolution. If the past is a guide, this process will last as long as there are scientists interested in putting questions to nature and to one another.

Next I find in the journals a report of a conference of physicists, on fundamental interactions at high energy; it warns that "the fundamental interactions—gravitation, electro-magnetism, strong and weak CP-conserving and nonconserving interactions—still offer a dazzling challenge to the physicists, with their patterns of disparate strengths, their hierarchy of symmetry properties, and their universality still to be explained." P.A.M. Dirac opened the conference with the question "Can equations of motion be used?" Although agreeing that Heisenberg's 1925 view can be a good guiding principle (viz., that only observable quantities should be used in formulating a physical theory), he felt it nevertheless unlikely that the analytic S-matrix description would

be the final answer in high-energy physics. Some day we would be discussing equations of motion involving "objects" only remotely related to experimental quantities; in this sense there would be determinism, subject only to the usual quantum-mechanical measurement uncertainties. Dirac thought this prophecy would come to pass because of what he called a "feeling for the unity of physics," and because of the important role played by equations of motion in all other branches of physics. This confidence, somewhat in the face of current fashions and experimental evidence, led Dirac later to say: "A theory that has some mathematical beauty is more likely to be correct than an ugly one that gives a detailed fit to some experiments."

These quasi-aesthetic judgments are a form of thematic commitment that has deep psychological roots. It is frequently the basis for choices made in actual scientific work (for example, when some ad-hoc hypothesis is accepted and another is rejected, or when a whole approach to a scientific field is adopted or dismissed). Everyone knows that Heisenberg wrote to Pauli, "the more I ponder the physical part of Schrödinger's theory, the more disgusting it appears to me." At about the same time, Schrödinger in his turn wrote about Heisenberg's approach, "I was frightened away [by it], if not repelled. . . ." Long before Copernicus defended his theory as "pleasing to the mind," it had been an everyday fact in the life of scientists that some of the terms and attributes they use have great motivating power for them.

Further on in the latest issue of the journal *Physics Today*, I find the report of a most delicate analysis made on a meteorite that fell on Australia. A team assembled from several institutions reports finding 16 amino acids, at least 5 of which are common in living systems: they would constitute an essential part of any chain in the chemical evolution toward living forms. What excites interest most is the evidence of equal quantities of laevogyrate, lefthanded, and dextrogyrate, righthanded, amino acids in the samples. The facts that the chirality in these samples is of both kinds increases the likelihood by far that these amino acids are not merely contaminations from handling, for the amino acids in living things on earth, for reasons that are still entirely mysterious, are almost all lefthanded. Now it has become quite likely that the ideas of chemical basis of evolution are entering a new phase of elaboration; e.g. a search for clouds of amino acids in space becomes sensible.

³See the summary in Ne'eman's paper, in this volume.

Triggered in part by this finding and this way of thinking is another review by a physicist, which he entitled "Chirality, Broken Symmetry, and the Origin of Life." This title alone alerts us to a whole set of thematic elements that are basic to research in the major areas of physics today, as in the past: the efficacy of geometry as an explanatory tool; the conscious and unconscious preoccupation with symmetries; the use of the themata of evolution and devolution that might be taken perhaps from the ordinary life cycle but that have at any rate become fundamental tools of scientific thought (as much in psychological and sociological research as in genetics and astrophysics).

V. A PUZZLE OF POLARITIES

Keeping now in mind the full range of tools upon which we can call—and particularly these two: the studies of psychobiographical elements and of thematic elements—we are ready to turn to the details of the problem at hand. It arose out of a study on the historical origins of special relativity theory. In retrospect, I find that the most haunting questions during that study have not been those of the sort one usually encounters in the work of most other scientists—such as the trajectory of conceptual development, fascinating though this is. Rather, there is the existence of what first strikes one as a remarkable set of puzzling polarities, or, if you will, symmetries and asymmetries, in Einstein's style and life's work. Let us make a brief list of some of these apparent polarities.

The folkloric image itself is that of the wisest of old men, who even looked as if he had witnessed the Creation itself; but at the same time, he seems also still an almost childlike person. Einstein himself once said, in a remark that will take on some significance later, that he was brought to the formulation of relativity theory in good part because he kept asking himself questions concerning space and time that only children wonder about.

Then there is his legendary, iron ability to concentrate, often for years, on a single basic problem in physics, regardless of contemporary schools and fashions. Similarly, there is his stubborn faithfulness to a clearly established personal identity, characterized by uncompromising rejection of every *Zwang* and external arbitrary authority, in physics as well as in clothing or in the demands of everyday life. But opposite to

this glorious obstinacy and solitary intransigence with which to search for the basic permanence and necessity behind nature's phenomena, there is also his ever-ready openness to deal after all with the "merely personal" from which he so longed to flee—to deal with the barrage of requests for help and personal involvements that appealed to his fundamental humanity and his vulnerability to pity.

Closely related is another opposition (to express it from the viewpoint of the external observer, in the only language easily available to us, but a language that may well mislead rather than reveal). Einstein is of course known as a grand public personage, radiant and lively, with profound wit and charisma. But from early childhood to his late years he was at the same time also characteristically a solitary person. M. Talmey, who observed him often between his eleventh and fifteenth years, wrote later that he had never seen him in the company of schoolmates or other boys of his age, but that he was usually aloof, absorbed in books and music. In 1936 Einstein wrote in a short "self-portrait": "I live in that solitude which is painful in youth, but delicious in the years of maturity." Einstein could oscillate between these states of the public and private person.⁴ He once confessed: "My passionate sense of social justice and social responsibility has always contrasted oddly with my pronounced lack of need for direct contact with other human beings and human communities."

Then there is Einstein as the apostle of rationality, whose thought was characterized by an exemplary clarity of logical construction. On the other hand, there is the uncompromising belief in his own aesthetic sense in science, his warning not to look in vain for "logical bridges" from experience to theory, but to make, when necessary, the great "leap" to basic principles. As he wrote in a well-known passage, "To these elementary laws there leads no logical path, but only intuition, supported by being sympathetically in touch with experience [*Einfühlung in die Erfahrung*.]"⁵

⁴Such an "oscillation" is described, for example, in Antonina Vallentin, *Einstein: A Biography*, London, 1954, p. 27.

⁵In the lead essay in the collection *Science et Synthèse*, Gallimard, Paris, 1967, Ferdinand Gonseth writes on Einstein's methodological autonomy in his early work, and concludes: "I believe that the seeds of all that followed were contained within his assumption of the freedom to discard a proof in favor of a conviction born of the pursuit of truth." (p. 28)

Then, on the one hand we have his well-known personal philosophy of liberal agnosticism, even a withering contempt for established religious authority of any sort; and on the other hand there is also a clear personal religiosity. As he says in one of his letters, "I am a deeply religious unbeliever."

Elsewhere⁶ I have discussed yet another apparent conflict, that between Einstein as a scientific revolutionary and Einstein as a conservative who stressed the continuity of physics—as in his remark quoted by Carl Seelig: "With respect to the theory of relativity it is not at all a question of a revolutionary act, but of a natural development of a line which can be pursued through centuries."⁷

VI. THE FIELD AND THE QUANTUM

It is surely significant that these personal "odd contrasts" have their counterparts in polarities that run right through his scientific work. The most striking of these is the well-known dichotomy between Einstein's devotion to the thema of the *continuum*—expressed most eminently in the field concept—as the basis for fundamental, scientific explanation, and, on the other side, his role in developing quantum physics in which the key idea is *atomistic discreteness*. This merits some amplification.

His devotion to the continuum was not exceeded by that to any other thema, except possibly of symmetry and invariance (that is, of "relativity" itself). It held to the very end; a paper published the year before his death is on "Algebraic Properties of the Field in the Relativistic Theory of the Asymmetric Field." In his letters, even more strongly than in his articles, we find him incessantly defending the continuum against attacks on it by the quantum physicists. He once called the clas-

⁶"Science and New Styles of Thought," *The Graduate Journal*, VII, 2 (Spring 1967).

⁷See also one of Einstein's earliest letters (1905), written to his friend Conrad Habicht and describing the various investigations he was just then working on. In one sentence Einstein describes the nascent relativity theory: "The fourth work lies at hand in draft [*liegt im Konzept vor*] and is an electrodynamics of moving bodies making use of a *modification* of the theory of space and time; you will surely be interested in the purely kinematic part of this work." (Italics supplied.)

And to Maurice Solovine, Einstein writes a year later, soon after his first papers on relativity were published, and in the middle of its further development: "Nothing much here by way of science. Soon I shall enter the stationary and sterile stage of life in which one complains about the revolutionary orientation of youth."

sical concept of the field the greatest contribution to the scientific spirit, and it is significant that in the first paragraph of his fundamental 1905 paper on relativity theory, Einstein motivates the whole discussion by describing the old, seemingly trivial, experiment of a current induced in a conductor that moves with respect to a magnetic field.

The field played a crucial role in his imagination even earlier. From his autobiographical notes and other testimony we know that his successful formulation of universal principles on which to reconstruct physics in 1905 depended on his fully understanding at last the solution to a paradox on which he had reflected for ten years, since the day it had occurred to him as a sixteen-year-old student at the Kanton Schule of Aarau in 1895–96.⁸ We shall later find that it was probably not by accident that this key perception happened at Aarau.

He later described the paradox as it appeared to him in a vivid thought experiment: "If I pursue a beam of light with the velocity c (velocity of light in a vacuum), I should observe such a beam of light as a spatially oscillating electromagnetic field at rest. [For example, looking back along the beam over the space of one whole wavelength, one should see a field in which the local magnitudes of the electric and magnetic field vectors are fixed in time but are different point by point in space, with local values ranging from, say, zero to full strength and then again to zero, one wavelength away.] However there seems to be no such thing, whether on the basis of experience or according to Maxwell's equations." Only by postulating the principles of relativity was the surprising expectation shown to be in error, and the physics of the field rescued from this absurdity.⁹

But the field held Einstein enthralled even earlier, as shown in the

⁸As Einstein's biographer P. Frank pointed out, the paradox continued to preoccupy him ever more urgently throughout his whole period of study at the Polytechnicum in Zurich. "He studied all works of the great physicists to the purpose of finding whether they could contribute to the solution of this problem concerning the nature of light." (Translated from P. Frank, *Einstein*, Paul List Verlag, Munich, 1949, p. 40.)

⁹One should note that, as Banesh Hoffmann has pointed out, it is hard to see why the situation described in the thought experiment should be considered surprising or impossible if one applies a Galilean instead of a Lorentz transformation to Maxwell's equations. Somehow, Einstein appears already tacitly to have been thinking that Maxwell's equation must remain unchanged in form for the observer moving along with the beam—thereby adopting a principle of relativity *ab initio*.

youthful essay which J. Pelseneer discovered in Belgium not long ago.¹⁰ Its title is nothing less than "On the Examination of the State of the Ether In the Magnetic Field" (*Über die Untersuchung des Aetherzustandes im magnetischen Felde*). It is a suggestive piece, for it shows that Einstein had already encountered Hertz's work on the electromagnetic field, and that he was thinking up experiments to probe the state of the ether which, he said, "forms a magnetic field" around electric current. For this purpose he suggested sending a lightbeam into the magnetic field as a probe. Any effects on the measurable speed or wavelengths of such a beam would reveal the "elastic deformation" of the ether or field.

It would be an error to think of that essay in any way as a draft of ideas on which the later relativity theory was directly based, or even to regard it necessarily as his first scientific work. But what is perhaps most significant about it is the idea of the lightbeam as a probe of a field. From the contemplation of how to measure the wavelengths of such a beam, it would be only a small step to the recognition of the paradox Einstein hit upon soon afterwards at the Aarau School.

We can go back even further when searching for the point where the thematic commitment to the continuum was formed. It is well known that as a child of four or five, Einstein experienced what he called "a wonder" when his father showed him a simple magnetic pocket compass. It was an experience to which Einstein often referred. His friend Moszkowski reported him in 1922 to have said, "Young as I was, the remembrance of this occurrence never left me." His biographer Seelig wrote in 1954 that the compass "to this day is vividly engraved in his memory, because it practically bewitched him." Another (although less reliable) biographer reported that Einstein told him of that early part of his life: "The compass, and only the compass, remains in my memory to this day." In his *Autobiography*, written at the age of sixty-seven, we read: "I can still remember—or at least I believe I can remember—that this experience made a deep and lasting impression on me. Something deeply hidden had to be behind things."

¹⁰I am grateful to Professor Pelseneer for a copy of the 6-page-long essay. The covering letter which Einstein had sent with the essay to his uncle, Caesar Koch, received later the added note in Einstein's own hand, "1894 or 95. A. Einstein (date supplied 1950)." The work was probably composed while the 15- or 16-year-old boy was preparing for his first attempt to enter the Polytechnic Institute in Zurich.

The scene is most suggestive: There is the mysterious invariance or constancy of the compass needle, ever returning to the same direction, despite the fact that the needle seems free from any action-by-contact of the kind that is usually and unconsciously invoked to explain the behavior of material things; despite the vagaries of motion one may arbitrarily impose on the case of the compass from the outside; and regardless of personal will or external *Zwang* or chaos. If Einstein remembered it so well and referred to it so often, it may be because the episode is an allegory of the formation of the playground of his basic imagination.¹¹ For anyone interested in the genesis of scientific ideas or the motivation toward scientific study, these sketchy remarks will indicate that there are here problems that cry out to be worked on: These are, however, not our chief concerns here. What does matter is that this long loyalty to the explanatory power of the continuum was destined to be put to a severe test.

For Einstein, of course, was also the brilliant contributor to the physics based on the thema precisely the polar opposite to the continuum, namely the discrete quantum, for example in the conception that light energy is not continuously divisible, but proceeds in well defined quanta or photons. By his own report Einstein came to quantum physics by studying what Planck's radiation formula may imply for the "structure of radiation and more generally . . . the electromagnetic foundations of physics." Einstein's first fundamental paper in physics, entitled "On a Heuristic Point of View Concerning the Generation and Transformation of Light," was finished at Bern in 1905, three days after he had turned twenty-six and only about three months before the relativity paper.

The wonder is that it is so completely different (in all ways but one) from the relativity paper. That is, its object is close to what Einstein later called "mental gymnastics": to overcome a problem in physics at any cost, but without a basic reformulation. As its title says, it is dom-

¹¹The significance and validity of childhood experiences of this sort among scientists and inventors have not been sufficiently studied, although interesting material exists. One recalls here Newton's reported construction of toys, and Ernst Mach's account of the lasting impression made on him by a visit, as a small child, to a wind-operated grain mill. Orville Wright, when asked when and how he and his brother Wilbur first became interested in the problem of flight, responded: "Our first interest began when we were children. Father brought home to us a small toy actuated by a rubber spring which would lift itself into the air. We built a number of copies of this toy, which flew successfully."

inated by a heuristic attitude. On the other hand, the relativity paper is Natural Philosophy in the deepest sense: rejecting everything arbitrary, even assumptions concerning the nature of matter, in order to find the nature of space and time that allows causal continuity and prepares for the great simplifications and unifications, first of all, that of the transformation properties of mechanics and electrodynamics.

But while the relativity theory became Einstein's own most enduring life preoccupation, he could not accept quantum physics seriously. To Leopold Infeld he said "I may have started it, but I always regarded these ideas as temporary. I never thought that others would take them so much more seriously than I did." Yet, he continued to make some of the most seminal contributions to it for a quarter of a century from 1905 on, with rarely a year going by in which he did not publish an article on this subject.

These, then, are some of the characteristics that seem to be polar opposites. It is, I think, significant that in the essay "Motiv des Forschen," which we discussed at the outset, Einstein himself drew attention to the existence of such polar pairs in the work and personality of outstanding scientists—precisely in the one essay in which he came closest to asking the question raised here: wherein lies scientific genius? It was not an accident that in the essay Einstein contrasted the "positive" and "negative" motives for doing research at that highest level, and the opposing demands of clarity and completeness, of logic and intuition, of private and public science.

If we now take it as granted that striking polarities exist, of both personality and of work, in this one case,¹² we must ask next: Are they important or accidental? Earlier, we disavowed the idea that a plausible list of individual characteristics adds up to an explanation or even a

¹²Shortly after a draft of this paper had been prepared, I came upon a congenial passage on the same subject, pointing to the existence of "certain tensions," in the Foreword by Russell McCormack, Editor of *Historical Studies in the Physical Sciences*, University of Pennsylvania Press, Philadelphia, 1970, vol. II, pp. x-xi. In recent correspondence, Professor McCormack kindly has drawn my attention also to C. P. Snow's profile of Einstein in *A Variety of Men*, Charles Scribner's Sons, New York, 1967, in which the existence of some paradoxes is noted. Needless to say, literary figures have also been subject to such analysis; one of the less familiar examples may be V. I. Lenin's discussion of the "glaring contradictions in Tolstoy's works, views, doctrines," in "Leo Tolstoy as the Mirror of the Russian Revolution" (1908).

characterization of genial scientists. If such singularities were not significant, why should polarities be? What special abilities do they convey to their possessor? Does a man of genius bring to bear upon his work the harmonies and disharmonies, the strengths and conflicts within his person—and the pressures and conflicts of his environment? Regardless of how a man like Einstein came to have his particular characteristics (that may in any case not be a very interesting question in the present state of such research), we can ask whether there was some special way in which he *put to use* these dichotomies and conflicting polarities.

We are encouraged to expect a positive reply to such questions if we notice the existence and brilliant exploitation of polarities in Einstein's *scientific contributions*. The most evident example is the presence and use of contrast in the original relativity theory paper itself: we find there both the positivism of the instrumentalist and operationist variety, which Einstein uses in defining the concepts, and, on the other hand, the rational realism inherent in the *a priori* declaration of the two basic principles of relativity (moreover, two apparently contradictory ones, introduced seemingly in arrogant disregard both of current scientific sensibilities and of the contemporary demand to base them plausibly on scientific experiments).¹³ Einstein himself acknowledged this ambivalence. In response to the charge¹⁴ "Einstein's position . . . contains features of rationalism and extreme empiricism . . . , " he replied: "This remark is entirely correct. . . . A wavering between these extremes appears to me unavoidable."

A similarly creative use of apparent opposites can be found in Einstein's contribution to quantum physics, centering on the wave-particle duality. It really is the hallmark of Einstein's most famous contributions that he could deal with, use, illuminate, transform the existence of apparent contradictions or opposites, sometimes in concepts that had not yet been widely perceived to have polar character. One need only think of his bridging of mechanics and electrodynamics, energy and mass, space coordinates and time coordinates, inertial mass and gravitational mass.

¹³For elaboration, see Gerald Holton, "Mach, Einstein, and the Search for Reality" in R. S. Cohen and R. J. Seeger, eds., *Ernst Mach, Physicist and Philosopher*, D. Reidel, Dordrecht, Holland, 1970, pp. 165-199.

¹⁴P. A. Schilpp, ed., *Albert Einstein: Philosopher-Scientist*, Evanston, Illinois, 1949, pp. 679-680.

VII. THE SIGNIFICANCE OF ASYMMETRY

We can now choose for detailed study a concrete example by means of which to find further hints on how personal characteristics interacted with scientific work. The example offers itself in fact at the very beginning of the basic 1905 paper on relativity theory. The title is "On the Electrodynamics of Moving Bodies," and neither there nor later on is the phrase "relativity theory" used. None of Einstein's papers has this phrase in the title until 1911, long after others began to refer to his work in that way. Indeed, it is of the essence to know that for the first two years Einstein, in his letters, preferred to call his theory not "relativity theory," but exactly the opposite: "*Invarianteentheorie*." It is unfortunate that this splendid, accurate term did not come into current usage, for it might well have prevented the abuse of relativity theory in many fields.

In the very first sentence of the paper, there is a term that attracts our attention, especially now that we have become sensitized to it by the previous discussion on polarities. The sentence is an *Eroica*-like opening: "It is known that Maxwell's electrodynamics—as usually understood at the present time—when applied to moving bodies, leads to asymmetries which do not appear to be inherent in the phenomena." It is not Maxwell's electrodynamics that is at fault; it is the way it is usually understood: a bold, not to say aggressive, statement from this relatively unknown young patent-office employee. And this usual way of understanding has led to—what? An experimental puzzle? No. A theoretical impasse? No. It leads to "asymmetries that do not appear to be inherent in the phenomena."

For example, you may think of inducing a current in a conductor—as Einstein immediately does in the style of a little thought-experiment. To calculate what current to expect when the conductor is moving with respect to a stationary magnet, you must use one kind of equation. When you calculate again on the assumption that now you will keep the conductor stationary and let the magnet move, you must use a different kind of equation—although the current actually produced is found to be identical in both cases, as has been known ever since Faraday first described the effect in 1831. The phenomenon is characterized by symmetry; but the machinery for calculation was characterized by polarity

or asymmetry (until Einstein showed, later in the paper, how to "relativize" the problem so that the same equation may be used for both cases).

The importance of this passage, its historic veracity, and the fact that a very similar thought process led Einstein later to the *General Theory of Relativity* is brought out in a striking manner in parts of a hitherto unpublished manuscript in Einstein's handwriting, dating from about 1919 or shortly afterwards, now located in the Einstein Archives at the Princeton Institute for Advanced Study, and entitled (in translation from the German) "Fundamental Ideas and Methods of Relativity Theory, Presented in their Development" [emphasis in original].

In the first nineteen pages of the manuscript, one finds largely an impersonal pedagogic presentation of a familiar kind, e.g. similar to Einstein's book written in 1916 and published in 1917—except for the passage on page 15 in which, under the heading "Special Relativity Theory on Aether," he introduces an evidently personal note:

"It is clear that in relativity theory there is no place for the conception of a resting aether. For if systems *K* and *K'* are equally valid for the formulation of natural laws it is a *non sequitur* to put at the base of the theory a conception which singles out *one* of these systems above all the rest. For if one postulates an ether that is at rest relative to *K*, then the ether is moving relative to *K'*, which does not fit with the physical equivalent of both systems."

"For this reason I was, in 1905, of the opinion (*Ansicht*) that one may not any longer speak of an ether in physics. . . ."

The next personal account appears, in a rather surprising way, on pages 20 to 21 which introduce Part II, entitled "General Relativity Theory."

"(15) *The fundamental idea of general relativity theory in its original form.* In the construction of special relativity theory, the following, [in the earlier part of this manuscript] not-yet-mentioned thought concerning the Faraday [experiment] on electromagnetic induction played for me a leading role.

"According to Faraday, during the relative motion of a magnet with respect to a conducting circuit, an electric current is induced in the latter. It is all the same whether the magnet is moved or the conductor; only the relative motion counts, according to the Maxwell-Lorentz the-

ory. However, the theoretical interpretation of the phenomenon in these two cases is quite different:

"If it is the magnet that moves, there exists in space a magnetic field that changes with time and which, according to Maxwell, generates closed lines of electric force—that is, a physically real electric field; this electric field sets into motion movable electric masses [e.g. electrons] inside the conductor.

"However, if the magnet is at rest and the conducting circuit moves, no electric field is generated; the current in the conductor arises because the electric bodies moving with the conductor experience an electromotive force, as introduced hypothetically by Lorentz, on account of their (mechanically enforced) motion relative to the magnetic field.

"The thought that one is dealing here with two fundamentally different cases was for me unbearable [war mir unerträglich]. The difference between these two cases could not be a real difference, but rather, in my conviction, only a difference in the choice of the reference point. Judged from the magnet there were certainly no electric fields [whereas] judged from the conducting circuit there certainly was one. The existence of an electric field was therefore a relative one, depending on the state of motion of the coordinate system being used, and a kind of objective reality could be granted only to the *electric and magnetic field together*, quite apart from the state of relative motion of the observer or the coordinate system. The phenomenon of the electromagnetic induction forced me to postulate the (special) relativity principle. [Footnote:] The difficulty that had to be overcome [then] was in the constancy of the velocity of light in vacuum which I had first thought I would have to give up. Only after groping for years did I notice that the difficulty rests on the arbitrariness of the kinematical fundamental concepts [presumably such concepts as simultaneity].

"When, in the year 1907, I was working on a summary essay concerning the special theory of relativity for the *Jahrbuch für Radioaktivität und Elektronik*, I had to try to modify Newton's theory of gravitation in such a way that it would fit into the theory [of relativity]. Attempts in this direction showed the possibility of carrying out this enterprise, but they did not satisfy me because they had to be supported by hypotheses without physical basis. At that point there came to me the happiest thought of my life, in the following form:

"Just as is the case with the electric field produced by electromag-

netic induction, the gravitational field has similarly only a relative existence. For if one considers an observer in free fall, e.g. from the roof of a house, there exists for him during his fall no gravitational field—at least in his immediate vicinity. For if the observer releases any objects they will remain relative to him in a state of rest, or in a state of uniform motion, independent of their particular chemical and physical nature. (In this consideration one must naturally neglect air resistance.) The observer therefore is justified to consider his state as one of "rest."

"The extraordinarily curious, empirical law that all bodies in the same gravitational field fall with the same acceleration received through this consideration at once a deep physical meaning. For if there is even a single thing which falls differently in a gravitational field than do the others, the observer would discern by means of it that he is in a gravitational field, and that he is falling into it. But if such a thing does not exist—as experience has confirmed with great precision—the observer lacks any objective ground to consider himself as falling in a gravitational field. Rather, he has the right to consider his state as that of rest, and his surroundings (with respect to gravitation) as fieldfree.

"The fact of experience concerning the independence of acceleration in free fall with respect to the material is therefore a mighty argument that the postulate of relativity is to be extended to coordinate systems that move non-uniformly relative to one another. . . ."

To return now to the clue that is offered by the use of the word "asymmetry" in Einstein's 1905 paper: At first glance, it surely is curious that Einstein used the term "asymmetry" for this apparent redundancy or lack of universality. Moreover, such terms as symmetry or asymmetry still referred at that time largely to aesthetic judgments, and the like are often (but wrongly) still considered to be the polar opposites of scientific judgments. In the physics literature of that period, symmetry arguments were quite uncommon, although they have become easier to find now, in retrospect;¹⁵ the terms "symmetry" or "asymmetry" were

¹⁵In addition, it is not unusual now erroneously to discern symmetry arguments where none existed in the literature. Thus, Alfred M. Bork (in "Maxwell, Displacement Current and Symmetry," *American Journal of Physics*, 31, 1963, 854–859) cites examples of twentieth-century textbooks that claim J. C. Maxwell used symmetry arguments in proposing Maxwell's equations, with the displacement current term included. However, Bork shows that "there is no direct evidence to support the notion that Maxwell introduced the displacement-current term in order to improve the symmetry of the electromagnetic field equations" (p. 859).

rarely used in physics except in such branches as crystal physics. For example, the word "symmetry" is mentioned only casually in Mach's *Science of Mechanics* (although the concept is used implicitly in discussions involving the Principle of Sufficient Reason, in a form traced by Mach to Schopenhauer). As late as 1929, the eleventh edition of the large encyclopedia of physics of Müller-Pouillet indexes only a single noncasual use of the symmetry concept outside crystal physics—and that is, naturally, the entry for symmetrical tensors, and refers to Einstein's General Relativity Theory itself. There is no entry for symmetry or asymmetry in the *Sachregister* of the encyclopedic volume *Physik* (E. Warburg, ed., B. G. Teubner, Leipzig, 1915). Nor, for that matter, is there any such entry in the eleventh edition of the *Encyclopedia Britannica* (1910).

Only with the growth of the role of quantum mechanics, and more recently of elementary particle physics, has it become clear that the conservation laws of physics are closely connected with the concept of symmetry of space and time, as had been implicit in the Lagrangian and Hamiltonian methods of solving physical problems. And it is only with the help of hindsight that we have come to see, as H. Weyl¹⁶ points out, that "the entire theory of relativity . . . is but another aspect of symmetry," in the sense that "the symmetry, relativity, or homogeneity of this four-dimensional medium [the space-time-continuum] was first correctly described by Einstein. . . . It is the inherent symmetry of the four-dimensional continuum of space and time that relativity deals with."

But all this was far in the future in 1905. In any case, Einstein's reference to asymmetry at the start of his paper was not a symmetry consideration of the same kind as those just mentioned. Few physicists, if any, can have thought in 1905, that there was something of fundamental importance in the asymmetry to which Einstein pointed. And if one considers how many troubles there were in electrodynamics at the time, it must have seemed peculiar indeed to seek out this quasi-aesthetic discomfort, and to put it at the head. Yet, Einstein's perception of asymmetry at this point *does* reveal to us his remarkable and original sensitivity to polarities and symmetry properties of nature that later became recognized as important in relativity theory and in contemporary physics generally.

¹⁶H. Weyl, *Symmetry*, Princeton University Press, 1952, pp. 17, 130, 132.

From everything we now know about Einstein, we have also been prepared to understand that his desire to remove an unnecessary asymmetry was not frivolous or accidental, but deep and important. At stake is nothing less than finding the most economical, simple, formal principles, the bare bones of nature's frame, cleansed of everything that is *ad hoc*, redundant, unnecessary.¹⁷ To his assistant, Ernst Straus, Einstein said later: "What really interests me is whether God had any choice in the creation of the world." In fact, sensitivity to previously unperceived formal asymmetries or incongruities of a predominantly aesthetic nature (rather than, for example, a puzzle posed by unexplained experimental facts)—that is the way each of Einstein's three otherwise very different great papers of 1905 begin. In all these cases the asymmetries are removed by showing them to be unnecessary, the result of too specialized a point of view. Complexities which do not appear to be inherent in the phenomena should be cast out. Nature does not need them.

And Einstein does not need them. In his own personal life, the legendary simplicity of the man is an integral part of this reaching for the bare minimum on which the world rests. I will not need to recall here the many stories about this simplicity. Even people who knew nothing else about Einstein knew that he preferred the simplest possible clothing. We noted that he hated nothing more than artificial restraints of any kind. He once was asked why he persisted in using the ordinary hand-soap for shaving, instead of shaving cream, despite the fact that it was clearly less comfortable for him to shave that way. He answered, in effect: "Two soaps? That is too complicated!"

Two equations? That is too complicated. Nature does not work this way, and Einstein does not work this way. The overlap between the two was once expressed by Einstein in a humble sentence: "I am a little piece of nature." We have here an important clue to our question what may be meant by the easy term "genius": *there is a mutual mapping of the mind and life style of this scientist, and of the laws of nature.*

At the same time, the desire to remove an unwarranted "asymmetry" contains a clue to a second connection between Einstein's work and his person. The area that seems worth exploring lies where two studies

¹⁷For a detailed discussion of Einstein's rejection of the *ad hoc* elements in physics, see G. Holton, "Einstein, Michelson, and the 'Crucial' Experiment," *Isis*, 60.2 (Summer 1969), 132-197.

meet: one is the well-known use of symmetry and asymmetry arguments in mathematics, particularly geometry, and the other is the investigation of mathematical and other thought processes in children by W. Köhler, M. Wertheimer, and others of that school.

VII. "WHAT, PRECISELY, IS 'THINKING'?"

We know far too little about Einstein as a child, but he is commonly reported to have been withdrawn, slow to respond, quietly sitting by himself at an early age, playing by putting together shapes cut out with a jigsaw, erecting complicated constructions by means of a chest of toy building parts. Before he was ten, he was making, with infinite patience, fantastic card houses that had as many as fourteen floors. He is said to have been unable or unwilling to talk until the age of about three. In an (unpublished) biography of Albert Einstein, his sister Maja wrote (in 1924): "His general development during his childhood years proceeded slowly, and spoken language came with such difficulty that those around him were afraid he would never learn to talk."¹⁸ Many pediatricians and psychologists today might still consider such evidence to indicate an almost backward child.

But an apparent defect in a particular person may merely indicate an imbalance of our normal expectations. A noted deficiency should alert us to look for a proficiency of a different kind in the exceptional person. The late use of language in childhood, the difficulty in learning foreign languages—one remembers that Einstein failed in foreign languages at the *Gymnasium* and again at the entrance examination in Zürich (one of the reasons for his having to go to the Kanton Schule in Aarau), that his vocabulary in English was fairly small—all this may indicate a polarization or displacement in some of the skill from the verbal to another area. That other, enhanced area is without doubt, in Einstein's case, an extraordinary kind of visual imagery that penetrates his very thought processes.

Although it seems to have been hardly noted so far, Einstein him-

¹⁸This fear soon proved unfounded, but she reports further: "The early thoroughness in thinking found a characteristic but curious expression. Every spoken sentence, no matter how trivial, he repeated once more silently with lip-motion. This remarkable habit left him only in his seventh year."

self plainly signals this point early in his *Autobiographical Notes*. He asks, rather abruptly:

"What, precisely, is 'thinking'? When at the reception of sense-impressions, memory-pictures [*Erinnerungsbilder*] emerge, this is not yet 'thinking.' And when such pictures form series, each member of which calls forth another, this too is not yet 'thinking.' When, however, a certain picture [*Bild*] turns up in many such series, then—precisely through such return—it becomes an ordering element for such series, in that it connects series which in themselves are unconnected. Such an element becomes an instrument, a concept . . . [One notes that a property akin to invariance is required even to produce a concept.] It is by no means necessary that a concept must be connected with a sensorially cognizable and reproducible sign (word). . . . All our thinking is of this nature of a free play [*eines freien Spiels*] with concepts. . . . For me it is not dubious that our thinking goes on for the most part without use of signs (words), and beyond that to a considerable degree unconsciously."

It is not accidental at all that this surprising passage comes just before Einstein tells of the two "wonders" experienced in childhood. One of these was the experience with the compass that we have mentioned. The other wonder, of a totally different nature, was the little book dealing with Euclidean plane geometry which, he recalled, was given to him at about the age of twelve. In this connection, Einstein described an early, successful use of his particular way of "thinking" in visual terms:

"I remember that an uncle told me the Pythagorean theorem before the holy geometry booklet had come into my hands. After much effort I succeeded in 'proving' this theorem on the basis of the similarity of triangles; in doing so it seemed to me 'evident' that the relations of the sides of the right-angled triangle would have to be completely determined by one of the acute angles. Only something which did not in similar fashion seem to me 'evident' appeared to me to be in need of any proof at all. Also, the objects with which geometry deals seemed to be of no different type than the objects of sensory perception 'which can be seen and touched'." (Italics supplied.)

The objects of the imagination were to him evidently persuasively real, visual materials which he voluntarily and playfully could reproduce and combine, analogous perhaps to the play with shapes in a jigsaw puzzle. The key words are *Bild* and *Spiel*; and once alerted to them, one finds them with surprising frequency in Einstein's writings. Thus, responding to Jacques Hadamard, Einstein elaborates the point made above:¹⁹

"The words or the language, as they are written or spoken, do not seem to play any role in my mechanism of thought. The psychical entities which seem to serve as elements in thought are certain signs and more or less clear images which can be 'voluntarily' reproduced and combined. . . . But taken from a psychological viewpoint, this combinatory play seems to be the essential feature in productive thought—before there is any connection with logical construction in words or other kinds of signs which can be communicated to others. The above-mentioned elements are, in my case, of visual and some of muscular type.²⁰ Conventional words or other signs have to be sought for laboriously only in a secondary stage, when the mentioned associative play is sufficiently established and can be reproduced at will."

Max Wertheimer, one of the founders of Gestalt psychology and a friend of Einstein, reports²¹ that from 1916 on, in numerous discussions, he had questioned Einstein "in great detail about the concrete events in his thoughts" leading to the theory of relativity. Einstein told him: "These thoughts did not come in any verbal formulation. I very rarely think in words at all. A thought comes, and I may try to express it in words afterwards." And later: "During all those years there was a feeling of direction, of going straight toward something concrete. It is, of course, very hard to express that feeling in words. . . . But I have it in a kind of survey, in a way visually. One is reminded of Wittgenstein's sentence:

¹⁹Published in Hadamard's book, *The Psychology of Invention in the Mathematical Field*, Princeton, 1945, pp. 142-143.

²⁰Einstein might have added "auditory" to visual and muscular. See his remark to R. S. Shankland (*Am. J. Physics*, 31 (1963), p. 50): "When I read, I hear the words. Writing is difficult, and I communicate this way very badly."

²¹Max Wertheimer, in the chapter "The Thinking that Led to the Theory of Relativity," in *Productive Thinking*, New York, 1945, p. 184.

"Was gezeigt werden kann, kann nicht gesagt werden." (*Tractatus*, 4, 1212.)

In Einstein's published work, his visual imagination sometimes breaks through vividly. One thinks here, for example, of passages where he describes thought experiments involving the picturesque tasks of co-ordinating watch readings, the arrival of light signals, the positions of locomotives and those of lightning bolts. Possibly Einstein's ability to deal with models and drawings in the patent office, and his own delight with the workings of puzzle-toys, are additional clues of some significance. More important for our purposes, this ability of visualization is evident in the haunting thought experiment of the lightbeam, begun at Aarau, and even in the thought experiment proposed in his earlier essay of 1894-5, where he envisages probing the state of the ether in the vicinity of a current-carrying wire.

I have little doubt that the ability to make such clear visualizations of experimental situations was crucial in his task of penetrating to the relativity theory (for example in the argument leading to the relativity of simultaneity). It is so to this day. As anyone who has tried to instruct students in relativity theory knows, the problem of getting a firm initial understanding of special relativity is not one of mathematics—at most, elementary calculus is required—but rather one of clear *Vorstellung*.²² What helps a beginner most is precisely the ability to imagine vividly some thought experiments involving the perceptions and reports of two observers who are moving relative to each other. The style of thinking necessary at the outset is quite different from working, for example,

²²This point has been additionally documented, most recently in the record of remarks made by Einstein in 1938:

"I grasp things as quickly as I did when I was younger. My power, my particular ability, lies in *visualizing the effects, consequences, and possibilities*, and the bearings on present thought of the discoveries of others. I grasp things in a broad way easily. I cannot do mathematical calculations easily. I do them not willingly and not readily." (Italics added.) A related point is made in the essay, "Recollections of Lord Rutherford" by P. L. Kapitza, who uses the cases of Benjamin Franklin and Faraday—and to some degree of Rutherford himself—"to show that at a particular stage of the development of science, when new fundamental concepts have to be found, wide erudition and conventional training are not the most important characteristics of a scientist required to solve this kind of problem. It appears that in this case imagination, very concrete thinking and, most of all, daring are needed. Strict logical thinking which is so necessary in mathematics hinders the imagination of a scientist when new fundamental concepts must be found." (Proceedings of the Royal Society, A 294 1966, 125).

through the machinery of a formalism such as characterizes the work of a Sommerfeld.

But in the long run, the strength of Einstein's visual imagination was not limited only to such uses. Rather, the hypothesis here proposed is that the special ability to think with the aid of a play with visual forms has deeper consequences. It animates the consideration of symmetries and a corresponding distaste for extraneous complexities from the beginning to the end—from the result embedded in his 1905 paper that the Lorentz transformation equations yield "contractions" and "time dilations" that are, for the first time symmetrical for all inertial systems, to his long wrestling with the task of constructing a relativistic theory of gravitation, basing its field structure on a symmetry tensor g and the symmetric infinitesimal displacement tensor Γ , and finally where he finds that he is forced to it, labors of finding a theory of the total field through the generalization of giving up the symmetry properties of the g and Γ fields. From the beginning to the end, Einstein's scientific thought was pervaded by questions of symmetry and the closely related concept of invariance.

But long before he wrote scientific papers, Einstein was Einstein—already at the age of three, playing silently, resisting verbal language, and refusing thereby to accept an externally imposed authority in names and rules by which many another child has had to "civilize" and give up his own curiosity and imaginative play. It is a world that by its very definition we hardly know how to describe. But it is the world in which, from all the evidence we have, the play with geometric and other visual images, and hence the perception of such transformation properties of forms as symmetry and asymmetry, appear to have been basic for the development of successful thought itself.

IX. THE ABC OF VISUAL UNDERSTANDING

The literature on the subject of the visual element of thought in children is small. This deprives us to some degree of one of the important strands in our net, and conversely alerts cognitive psychologists and educators to a promising research topic. But there was one pioneer in this field who, in an unexpected way, now comes into our story. It is Johann Heinrich Pestalozzi, the Swiss educational reformer. Born in

1746 in Zurich, he was educated at the University of Zurich, and first tried farming in a small town in the Kanton of Aargau. During the French invasion of Switzerland in 1798, a number of children were left without parents or food at Lake Lucerne, and Pestalozzi devoted himself to their care and education during that winter. In 1801 he published his ideas on education in the book *Wie Gertrud ihre Kinder lehrt*, and from then on he became a widely influential force in education. It is recorded that von Humboldt, Fichte, Mme. de Staël and Talleyrand visited him and his schools.

Basic to Pestalozzi's approach to education were the development of observation, the humanistic approach to each subject, the collaborative and sympathetic relation between teacher and student—and above all his view that "conceptual thinking is built on visual understanding (*Anschauung*)."²³ His method was, for that reason, to put the ABC of visual understanding ahead of the ABC of letters.

An excerpt from his book will give some indication of the approach followed in his own schools and in those that were founded under his influence:

"I must point out that the ABC of visual understanding is the essential and the only true means of teaching how to judge the shape of all things correctly. Even so, this principle is totally neglected up to now, to the extent of being unknown; whereas hundreds of such means are available for the teaching of numbers and language. This lack of instructional means for the study of visual form should not be viewed as a mere gap in the education of human knowledge. It is a gap in the very foundation of all knowledge at a point to which the learning of numbers and language must be definitely subordinated. My ABC of visual understanding is designed to remedy this fundamental deficiency of instruction; it will insure the basis on which the other means of instruction must be founded."²⁴

The "quintessence" of Pestalozzi's fundamental book *Wie Gertrud ihre Kinder lehrt* was summarized by Albert Richter in these words:²⁴

²³Translation of quotation as given in Rudolph Arnheim, *Visual Thinking*, Univ. of California Press, 1969, p. 299. Arnheim's book itself is very suggestive and should be consulted.

²⁴In the appendix to the fourth edition, Leipzig, 1880, of Pestalozzi's book, pp. 184–185; Richter based his work on Morf's *Biographie Pestalozzis*.

1. The foundation of instruction is the *Anschaung* (visual understanding).
2. Speech (words) must be connected with the *Anschaung*.
3. The time of learning is not the time of judgment, of critique.
4. In every subject, instruction shall begin with the simplest elements, and from that point on be brought forward stepwise in accord with the development of the child—that is to say, in psychological order (sequences).
5. One must rest at every point as long as is necessary for the particular material of instruction to become the pupil's free mental property.
6. Instruction has to follow the way of development, not the way of expostulation, memorization, information.
7. For the instructor, the individuality of his charge should be holy.
8. The main purpose of elementary instruction is not the acquisition of information and skills, but the development and strengthening of mental powers.
9. Ability should follow knowledge; then follows skill.
10. The interaction between instructor and pupil, and particular school discipline, should be pervaded and guided by love.
11. Instruction should be subordinated to the purpose of education.
12. The foundation of moral-religious development of the child lies in the relationship between mother and child.²⁵

One notes the modern and humane ring of many of these guidelines. Still, Pestalozzi himself is quoted as having made this final assessment: "When I look back and ask myself what I have really achieved for the cause of human instruction, I find this: I have firmly anchored the first and fundamental axiom of instruction in the recognition of *Anschaung* as the absolute foundation of all knowledge."

²⁵This attitude may well account in good part for the fact, as Richter reports (*ibid.*, p. 184), that major opposition arose against Pestalozzi as a consequence of his remarks concerning religious education, and that in particular this work was considered an "estrangement from Christ."

X. SUCCESS IN AARAU

How different a school run on Pestalozzi's principles must have been from the school which Einstein fled when he left Munich as a boy of about fifteen—his regimented, militaristic *Gymnasium*, so verbally oriented, and so deeply unappreciative of him as a student and as a person!

In the late summer of 1895, when Einstein arrived in Zurich to sit for the entrance examination to the Polytechnic Institute, he must have thought he had put all schools behind him. But then he failed the entrance examination, as we have noted. He had given up his native country and taken steps to renounce his citizenship; he had left his parents in Italy; he was a foreigner in Switzerland; he had failed to get into the Polytechnic Institute. The alienation and dislocation was complete.

There ensued a kind of moratorium for this boy who seemed to have failed in many ways (except, always, in mathematics and physics). The next test being scheduled a year later, he was advised by the director of the Polytechnic Institute to enroll in the Kanton Schule at Aarau, thirty miles northwest of Zurich, in the Capital of the Kanton of Aargau. There is no doubt that this was a crucial turning point—for while he was at that particular school, everything somehow changed for him, from getting his first great *Gedankenexperiment* that led him to relativity, to finding true friends (one of whom—the son of the teacher, Jost Winteler, in whose house Einstein was a happy boarder—eventually married Einstein's sister, Maja). Later, Einstein's thoughts often and gladly turned to this school. He corresponded, and occasionally met, with several of his classmates for years afterwards. He mentions the school, and his obtaining his *Abitur* there, in his autobiographical note of 1922 or 1923 which he had to compose, as is the custom, on receiving the Nobel Prize, for official publication in *Les Prix Nobel*—even though, characteristically, Einstein's essay is otherwise embarrassingly short—only fourteen lines. Just a month before his death he remembered the school again in these words: "It made an unforgettable impression on me, thanks to its liberal spirit and the simple earnestness of the teachers who based themselves on no external authority."²⁶

²⁶Cf. Carl Seelig, ed., *Helle Zeit, dunkle Zeit*, Europa Verlag, Zürich, 1956, p. 9.

Some of his teachers, particularly the science instructor Fritz Mühlberg, seem to have interested and encouraged Einstein. There also appears to have been an easy atmosphere, relaxed, informal, and democratic. In science instruction, too, the aims of general education and humanistic learning were kept foremost. The school prided itself not on memorization, but stressed instead the kind of approach that would develop individual thinking. In addition to the more usual learning materials there were excursions, work with specimens in the museum, and laboratory work. There were two kinds of drawing courses, both engineering and free-hand styles. Maps and other visual materials seem to have been freely used.

These and other elements in the suggestive though fragmentary descriptions of the school by Einstein's most reliable biographers—Frank, Reiser, and Seelig²⁷—show that this school seems to have been characterized by many of the same fundamental pedagogic guidelines that had earlier been laid down by Pestalozzi. And this prepares us for the discovery that indeed the Kanton Schule of Aarau was first founded in 1802 by democratic patriots,²⁸ reportedly acting in the spirit of Pestalozzi—just a year after the publication of Pestalozzi's manifesto, and while Pestalozzi himself was running one of his schools at Burgdorf, less than 50 miles away. So it would not be an accident that Einstein may have discovered the strength of his genial scientific imagination at this particular school. Here, at last, he was in a place that did not squash, and may well have fostered, the particular style of thinking that was so congenial to him.

XI. DEALING WITH ANTITHESSES

We began by noting some odd contrast and polarities that appear in the work and life-style of Einstein. We have added others: early failure and success, handicap in one direction and extraordinary ability in another. In discussing the role of simplicity and symmetry, we found one clue to genius in the proposition that there is, at least in this case, a

²⁷E.g., Carl Seelig, *Albert Einstein*, Europa Verlag, Zürich, 1954, pp. 16 and 21–24. There also was a muted anticlerical tradition in the school at Aarau, as documented by some of Einstein's classmates.

²⁸Noted, for example, by F. Herneck, *Albert Einstein*, Buchverlag der Morgen, Berlin, 1967, p. 49.

mutual mapping of the habits and life style of the genial scientist and of nature's own laws. The investigation of the circumstances surrounding the *Gedankenexperiment* at Aarau, from which the relativity theory grew, supports a more generalized phrasing of our initial hypothesis: At least in this particular case, *there is a mutual mapping of the style of thinking and acting of the genial scientist on the one hand, and of the chief unresolved problems of contemporary science on the other*. Thus, what seemed to us at first to be puzzling internal polarities in Einstein may equally well be viewed as talents for dealing with the dichotomies that often have turned out to be at the base of the most unyielding problems of science. For example, we have noted earlier that epistemologically the 1905 relativity paper oscillates between the positivism of the Machist kind, needed for the definition of the concepts, and on the other hand, a rational realism needed for the *a priori* declaration of the basic principles of relativity, and that Einstein confessed later to harboring both of these extremes in his own thoughts. But to this day, it is virtually inconceivable that he, or anyone else, could have attained a genuine formulation of relativity without just these two contrasting elements.

Much good and even excellent science can be done in a more monolithic way, neglecting or avoiding any evidence of conceptual dichotomies. But it is not often stressed that such dichotomies are by no means unusual in science. They exist from its smallest observational protocol to the most over-arching theory. Ascending from the lowest level, we note first the antithesis between the obvious, observable, palpable, limited, material object such as a magnet needle, and, say, the field in which it is caught—tenuous, invisible, appearing usually rather mysterious to the beginning student, but commanding, and stretching into infinity. A further step up, and we encounter conceptual pairs with various types of oppositions, such as matter and energy, space and time, the gravitational and electromagnetic fields—even theoretical and experimental activity, and the parsimony of a good theory versus the infinite number of actual cases it embraces.

Another step up, and we note the antitheses between the great themata—be it the continuum versus the discrete, or classically causal law versus statistical law, or the mechanistic versus the theistic world interpretation. Such thematic antitheses, expressed in the great current puzzles of science, have haunted such scientists as Newton, Bohr, and Einstein,

even when lesser scientists could afford the luxury of avoiding these confrontations, and doing more comfortable work on thematically unambiguous problems—an activity similar to that which Einstein once dismissed as seeking the thinnest part of a board in order to drill one's hole there. After all, genius discovers itself not in splendid solutions to little problems, but in the struggle with essentially eternal problems. And those, by their very nature, are apt to be problems arising from thematic conflicts.

It has been lately fashionable in some quarters to think that physical science normally progresses by moving on the whole fairly calmly in one direction and in one stream bed, and that such progress is interrupted only at certain periods of great upheaval in science. But this can be true only in a limited sense. Not far below the surface, there have co-existed in science, in almost every period since Thales and Pythagoras, sets of two or more antithetical systems or attitudes, for example one reductionist and the other holistic, or one mechanistic and the other vitalistic, or one positivistic and the other teleological. In addition, there has always existed a methodological set of antitheses or polarities, even though, to be sure, one or the other was at a given time more prominent—namely between the Galilean (or, more properly, Archimedean) attempt at precision and measurement that purged public, "objective" science of those qualitative elements that interfere with reaching reasonable "objective" agreement among fellow investigators, and, on the other hand, the intuitions, glimpses, daydreams, and *a priori* commitments that make up half the world of science in the form of a personal, private, "subjective" activity.

Science has always been propelled and buffeted by such contrary or antithetical forces. Like vessels with draught deep enough to catch more than merely the surface current, scientists of genius are those who are doomed, or privileged, to experience these deeper complex currents. It is precisely their special sensitivity to odd contrasts that has made it possible for these scientists to do so, and it is an inner necessity that has made them demand nothing less from themselves.

This, it seems to me, is the direction in which to look for answers to the question of why the "Angel of the Lord" chose them . . . or, at least, chose this one among them.

XII. A PRINCIPLE OF RELATIVITY FOR HISTORICAL STUDIES

As I warned earlier, we are left at this point with a number of suggestive questions and problems along the several dimensions characteristic of the "total" approach I have advocated as appropriate to modern studies.

Such a "total" or multidisciplinary approach, if sufficiently interesting and visibly successful, may help to improve the place which the history of science takes within the general field of historic studies. We know the pronouncements of great synthesizers such as Lucien Febvre's, in *Combats pour l'histoire* (Paris, 1953): "Il n'y a pas d'histoire économique et sociale. Il y a l'histoire tout court, dans son Unité." Nevertheless, the realities are different. It is a just and recurrent complaint that historians by and large do not take the history of science very seriously. It is no longer quite as bad as it was some twenty years ago, when the most authoritative encyclopedia of history in the U.S. listed Newton only once, and then only to indicate that he was made Warden of the Mint in 1696. But on the whole, one must agree with the recurrent complaint: despite the universal lip-service paid by historians to the special role of science in the development of Western culture during the past four centuries, history of science is for them foreign territory.

Of course there are differences between styles and detailed research programs in various types of historical study. But my claim is that the history of science, when properly defined, can and should assert a claim to central importance to all historical studies, particularly those concerned with the 20th century.

Let me merely mention a few obvious cases to show that the traditional wisdom of, say, political historians can benefit from the findings of the history of science when carried out in the wide-ranging way I have advocated. Thus I find that the correspondence in the Einstein Archives at Princeton Institute can be considered as rich a resource in documenting in its own way the rise of Nazism as are the more conventionally used State Archives or speeches of political leaders. The experience of Einstein as a boy in Munich, sensitive to the spectre of militarism in school and street; his decision in 1922 shortly before his receipt of the Nobel Prize to leave Germany after the assassination of Walter Rathenau.

nau; his correspondence with some of his colleagues and with German officials after the victory of the Hitlerites—all this unhappy material need not be overlooked by the historian of political events and statescraft.

Similarly, the careers of Fermi or Szilard are cases that no historian of political events need neglect if he fully wishes to understand our time. For that matter, the history of absorption and integration of refugee scientists from fascism into the universities of England and the U.S. can provide valuable leads to a better understanding of the structure and flexibility of these institutions. The scientific community, being relatively easy to identify, isolate, and study historically, lends itself particularly well to these kinds of studies. On the other hand, the involvement of scientists in all major countries with the war machine today is surely an important part of contemporary history—as is, in a more hopeful vein, the Pugwash movement of scientists.

One can be more ambitious still. If historians of science have prepared the ground properly, historical research is not the only area that can benefit by looking at the work of scientists. Ours is a corpus of knowledge to which scholars from many different disciplines should routinely turn in the future when they want to know how the educational system of a country worked, or what the cognitive readiness of a student is at different life stages, or what family relations were like at a certain point in history, or how groups of people act in organizational settings and teams or respond to leadership, or how open or closed the democratic process was to inputs of technological expert information, or what the degree of anti-rationalist bias is among the people at large at a given time, or what the epistemological bias of a period was.

Also, we know that the study of psychology has received key insights for the theory of the structure of personality from studies centering on animals, hysterics, or psychotics, or on political and religious leaders like Luther and Gandhi. I see no reason why the next advance here should not come through the study of, say, a Bohr or a Fermi. Again and again, the history of science perceived widely enough will provide a ground which scholars from many disciplines can and should harvest for their own purposes, instead of so regularly neglecting this field.

In order to focus our courage and self-confidence in this intellectual mission, let me finally propose here a principle, a fruitful oversimplification of the type that has often worked in physics itself. I would like to

call it the Principle of Relativity in Historical Studies of Science. It goes as follows:

“In studying intellectual, institutional, and human events around him, an observer within a framework labeled ‘history of science’ will gain an understanding of these events that in quality and interest is on a par with the understanding gained by other observers in frameworks labeled ‘political theory,’ ‘socio-economic history,’ etc.”

This is not to say at all that all fields are interchangeable, just as in physics itself the observers looking from different frameworks do not have the same experience. Nor do I claim, on the other hand, that the history of science is inherently more valuable than any other field. The Principle of Relativity in Historical Studies of Science does not therefore either support or deny the much more ambitious aims some have expressed for our work—for example, Robert Oppenheimer, who expressed the hope in 1962 that “studies in the history of science can bring some coherence to the general intellectual and cultural life of our time.”

What the Principle does say is that there is an inner unity to all intellectual work, and that today the scholar in the history of science is no less warranted than one in any other field to claim a position as a central observer of the phenomena.

DISCUSSION

On papers by E. HIEBERT and G. HOLTON

JAMMER: I would like to ask Prof. Holton, in connection with this analysis of different world lines of which he speaks, whether one should not take into consideration a picture of scientific world-space, not taken from the inner world of the scientist himself, but taken from the history of the dissemination and especially the acceptance of his work, regarding this as an additional factor?

Some kind of indeterminism—chance or luck if you like—of quite external factors may be involved here, and I would like to illustrate it by asking you about the South American expedition of 1919 when, as you called it, the charisma of Einstein started. Now, Einstein at first had a different figure for the deviation of starlight, and in 1915 an astronomical expedition was already organized to go to Russia in order to put it to the test. But now you have the unexpected event coming in, namely World War I, and those astronomers were arrested, so that the expedition did not complete its work. In other words, the experimental test of Einstein's prediction did not take place and its wrong calculation could not be falsified.

Now what would have happened if the Russian expedition had taken place as planned and moreover falsified Einstein's prediction? Of course by itself it would not have mattered very much within science, because we know very well that even later Einstein did not pay any attention to experimental data. There is a famous story told by a lady who was in his study when the cable came in from the 1919 expedition—that Einstein was not interested in opening the cable at all, because he said it did not matter what they found. He was so fully convinced of the truth of it.

But the question is, would it have been possible for the whole image of Einstein to have been completely changed if the Russian expedition had completed its task?

*Editor's comment: Prof. E. Hiebert gave a different version of his paper at the conference than the one included here. As a result, the account of the discussion had to be severely curtailed.

HOLTON: I think we must not let "world lines" or trajectories as used in my analysis take on a meaning that is too concrete. The word trajectory apparently tends to make you involuntarily think of some causal point-by-point process or progress in some external field which determines the trajectory. This is not what I had in mind, and so I would not be surprised if "accident" comes in between one point and the next. There are no laws of history that tell us how to predetermine events. Only on looking back can one analyze what happened. So these are retrospective trajectories, in the sense of reconstruction.

As to the remark that Einstein made when Ilse Rosenthal-Schneider read the telegram in 1919—well, I think it is important to get this precisely right. She reports he was quite astonished at her enthusiasm and said: "But I knew that theory is correct." She asked what would have happened if the expedition had found a different result, and his answer was "Then I would have been sorry for the Dear Lord—the theory is correct."

I think the meaning is this—in his view the only way in which the expedition could have noticed a different result was if nature had arranged circumstances in a very unusual and painful way for his particular experimental test not to work. Sooner or later it would have worked out and Einstein would have been sorry for the Dear Lord to have gone to so much trouble in order to produce a different result in this case.

If you have a man with such extraordinary resonance, so deeply in touch with nature's ways, and with so much success to back him up, what happens is that he is not so accessible to reports of experimental "disproof." Somebody might come and say this experiment did not work out—but that would not yet disprove his theory. He would want to see a good physical reason why it had to be that way in order for him to take that experiment's results seriously. After all, to a degree that is what every physicist does in his work in his laboratory. When you first assemble and turn on equipment of any degree of sophistication, it usually doesn't work. You know that your equipment is really working only when it gives consistently results of the kind and general magnitude that you expected to get on the basis of some theory. Important "chance" or "surprise" results exist, but they come very rarely.

HIEBERT: This thesis seems to raise another question that touches upon Einstein's later life when he was less productive. Did he become a dis-

gruntled man during the last thirty years of his life because things did not work out so well for him? If Einstein had a special right to defy unfavorable experimental evidence, then perhaps he should not have become a disgruntled man.

HOLTON: There are very many reports by his collaborators like Straus, Maier, Infeld, Banesh Hoffmann and so on. They agree that Einstein had the characteristic of many good scientists which is, in the words of Anne Roe's study on the psychology of scientists, "an unreasonable optimism" in the face of discouraging results. The fact that the last 30 years were a period of no final success hides the other fact that he was attacking one of the greatest scientific problems of all time. He felt he was on the right track, and from time to time he saw an improvement, and that became the satisfaction for the moment. When a false step had to be taken back—the record is very clear on that—the next morning he and his assistant would sit down with a new idea and begin work on that. There was no time to cry over ideas that had not turned out well.

SCIENTISTS AS SLEEPWALKERS

JOSEPH AGASSI

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The problem of the present talk is, does the research worker know what he is doing, and to what extent? I shall not answer but merely present and explain this problem.

The title of my present discourse averts to Arthur Koestler's *The Sleepwalkers*, whose subtitle is: *A history of man's changing image of the universe*. According to Koestler, it seems, scientists are like sleepwalkers: ignorant of what they do, they do it with perfect assurance and with success. But I am not quite clear about that. Only in one passage does Koestler speak openly on this issue. He likens Kepler to a sleepwalker there, and in that manner: with the sleepwalker's assurance, etc. That passage, by the way, concerns two errors of computation which Kepler made and which cancelled each other out. This startling fact is explicable, and hence it is much less startling than it seems. Kepler calculated to a very high degree of precision, far beyond factors which might influence the accuracy of his results. Indeed, by sheer probabilities, he could have made a few more computational errors without being much the worse for it. It seems, then, that Koestler's likening of scientists to sleepwalkers fizzles out at once.

Let me stress that it is not at all clear whether Koestler likens Kepler elsewhere to a sleepwalker, and whether he likens other men of science to sleepwalkers. But it looks that way. Koestler takes seriously Kepler's speculation of the *Mysterium*, according to which the planetary orbits are captured in perfect polygons, and Kepler's hope that this would explain the number of the planets and the ratios of the distances of the planets from the sun. Koestler says that this was Kepler's *leitmotif*, the erroneous but fruitful idea, the prejudice which miraculously led him to his destination—rather than to blind alleys—much like a sleepwalker, I suppose.

I shall go further. I will say that Koestler's metaphor is part illusion

part truism, yet a truism well-worth stressing. The illusion is that a sleep-walker who has survived all the hair-raising obstacles and arrived has arrived because he was predestined to arrive. Though Koestler does not assert the doctrine of predestination, he repeatedly harps on it—and harping on a popular prejudice is worse than explicitly asserting it. So much for the illusion. There is the truism that sleepwalking or awake we walk on a tightrope and that looking hard at the abyss is dangerous. It looks as if this amounts to saying, ignorance is bliss. Of course it is not, since we may more easily avoid risks when we know them; but we need some optimism as a better method than looking hard at danger all day long.

Consider the fact that Kepler's speculation is false. Had he known it was false he would not have undertaken his researches. Hence ignorance is bliss. So suggests Koestler. I say "suggests," as I am not clear about Koestler's thesis. Anyway, the suggestion is not convincing. Had Kepler known his speculation to be false he might have tried a better one, or he might not. We do not know. Moreover, as his speculation is false, though it spurred him on to do research it also blocked his progress. Indeed, Kepler was so prolific in producing his ideas and publishing them, he was so careless at times with his deductions, that it is all too obvious that he repeatedly found himself in blind alleys and said so out loud. Koestler quotes him occasionally admitting that he was in a blind alley, and at one time, as I said, he speaks of him going to his destination with the assurance of a sleepwalker. Koestler, it seems, cannot lose. Kepler's great progress proves he had to progress and Kepler's retardation proves he had to progress in spite of all obstacles. As I say, the only hard evidence Koestler has is that Kepler did make great discoveries, that he also goofed many times, that his main idea which was the motivation for his actions, successful and failed ones, was a failure.

Well, we all know of tactical success in the midst of strategic failure. What of it? So much for Kepler. But Koestler is in error about sleepwalkers too. Admittedly they are assured; yet successful they are not: they too meet with blind alleys and other mishaps. One does not have to be a sleepwalker to find oneself in a blind alley or to meet with an accident; but it helps. And so, the very image of Koestler of a scientist moving towards his unknown goal quite successfully yet closed-eyed is not really the truth even about sleepwalkers as such, only about an amaz-

ing feat of an amazing sleepwalker, about a quaint fact which has caught people's fancy.

Let us forget the lucky sleepwalker and ask, how does the scientist arrive? Is it by luck? Is it by predestination? Is it in any other way? Is it perhaps the case that quite unlike the sleepwalker the scientist knows what he is doing?

There is a theory of science which declares the scientist to be the one who has the sixth sense to guess right more often than others—and to guess, not to deduce. If science were merely mechanical deduction, so the argument goes, it would better be performed by computers; and if it were to make in a series all the errors there are to make, then common men would be better at it than men of science. No, says this theory; there are too many deductions to make and too many guesses to explore; the scientist senses, smells the avenues worth exploring, the paths worth taking. Science, thus, is an amazing feat, and justly catches people's fancy.

This theory says, we are all blind, we are all sleepwalkers; most of us naturally end up in blind alleys, and some end up at the right place—and thus earn the title of scientists. It is quite possible that this is what Koestler has in mind. It has been adumbrated already by many writers, including Knoblauch, Tyndall, Sir Oliver Lodge, and others. It was incorporated, I suppose, in the philosophy of Michael Polanyi and echoed by Thomas S. Kuhn. I suppose that to the extent that Koestler propagates a consistent doctrine he may have this very suggestion in mind.

For my part I find this suggestion not very interesting, as it is permeated with materialism, namely the worship of success: We all fumble and some of us nonetheless arrive; hurray for them! I shall not stoop so low as to attack such a doctrine. Were Koestler advocating nothing but materialism, nothing but success worship, his work would not be so important. Admittedly, his approval of Kepler made commentators like his chapters on Kepler and his disapproval of Galileo made the same commentators dislike his chapters on Galileo. This, however, only makes Koestler's commentators more materialistic, more success-worshippers, than he is. But I still wonder, what does Koestler's *Sleepwalkers* tell us about science in general? It tells us that all astronomers up to Kepler's contemporaries loved circles but he broke the magic and invented the elliptical orbit. It tells us that science is a mixture of the rational and the

irrational, as practiced by the Pythagoreans and their successors up to and including Kepler; but excluding Galileo and his followers who attacked religion as irrational and who wished to see science as purely rational.

I do not know how these two theses relate; how can it be both that circles are all bad and yet that they illustrate partial rationality which is good; how was Galileo alone too bad because he held the circle; is this *because* he advocated a purer rationality; how the pure rationality of modern science is all bad even though science developed so much. Koestler himself notes the great modern development—but only when he compares mankind to a psychopath—morally backward and physically strong enough to be self-destructive. It seems that Koestler only notices a fact when it helps him moralize. But I must leave Koestler now and center on pure rationality versus partial rationality, as it may hopefully offer us the key to his appealing metaphor of the sleepwalker.

The appeal of the metaphor is partly in its paradox: the sleepwalker is ever so blind, so poor in rationality, and the scientist is ever so rational. Partly the appeal is in a fresh idea which the metaphor contains. We are so used to contrasting rationality or reason with irrationality or unreason, that we forget the more commonsense idea of reason, the contrasting of reason with stupidity and the fruit of reason with sheer luck. In Yiddish the successful is normally expected to express humility by applying the expression, “with more luck than wit.” Here the fruit of reason, the wages of hard thinking, is contrasted with grace: success may be a reward for thinking or not:—the Lord, says the Psalmist, protecteth fools. Within such a universe of discourse, there is no room for irrationalism; even the irrationality of the hothead is here viewed as but one example of a case where not all possible foresight was employed: irrationality, from the viewpoint of common sense, is but a rationalization of foolishness, and foolishness is but an affliction.

Let me elaborate a bit on this common sense idea and show the difficulties inherent in any attempt to apply it to scientific research proper. What I like about this idea is its immense common sense. From the common sense viewpoint rationality is so straightforward and desirable that irrationality is automatically placed not as a rival but as an affliction—as a matter of course, of course. But the rationality which can be taken so much for granted, itself takes too much for granted; it is too naive to be

of use for science. For, when common sense contrasts wit with luck, it implies that wit—being foresight—leads to success without luck; that wit, in other words, must deliver the goods repeatedly and at will.

Let us take a simple example. If a patient recovers from a fatal disease, for example, we immediately employ common sense and say, either it is by luck, or the doctor used his foresight—he has a new method of cure. We do not distinguish between a recovery due to a lucky event in the domain of the patient or in that of the doctor's; we distinguish between the case of an accident which has caused the cure—by luck—and the foreseeable case, the one based on a routine which causes the cure, which surely is more a matter of wit than of luck. Of course, if the doctor's routine increases the chance of recovery from very low to very high, then, surely, we will all say it is not luck but wit. Even if the doctor's routine merely increases the chance of recovery, say from 30 to 60 percent, *if it does so regularly*, if it is routine, then the recovery is in part by wit, indeed, 30 percent wit. We can mix wit and luck, then; but they do not overlap. Or do they?

For now comes, indeed, the sixty-four dollar question. Given the method of cure, we say, a given cure can be foreseen as the effect is repeatable, and it thereby makes the cure more a matter of wit and less a matter of luck. But is the discovery of the method of cure itself a matter of wit, or is it a matter of luck? When I am in a cautious mood, I tend to say, common sense is limited in its application to common phenomena, and the discovery of a new cure is anything but common; and so, my question does not obtain. In a bold mood one may repeat the Einstein-Born hypothesis: science is but an extension of common sense. This hypothesis is very bold, and perhaps even false. It is in any case fascinating and worth pursuing for a while.

In our own case, pursuing the Einstein-Born hypothesis will force us to say, if a discoverer can discover at will, he does so more by wit than by luck; and if it is by luck then it is not producible at will. This fits extremely well with Einstein's claim that his successes were more due to luck than to wit: he had little control, he said, over his results. I do not think many people would accuse Einstein of false humility, even of the false humility which is accepted in polite society as *comme il faut*; it is well-known that Einstein's humility was both considerable and sincere—it was the humility becoming to people as members of the species.

We do not have a scientific method, and so all discovery is a matter of luck.

Before debating this point, let us acknowledge that we owe it to Sir Francis Bacon, one of the most brilliant and ingenious philosophers of all times. He was a man in many ways the opposite of Einstein: cruel, dishonest, quarrelsome, vain; a strange mixture of conceit and arrogance, and unlike Einstein he believed in scientific method. Yet, like Einstein he was brilliant, and like Einstein, he contrasted wit with luck; even like Einstein, he put ingenuity on the side of luck rather than of wit.

Discovery, says Bacon, can be compared with the drawing of a straight line or of a perfect circle. With a ruler or a compass you can perform this task repeatedly and with ease. The knowledgeable draftsman is usually the one who uses these tools and with their aid performs the task repeatedly and at will. Without the tools, one needs immense luck to perform them, to draw reasonably straight lines or round circles. Bacon, unlike Einstein, thought that scientific method was possible. He also thought that it is given only to the one who is humble enough yet ambitious, nimble enough though cautious, etc. And, he humbly confessed, he was lucky enough to fit this description very well and so he intended to become a great discoverer—if he only had a bit of time and a few assistants. These he unfortunately never had, much to the lament of his disciples, including the great Amos Comenius.

The drawing of lines and circles according to wit is an art given to one schooled in the trade of craftsmanship. Also, by sheer luck, on occasion, the same art is given to any Tom, Dick, or Harry; perhaps for a moment or two in his life. Also, on a rare occasion, we do meet a gifted person who, without schooling at all, has the same art for his whole lifetime: he can deliver the goods at will. His gift or his knack is his luck: though he can reproduce the job at will, he himself is a lucky streak; his knack, to be precise, is. We cannot, that is, produce at will people who, without schooling, have the ability to discover. And this, by our very criterion, makes the existence of people with a knack a matter of luck, even though a knack is something which enables its possessor to repeat his performance at will.

To show how commonsense this view of Bacon's is, let us take a simple problem. Take a trade school—say, Harvard University Architecture Department. Suppose it produces streams of able draftsmen every

year. Is this a matter of wit or luck? In a sense our question is already answered: since the able draftsmen are produced regularly, clearly Harvard University Architecture Department exhibits a certain wit, which we call educational proficiency or such. But we may question this. We may say, one in a thousand is, by sheer luck, a born draftsman, and he naturally gravitates towards Harvard, the Fair Haven of the born draftsmen. If this were so,—personally I will not enter this debate—clearly, we would all agree, Harvard only can draw the best, not produce the best. It does have wit, we would still admit; namely, the wit of repeatedly drawing the best, of repeatedly creating the illusion that it repeatedly creates the best (rather than attract them), etc. Right or wrong, this discussion as I have outlined it is very commonsense, regardless of the question of the truth or falsity of specific claims it contains. Also it is in complete agreement with both Bacon and Einstein about the contrast between wit and luck. If so, then we can take what the two thinkers agree about and approach their central disagreement. As it happens, we all today agree with Einstein and disagree with Bacon. There is no scientific method, no science-making algorithm; and therefore, we must conclude with the philosopher Einstein, that the scientist Einstein—or any other scientist—is successful more due to luck than to wit.

There is an obvious objection to this, which I shall briefly note before winding up my present commentary on a Yiddish expression (regarding luck and wit). The idea that there is no science-making algorithm, one may say, overlooks the fact that Einstein made quite a few discoveries, whereas most of us make none. Though there is no science-making algorithm, Einstein had a higher than average chance of making a discovery. Admittedly, Einstein had no guarantee—the like of which Bacon had vainly promised—that effort over a given span of time would lead to discovery. Yet, he had a higher than average likelihood, and this is better than mere luck. Just as a doctor would prove his use of his wit by a high success ratio even if he could not give perfect cure procedure, so the very existence of an Einstein does prove that something like a science-making algorithm does exist—perhaps some partial algorithms.

So much for the objection. It has been answered here already. I shall repeat the answer. Never mind the question “do partial algorithms exist?” Had we been able to produce an Einstein every generation we would, thereby, create something akin to a science making machine—

whether because Einsteins do use partial algorithms or for any other reason. But we cannot produce an Einstein. Let us assume, from now on, that an Einstein does and will appear in every generation. It is still doubtful that the objection holds. For, it is one thing to have by luck one Einstein in every generation of billions of offsprings and another thing to say that we produce him. Of course, there are people who say that Einsteins are produced in the best intellectual hothouses. The facts speak differently. And so, Einsteins, to conclude my answer, are not produced: they just happen.

My answer to the objection is not complete. We may nevertheless insist that we do produce an Einstein every now and then, a big one or a small one, at long intervals or short ones, simply by what is euphemistically known as replenishing the earth. Since this sounds so very silly an idea, let me draw attention to the fact that this is what a well-known philosophy, namely apriorism, amounts to. If knowledge is inborn, then the only way to bring about knowledge is to bring about birth, the only creation of knowledge is procreation. Since apriorism is so commonly held to be a rather silly and old-fashioned doctrine, I shall now argue that it is far from being extinct or even unpopular—it has simply gone underground.

Descartes lived at the peak of the Renaissance, when the sense of awakening was the strongest, when it was easiest for a thinker to say, my teachers were great ignoramuses, and I am a great scholar—my knowledge is truly mine. And yet, this very stance raises a brow: what is so special about Descartes that he should be the first learned man? How come he, of all people, knew more than all his teachers put together? Descartes must have been asked this question. He said, seemingly in reply, that being so special is not so surprising; for example, that a town planned by one man is better than one planned by many different people, which is to say an unplanned town. Descartes could say, perhaps, too many cooks spoil the broth. Yet, no doubt, all this is but an aside. For a town or a broth reflects one man's taste, whereas knowledge reflects not tastes but the truth. What made Descartes so special, said Descartes, is the fact that he was methodical. Here we come again to Bacon's ruler and compass idea. There is a difference between Bacon's detailed views of method and Descartes'. To the insider the details are of supreme importance; to the outsider what matters more is the fact that they both

recommended a conscious and methodical application of a fixed and simple set of rules. Indeed, for Gassendi, not even much of an outsider, Descartes was an immediate disciple of Bacon; for Mersenne the difference was more troublesome, and Descartes tried to pacify him.

The point to stress is that both Bacon and Descartes explained the allegedly sharp transition from the depth of the Dark Ages to the enlightenment of today: We have an algorithm, we know that we have an algorithm, and so we can start cranking the computer. Bacon and Descartes both said explicitly that the knowledge of the method must be conscious, and so must be its rigorous use. In a moving passage, in his posthumous fragment *Valerius Terminus*, Bacon says, unless one knows the seats and pores and passages of the mind, one is not qualified to do research. Clearly, this raises serious doubts about the starting point: how do we do research to find out the algorithm if we need the algorithm to do research?

My intention here is not so much to raise doubts about Bacon and Descartes, however, nor to question their apriorism. My intention is to notice their stress on the need for an explicit knowledge, as well as for conscious and extremely methodical application of set rules; this stress is too much common sense to require comment. But let me give you a simile. We do have computers, and at times we do use them intelligently, whereas our forefathers had no computers and many of our own contemporaries just fiddle with their computers until they run out of public funds. Why? What is so special about us? How come only we use computers properly? Obviously, we know the rules, and we use them properly to build and employ computers. This much, one must notice, is our seventeenth century heritage. We view our privileged ability to use properly our computers the way the seventeenth century pioneers viewed their privileged ability to use properly their minds. Here I must speak briefly of partial algorithms—a topic to which I have already given a few papers. That these algorithms exist is a fact, although they are not always as precise as to be called algorithms proper. But analyzing and synthesizing chemicals is a good example for them; some people devote their scientific careers to jobs which yield results with regularity.

Polanyi and Kuhn view this as irrational as there is no guarantee for the success of the algorithm. I find this hypersophisticated since by common sense following an algorithm is rational, repeatable etc. And if the

algorithm is exhausted, then the one who applies it and fails for the first time is extremely lucky as he found—by serendipity—a much greater find than the one he was looking for, to wit, the limit of the partial algorithm.

I do not wish to endorse the view of Polanyi and Kuhn about normal science. Contrary to their view, I assert that many ordinary scientists do little of any significance, and that many ordinary scientists perform small scientific revolutions, i.e. refute small accepted scientific hypotheses. But I do agree with them that some scientists follow partial algorithms—a fact which I consider rational and they view as irrational.

I have mentioned all this not so much in order to criticize Kuhn as to illustrate a common sense view of rationality which, when overgeneralized in an over-optimistic mood leads us straight to Bacon's and to Descartes' view of science as the epitome of rationality. There is, then, an element in these people's view which exceeds common sense by overstressing it. Yet they had a strong common sense aspect to their philosophy of science which we may profitably keep in mind.

Then came Newton, and what method he used God only knows. What he said about method is either puzzling or not worthy of his great intellect—as E. A. Burtt has noticed. To take one example, he said that we have to adopt a generalization which is not violated by facts until it is, and then cling to it with proper qualifications. This would make a European say all swans are white except those in Australia, and it would make the Australian say, all swans are black except, etc.

Perhaps I am unfair to Newton; perhaps his idea of generalization has nothing to do with the color of swans. It was a bold statement that Max Jammer and Stephen Toulmin made on a different occasion, to the effect that "all swans are white" is unscientific. I do tend to agree, but will not say whether Jammer or Toulmin himself would stick to this extremist and highly Kantian remark.

Kant said that only a generalization cast in the conceptual scheme of science is worthy of the name of a scientific experience. Before we can generalize about Mars's orbit, for example, we must have developed our mathematical framework so as to have the concept of an ellipse. Kant's theory, I suggest, fits Newton's *Mathematical Principles of Natural Philosophy* best: we build a framework, as in Newton's first two books, and then generalize within it, as he does in his third book. Yet

Kant's theory is also a disaster: there is no doubt that Newton himself would not have accepted Kant's analysis of Newton's intellect any more than he would accept a Freudian analysis of his psyche.

It is well known that Kant was weak about the application of his theory of knowledge to the history of science. He mumbled some traditional apriorist remarks about observations awakening knowledge which is, however, a priori valid. His chief contemporary critic, Solomon Maimon, flatly declared inapplicability to history to be the chief weakness of Kant's theory of knowledge. This, like most of Maimon's contributions, was overlooked.

The root of the trouble is, of course, in the difficulty we have seeing a man systematically planning to do one thing and doing another. Freud has meanwhile shown us that we can act this way and make fairly good sense. But this is all on a rather primitive level. On a more sophisticated level the subconscious and the id are just too dumb. We can imagine, thanks to Freud, a man seemingly concerned with problems of other people, yet with personal interest at heart. But we cannot, in any like manner, imagine a man thinking he plays a piano while he effectively programs a computer—or vice versa. It is too much even for a sleep-walker to follow Kant while thinking that he follows Bacon. Things get so bad, that the best advice is often to ignore the question altogether. It came to me like a brick on the head that nowhere in I. B. Cohen's *Franklin and Newton* is there a discussion of the fact that the method these gentlemen had allegedly employed is different from what they said they had employed, that indeed they staunchly opposed the method Cohen ascribes to them. The problem is just overlooked by almost no reference at all to this puzzling fact, and by engagement in polemics with those who today share the view of Newton and Franklin on science.

It was Pierre Duhem, I think, who started it all, but at least he was very explicit about what he was doing. And Sir Karl Popper gave Duhem's idea the rubber stamp of approval, while using Einstein's famous joke. Do not listen to what a scientist says he does, said Einstein, look at what in fact he does. This was merely the application of the inductive method against Newton's claim that he had employed the inductive method—or rather against the claim that Newton knew best what Newton did. Clearly this is untrue, and Einstein's joke is the reduction of the claim to an absurdity. Einstein himself, of course, meant no more than

a criticism, as he makes clear in his comment on his joke: he says, this should silence me too, but I shall not be silenced. The question remains, if Newton thought he was employing one method, how could he employ another, and systematically so?

As I said, psychology cannot offer an answer, as subconscious tricks are not sophisticated enough. But social anthropology can offer something more sophisticated since when we follow a tradition we may be following something more sophisticated than we can produce ourselves. As Malinowski showed empirically in the case of the Kula in the Trobriand Islands, tribal rituals may be a sophisticated method of keeping the economy going, yet without the knowledge of the practitioners who ascribe to the ritual a mere magical symbolic meaning. To apply this functionalism, as it is called, to our case is not difficult. The model here is not the scientist operating a computer while thinking he plays Bach, but the apprentice who has fairly mystical ideas about the computer, yet who operates, to the extent that he does, impeccably. I have mentioned the apprentice in allusion to Michael Polanyi. The leaders of the scientific community, unlike other chieftains, have to innovate. Thereby, Polanyi adds, they take the chance: if the herd does not follow them they lose their positions as leaders. This sounds so trite, perhaps even tautologous, that one can understand Kuhn's failure to mention it. Of course, if you are not followed you lose your leadership. Yet looking at it thus, Kuhn fails to mention, and his readers thus lose, the incredible hypothesis concealed in the discussion: when the leader makes a good move he is followed by the herd, but his bad move is not. Hence, the mob of scientists, the normal scientists so-called, have the sleepwalker's assurance: ignorant and dogmatic as they admittedly are, nonetheless they follow the right leader to their destination. This is part and parcel of the Polanyi-Kuhn lore.

And so, the introduction of social anthropology into the picture only replaces the individual sleepwalker by a crowd of sleepwalkers. This is true not by virtue of our subject matter but by virtue of the subject-matter of anthropology: not only Duhem created a problem, but Malinowski created its analogue. Indeed, he even felt the problem, for he tried to solve it—by invoking the name of Darwin. I do feel, by the way, it is high time we coin the phrase "Darwin ex machina." Natural selection, says Malinowski, eliminates the unfunctional cultures, so that all

existing cultures are, *eo ipso*, well functioning, or functional. Applying this final touch to the Polanyi-Kuhn sociology of knowledge will make the crowd of scientists not sleepwalkers but lemmings, some of which are selected or elected, some of which are doomed. It makes me shudder. I prefer sleepwalkers to lemmings.

I do not share the Polanyi-Kuhn hypothesis about leaders and rank-and-file. I think the sociology of science is a complex and variable phenomenon. I think, with Dr. William Whewell and Sir Karl Popper, that genuine scientific discovery, small or large, is in part a matter of genius, creative genius, creative imagination. Partly it is also a matter of luck, and, of course, partly a matter of wit or schooling or established scientific practices. I do not doubt, myself, that the established scientific practices, though not quite algorithmic, are rational par excellence, that common sense deviation from them according to the context and its requirement are equally rational (as they occur simultaneously to various investigators), that utilizing any chance insight is similarly rational. I assume, with some misgiving, that the chance insight is not rational and we cannot produce it at will though we can produce it by worrying about problems. So much Whewell and Popper. Query: does this amount to a science-making sausage machine in any sense of the word, however weak? Whewell evaded this question. Popper says emphatically, no. I am at a loss to find the answer or even the way to approach it. Are insights matters of pure luck?

The fact remains: most great thinkers were imbued with a profound sense of their being very lucky, with a strange sense of gratitude even. We find it in Kepler, in Galileo, in Einstein. When a man like Newton avoids such expressions, it is obvious that he is tremendously ambivalent. At times he thought nothing of his works, viewing himself as a mere child standing at the shore of the ocean of truth merely picking up a shiny pebble; at times he valued his work some, and then he said he was a dwarf standing on the shoulders of giants. It is no surprise that when he was forced—as he falsely felt he was—to defend his originality, his own significance, he was venomous.

I say this not in order to reduce the greatest man of science to a psychological case, I say this as an admission that psychology as well as social anthropology do enter the picture. Yet the fact remains that Duhem, Popper, I. B. Cohen, all say Newton only thought he was employ-

ing his method, but in fact he consistently employed theirs. This is *apriorism* the like of which even Descartes never dared pose. How lucky of Newton to be an unknowing disciple of Duhem, Popper, or I. B. Cohen!

Is the sleepwalker lucky each time? Is there a consistency here, a lucky streak? A lucky streak, as opposed to a case of luck, is what we are talking about. Repeatedly science advances, and nobody knows why. Perhaps the Darwin *ex machina* is right. Perhaps failure is left behind yet success accumulates. If so, then, as I have said before, the method of creation is the method of procreation. In a sense we all think so, Darwin or no Darwin. We all think, if we only survive as a species, and if we avoid stupid disasters like atomic warfare and population explosion, then all will be well. Wealth is bound, then, to accumulate; material, technological, intellectual. Of all the science-fiction novels and novellettes and stories which came my way, I found only one which contemplates a different possibility, and it is E. M. Forster's lovely short story, "The Machine Stops."

Can the machine stop? Popper keeps saying, science is not a sausage making machine, it is not guaranteed to advance ever. True; we can all imagine all sorts of stoppage. But we do not for a moment doubt that given enough occasions, and chances however small, and we must win. By Bernoulli's law, we say. True, Bernoulli's law plus the hypothesis that knowledge accumulates will guarantee success without a science-making machine, by sheer random efforts. Random effort then is a weak version of a science-making machine, even.

Moreover, though we do not even know if Bernoulli's law meaningfully applies here, yet we think anyway we stand a better chance than mere randomness. Is this luck or is it wit? Where do we stand now anyway? A glance at a projection, whether of scientific advisers to governments or of the Hudson Institute, shows the facts: we are optimistic without knowing what about, never mind why. Do we trust our luck or our wit? What is genius—in science, in the arts, in devising a new chess gambit; is genius luck or is it wit? Is genius at all describable with the help of these distinct categories? Is genius the ability to take a chance at a lucky moment or is it inspiration? Can genius offer us the way to break away from the dichotomy between luck and wit? Can systematic luck? Or can a lucky streak? Is sleepwalking unharmed a kind of a knack or

of genius? Is there a method to develop the knack, say of a gypsy violinist? Is a gypsy violinist so very different from a Yehudi Menuhin?

The idea of scientists as sleepwalkers looks absurd at first. Only at first. Science, we say, is the peak of rationality, and sleepwalking is as blind as can be. But if scientific discovery is inspired and if sleepwalking is a knack, can any of us here say whether a scientist was inspired or had a knack? A thinker or an artist walk in a *terra incognita*, like a blind man, yet he does what he has set on to do, and as best he knows how. This, it seems to me, is the epitome of rationality. Calling it sleepwalking is but a poignant way of stressing that reason is limited, that the best vision is merely partial. The problem is, can we delimit and describe this partial rationality?

REVOLUTION AND REDUCTION:

The sociology of methodological and philosophical concerns
in nineteenth century biology*

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In a letter to his father written in the heady spring days of 1848, Rudolf Virchow confessed his optimistic world view of science:

"For a long time I have been conscious of the fact that I stand free and open-eyed in my times, and assimilate their trends early and quickly. I have often misjudged people, the times never. This has given me the advantage of being now not half a man, but a whole one whose medical beliefs fuse with his political and social ones. As a natural scientist I can be but a republican. The republic is the only form in which the claims, derived from the laws of nature and the nature of man can be realized."¹

The clear sense of fusing the scientific with the political and social was a hallmark of Virchow's early career. Was it the aberration of a single man of science who linked his newly developing philosophy of science to the philosophies of political and social reform, or did the reformist and revolutionary movements of the 1840's leave their mark on many scientific practitioners? In the specific instance, was the outspoken materialism and mechanical-physical reductionism of physiology and medicine of the late 1840's linked in any important way to the revolutionary era of 1848? These are the immediate questions of a much more general quest.

Can we talk in any meaningful way about a sociology of scientific knowledge? Can we go beyond theoretical statements and methodological pronouncements to actual historical problems? After all, since sci-

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¹Cited in the excellent study by Erwin H. Ackermann, *Rudolf Virchow: Doctor, Statesman, Anthropologist*, Madison, 1953, p. 16, from Marie Rabl, ed., *Rudolf Virchow, Briefe an seine Eltern, 1839 bis 1864*, Leipzig, 1907, pp. 144-45.

ence is done by humans, we should not be surprised at the idea of there being a relationship between the social structures and the cognitive structures which humans create and live with. Nonetheless, our historical treatment of science to-date has assumed an internal coherence at the cognitive level which has all but ruled out fruitful examinations of the interactions between science and the institutional and social forms with which it is surrounded.

The weakness of the limited scope of investigations in the history of science was brought home to me with great clarity at a recent conference on "The Foundations of Scientific Method" in the 19th century.² In a series of individually interesting papers, each author explored the changing sequence of ideas in the scientific discipline he was examining and sought within them the sources for the sharp new concerns for method which were being expressed. It struck me as an auditor that the active interest in new method and new philosophy of science being identified was as much a sociological phenomenon as it was an isolated intellectual one. Indeed, the more closely I examined the problem, the more aware I became that a proper explanation of these methodological concerns must transcend the narrow limits of the ideas of a scientific discipline, and focus as well on the social processes in which the men of science were engaged and the relationship that their science had, at that time, to the society within which it was being practiced.³

My own earlier studies of 19th century physiology have focused on the nature of the explanatory models developed and the explicit emphasis in so much mid-19th century physiological writing on establishing a new method.⁴ As we turn to the works of Magendie, Dutrochet, Ber-

²"The Foundations of Scientific Method: The Nineteenth Century", conducted by the Department of History and Philosophy of Science, Indiana University, November 19-22, 1970.

³Shortly after an earlier version of this paper had been presented, I was introduced to the essay by Robert K. Merton, "Social Conflict Over Styles of Sociological Work", *Trans. Fourth World Cong. Sociol.*, Louvain, 1959, vol. 3, pp. 21-44 (reprinted in Larry T. Reynolds and Janice M. Reynolds eds., *The Sociology of Sociology*, New York, 1970, pp. 172-197). My formulation of the framework within which scientific ideas develop was much aided by the structure that Merton had adopted for his own analysis.

⁴See Everett Mendelsohn, "Physical Models and Physiological Concepts: Explanation in Nineteenth-Century Biology," *Brit. Jour. Hist. Sci.*, 2 (1965), 201-219; "The Biological Sciences in the Nineteenth Century: Some Problems and Sources," *History of Science*, 3 (1964), 39-59; "The Origin of Species vs. the Origin of Life", *Actes 13th Congr. Int'l d'Hist. Sci.* (Moscow, 1971), (in press).

nard or Schwann, Schleiden, Liebig, Virchow, DuBois-Reymond and Helmholz, we are struck by the conscious involvement in bio-philosophical and methodological problems. Each of these individuals prepared lengthy methodological prefaces or composed "popular" philosophical lectures. They were all involved in attempts to explain the work in which they were engaged, its location within the sciences, and what they believed to be the nature of a proper philosophy of the biological sciences and a proper method for pursuing research in their field.

To understand the sources of these methodological concerns and the structure of the new proposals, we certainly must be aware of the teachers these men had and the texts they were reading—in short, what has been called "the historical filiation of ideas". But for physiology, at that time coming through its birth pains as a newly independent discipline, the many attempts to define its proper methodology reflect also the social moves of institutionalization.⁵ The practitioners of physiology were differentiating their activities from their former patrons, anatomy and medicine, and were searching for an academic and intellectual identity. Justus von Liebig, as an organic chemist well aware of the process of new discipline formation, caught the sense of the physiologists' moves: "Physiology, clearly rests on a double foundation, on physiological physics itself based on anatomy and on physiological chemistry based on animal chemistry. From the marriage of these two will be born a new physiology, which will be to the science that goes by that name what modern chemistry is to eighteenth century chemistry."⁶

The continuing concern that Claude Bernard demonstrated for the methodology and philosophy of physiology, shown so clearly in the explicit statements opening each volume of his lectures and *Leçons* and in the very consciously constructed *Introduction to the Study of Experimental Medicine*, can be fully understood only when seen as part of a campaign to construct a new discipline, intellectually and institutionally. His disappointment that the latter goal was never achieved as fully in France as in Germany is no secret, while his efforts to define the approach

⁵For a study of the emergence of physiology as a university discipline, see A. Zloczower, "Career Opportunities and the Growth of Scientific Discovery in 19th century Germany with Special Reference to Physiology," M. A. Thesis, Hebrew University, Jerusalem, 1960 (unpublished).

⁶Justus von Liebig, J. Gardner ed., *Familiar Letters on Chemistry, and Its Relation to Commerce, Physiology and Agriculture*, New York, 1843.

of the physiologist succeeded in establishing a methodological alternative to vitalism that was organicistic and not reductionist.

I believe that it is no accident that it is in the two new disciplines of organic chemistry and physiology that the concept of the teaching laboratory emerged almost simultaneously, for here was a striking new technique for the training of a new generation of practitioners, a new technique for passing on in organized fashion the codes or moralities of science, the manner in which the scientist should work. These new modes of education emerged at just the time that campaigns were being waged to establish professorships and accompanying institutes in the new fields of physiology and organic chemistry.

Among the key figures involved in the early emergence of physiology as a recognizably separate discipline were Johannes Müller and François Magendie. Who were their students? Bernard was the student of Magendie, while among Müller's students were Virchow, Helmholtz, DuBois-Reymond, Carl Ludwig and Ernst Brücke. The methodological concerns expressed by this generation of scientists can be seen in part, at least, as an attempt to redefine a theory of science, its practice, its code of operation, its system of gathering evidence, displaying it and explaining it. That the patterns of institutionalization, as they were developed in France and Germany, turned out to be different should come as no surprise. For although much of the intellectual focus was similar, the social settings of France and Germany in the mid-nineteenth century were sufficiently different to make it almost certain that the institutional structures would vary.

If an important part of the interest in method and philosophy displayed by physiologists can be associated with the period of emergence of a new discipline, other aspects of the mid-19th century methodological interests in physiology (and other sciences as well) can be linked to the process of professionalization which involved all scientific fields (probably at differential rates) during the course of the century. Having discussed professionalization at length in an earlier study, I will not rehearse all the questions again, but instead will point to several features which seem particularly à propos.⁷

⁷Everett Mendelsohn, "The Emergence of Science as a Profession in Nineteenth-Century Europe," in Carl Hill ed., *The Management of Scientists*, Boston, 1964.

One phenomenon of the period of professionalization which is often overlooked is the attention given to public explanations of science. Popular scientific lectures, given publicly and then widely distributed in printed form, became an important new mode of communicating with the public in England, France and Germany during the mid-decades of the century. These lectures seemed to serve at least two functions beyond the obvious one of sharing information. First they had the effect of enlarging the pool from which new recruits to science could be drawn; secondly, they extended the audience from which to seek legitimization and support. Indeed, by turning to a public audience to discuss methodological and philosophical topics, science was engaging its wider society (or at least a selected aspect of it) in an exchange which suggests that society had recognizable claims upon science and its mode of operation, and that the scientific community expected that an interaction would occur.

An important feature of the process of professionalization clearly reflecting the efforts at discipline definition was the establishment during the early decades of the nineteenth century of specialized scientific societies. Each discipline created its own new organization, from the Astronomical Society to the Zoological Society, with the new disciplines forming their own independent group, or sub-section of a larger organization, just as soon as possible. Although these groups all came into being during a period of intense industrialization in Europe and Great Britain, a time when industry and technology were beginning to tap science in a substantial manner, their discussions and their publications are almost devoid of reference to applied science. Yet many of the members of these groups, when outside the halls of these specialized scientific societies, were involved in applied scientific projects. It seems to me that what occurred was a conscious separation of two functions of science. At just the time that science was becoming convincingly useful to industry, its practitioners involved themselves in attempts to distinguish between applied and pure science. This is another facet of the concern for method which was so prominent in the early decades of the century.

Among the most active public spokesmen for science are just those individuals who were engaged internally in the process of redefining the discipline or arguing for a new philosophical mode. Helmholtz, DuBois-Reymond, Liebig, Carl Vogt, Claude Bernard, all active men of science,

are joined by individuals who operated at the public interface of science and became important programmatic statesmen, even while not being contributors to scientific knowledge itself; one thinks of the significant impact that Louis Büchner's *Kraft und Stoff* (1855) had as a tract for materialism. In an effort to avoid confusion among the new generations coming to science, and to convince the legitimating public on whom science had to count for support of the importance of the scientific endeavor, statements for internal consumption and for external use were fashioned and disseminated.

But to return directly to the problem of context for the new philosophy of physiology and experimental medicine, what further evidence can we gather to provide a richer explanation, one which accommodates rather than excludes the immediate socio-cultural environment?

"The medical reform we mean," wrote Rudolph Virchow in 1848 in the pages of his newly founded medico-political weekly, "is a reform of science and society. . . . Eventually the days of March arrived, the great fight of criticism against authority, of natural science against dogma, of the eternal rights against the rules of human arbitrariness, this fight which had already twice shaken the European world broke out for the third time and victory was ours."⁸ It was not, of course, a victory by the end of the year, but for Virchow reflecting on the world around him, the role that science and medicine should play and the way in which that role should define science was quite clear. He was connecting an attitude of radical social reform with a movement for radical methodological and philosophical reform in science. In the case of Virchow, both these lay in the work of a single man, but they spread far beyond him to significant segments of the scientific community as well.

In his 1847 essay "*Über die Standpunkte in der wissenschaftlichen Medizin*," Virchow called for a "precise understanding of the conditions of the individual and of community life in order to establish the laws of medicine and philosophy as general laws of Mankind and only then", he added, "will the saying *scientia est potentia* be fulfilled."⁹ Virchow was

⁸Quoted from Ackermann, *Virchow*, p. 44, from articles originally printed in *Die Medizinische Reform* and reprinted in Rudolf Virchow, *Gesammelte Abhandlungen aus dem Gebiet der öffentlichen Medizin und der Seuchenlehre* (2 vols.), Berlin, 1879, vol. 1, pp. 78-9.

⁹Rudolf Virchow, "*Über die Standpunkte in der wissenschaftlichen Medizin*," *Arch. path. Anat.*, 1 (1847), translated in Leland J. Rother ed., *Disease, Life, and Man: Selected essays by Rudolf Virchow*, Stanford, 1958, p. 29.

challenging the philosophical confusions which he saw in his day, a confusion so well developed in Germany and so deeply imbedded into the German system. What was this philosophical confusion? He listed it in a single phrase: "Science for its own sake." "It certainly does not detract from the dignity of science", he wrote, "to come down off its pedestal and mingle with the people, and from the people science gains new strength."¹⁰

Virchow was reflecting the challenging political attitude of his day right within science. He was questioning the way in which science should operate, who its audience should be, and who its legitimizers should be. He was clearly demanding a change for science to match the political and social changes being demanded of the German polity. What he presented was a challenge to the commonplace morality of science in 1847 and 1848. It was a challenge to its code of operation; it was a challenge to its source of legitimization. It seemed certain to affect the way in which he and others would explain how science should be done, that is to say the methodologies, and the methodological statements that they would make.

On the basis of this political challenge to science, Virchow attempted to provide a corrective, a new manner in which "scientific investigation (should) proceed". The substance of the new method unites Virchow with his immediate contemporaries who were simultaneously engaged in their own reconstructions of the science of biology. What he proposed was a pathological physiology which is both reductionist in metaphysic and largely inductivist in its operation. The specifics, however, are less important than the fact that he believed that out of a radical change in his views of society—and in the way society is being organized—should come a change in the way science itself should operate.

The point that I am trying to make is a fairly simple one. There is a self-conscious concern with method, with philosophy of science which in Virchow and his associates is directly tied to a political crisis and to the movements and the organizations which grew up in and around this political crisis; that an examination of this interaction gives part of an understanding of the manner in which science proceeds, and the manner in which science explains itself.

¹⁰Ibid., p. 29.

But why should a crisis in society affect science? It is easy enough to recognize the impact that should be expected on the institutions of a society in crisis, and there is little doubt but that the institutional aspects of science are included. One need look no further than the French Revolution and its dismemberment and subsequent reconstruction of the Academy and the Schools for good evidence.¹¹ But there is more. The need to construct a new world view embraces philosophies of man and nature alike. Tom Paine, the American revolutionary, spotted what it was that a crisis does—it amplifies and exaggerates: "Their peculiar advantage is that they are touchstones of sincerity and bring things and men to light which might otherwise have lain forever undiscovered. . . . They sift out the hidden thoughts of men, and hold them up in public to the world."¹² And having sifted out and brought to light, they force choice.

It is not that physical-mechanical reductionism for biology was a new invention of 1848, rather that in the course of an intensifying social-political crisis, the philosophy of nature, together with the philosophy of the polity came under harsh scrutiny. For some in the community of scientists, particularly those in disciplines like medicine, with its substantial social component or physiology which was in the throes of establishing an independent identity, the concern for a new and radical philosophy was stimulated by the search for political and social reconstruction.

While the immediate source of methodological concerns can be closely linked to the process of institutionalization and discipline differentiation, the substance of the methodological re-definitions and the intensity with which they are pursued probably is strongly influenced by the nature of the political and ideological surroundings. This becomes more sharply apparent during periods of social crisis and the attendant moves towards new ideological structures.

In Rudolf Virchow, the search for a new meaning, a new reality, for medicine and physiology took both intellectual and institutional form. As a student in the laboratory of Johannes Müller during the same years

¹¹See the recent study by Roger Hahn, *The Anatomy of a Scientific Institution, The Paris Academy of Sciences, 1666–1803*, Berkeley, Los Angeles, 1971.

¹²Cited in Randolph Starn, "Historians and 'Crisis'", *Past and Present*, 52 (1971), 6, from Tom Paine's *The American Crisis*.

as DuBois-Reymond, Helmholtz and Brücke, Virchow was doubtless exposed to the incipient physicalism which has been traced to their slightly older compatriot, Theodor Schwann. The first clear expression of these views by Virchow came in the form of a public lecture, 3 May 1845, in celebration of the birthday of the founder of the Friedrich-Wilhelms Institut. His title made his point of view abundantly clear: "Über das Bedürfnis und die Richtigkeit einer Medizin vom mechanischen Standpunkte."¹³ The terminology he used and the examples he chose were often those familiar to the graduates of Müller's laboratory. The point of view expressed was the one that became the hallmark of the reductionist quadruprate, Brücke, DuBois-Reymond, Helmholtz and Ludwig. "The new medicine", declared the twenty-four-year-old Virchow, "has a mechanistic approach and its object is the establishment of a physics of the organism. It has shown that life is nothing more than the sum of phenomena which proceed from general physical and chemical (that is to say mechanical) laws. It denies the existence of an autocratic Life or Healing Force."¹⁴

What did Virchow think he was doing in a lecture like the one cited above (and, incidentally, the same points are made several years later in a published paper, "Über die Standpunkte in der wissenschaftlichen Medicin")? Something of the answer can be discerned in his correspondence. He confided to his father after his lecture, "Nothing has been examined properly. Everywhere one has to start from scratch again, and there is so much that sometimes one really despairs."¹⁵ Adopting an attitude of the necessity for total reconstruction, Virchow mirrored what his Berlin contemporaries were also doing. For him, as for them, the first new radical statements were not met with enthusiasm in the established scientific community—his lecture was turned down for publication by Wunderlich and Griesinger in their prestigious journal, the *Archiv für physiologische Heilkunde*. This rejection probably precipitated Virchow's taking the important institutional step of founding a new journal, the *Archiv für pathologische Anatomie und Physiologie und für klinische Medizin*, in 1947, which he edited jointly with Reinhardt. The publisher

¹³Rudolf Virchow, "Über das Bedürfnis und die Richtigkeit einer Medizin vom mechanischen Standpunkte," *Arch. path. Anat.*, 188 (1907), 1–21. Although read in 1845, the paper was not published until 1907.

¹⁴Ibid., p. 7.

¹⁵Ackermann, *Virchow*, p. 10, from Virchow, *Briefe*, pp. 96–7.

of this journal was Georg Reimer, identified as being sympathetic to political reform.¹⁶

The event that seems to have given greatest impetus and form to his medical-political convictions was the opportunity he had to survey the typhus epidemic in Upper Silesia. What emerged was a report which contained not only a fine clinical and epidemiological analysis, but identified the government, a political body, as bearing primary responsibility for a medical tragedy.¹⁷ There can be little doubt that Virchow was receptive to this conclusion, for he was already moving in a circle of liberal and radical intellectual and medical figures. But again he gave institutional form to nascent convictions through the establishment of a weekly magazine, *Die medizinische Reform*, which began publication in July of 1848.¹⁸ Although it survived the counterrevolution of the Fall of 1848, it succumbed in June, 1849.

The young Virchow, who fought at the barricades of 1848,¹⁹ lectured his colleagues on the necessity of a mechanistic physiology and medicine, publicly toyed with being a materialist, became a member of the Democratic Congress of Berlin in October 1848,²⁰ established a new journal for his kind of science, and launched a politically oriented magazine of medical reform, can be thought of as representing the energetic expression of the scientist interacting with the social and political processes of his day. Through his prodigious writing on medical, scientific and political subjects, we can follow quite closely the day to day developments of his philosophy of man, medicine and science. The dual thrusts of a seeming necessity from within science to provide greater methodological clarity and, from without, the great stimulus of movements for political and social reform, made the adoption of a mechanical viewpoint seem to him urgent and compelling. It does not take a great deal of

¹⁶Ackermann, *Virchow*, p. 12.

¹⁷Ibid., p. 15. See Rudolf Virchow, "Mittheilungen über die in Oberschlesien herrschende Typhus-Epidemie," *Arch. path. Anat.*, 2 (1849), 143–322.

¹⁸The journal was published weekly from 10 July 1848 to 29 June 1849. Ackermann refers to it as "the Berlin mouthpiece of a nation-wide medical reform movement." More about that movement can be found in the monograph by Erwin Ackermann, "Beiträge zur Geschichte der Medizinalreform von 1848," *Arch. Gesch. Med.*, 25 (1932), 61–183; see also Kurt Finkenrath, "Die Medizinalreform, die Geschichte der ersten deutschen ärztlichen Standesbewegung von 1800–1850," *Stud. Gesch. Med.*, 17 (1929).

¹⁹Ackermann, *Virchow*, p. 15; see also Virchow, *Briefe*, p. 137.

²⁰Ackermann, *Virchow*, p. 16.

insight to recognize in the writings of Virchow the growing fever pitch of new ideas, and the acceleration of the moves to sharpen methodological and philosophical distinctions, that accompanied the political and social crises of the early 1840's. His political and his scientific consciences are on display. But what stood out in the open for Virchow is more cryptic in the work of his contemporaries, and their expressions will be only partially understood if not seen in the context of the intellectually challenging events of the day.

John Tyndall—the English physicist and scientific publicist, the man who first translated Helmholtz into English—in one of his lectures to working men (notice the audience here), in Dundee, Scotland in 1867, caught something of the spirit that I am talking about. In his remarks, he was defending the studies of the transformation and conservation of force and energy. "It is perfectly vain," he said, "to attempt to stop inquiries as to the actual and possible actions of matter and force. Depend upon it, if a chemist by bringing the proper materials together in a retort or crucible could make a baby, he would do it. There is no law, moral or physical, forbidding him to do it. His inquiries in this direction are limited solely by his own capacity and the laws of matter and force. Let them pursue their studies in peace. It is only by such trials that they will learn the limit of their own powers."²¹ He was attempting to set a public frame with which science can operate—he was also setting out a paradigm or at least reflecting a new reductionist view which had forced its way into biology.

The intellectual roots of this kind of challenge are deep and varied and two are immediately obvious—Darwin and the development of physical reductionism. As an obvious witness, we can call upon a spectator of the day, the Queen's first Minister—also a novelist—Benjamin Disraeli. Remember the Monsignor in *Lothair* (1879) who claimed that "instead of Adam our ancestry is traced to the most grotesque of creatures, thought is phosphorus, the soul complex nerves, and our moral senses a secretion of sugar!"²² I will leave Darwinism aside (for this paper) but instead continue to address the problems of physical reductionism—where to look for it, why it came and what it said.

²¹John Tyndall, "Matter and Force. A Lecture to the Working men of Dundee," *Fragments of Science for Unscientific People*, New York, 1872, p. 92.

²²Benjamin Disraeli, *Lothair* (3 vols.), London, 1870.

I am talking about a fairly radical change in the framework of research programs, conceptual systems, or modes of understanding in the biological sciences. It clearly had deep implications for the way in which men would explain the work they were doing. The general claim was one of the reducibility of all biological activities, particularly functional biological activities to the laws, techniques, rules and assumptions of physics and chemistry. It affected both the obvious and the tacit dimension of scientific activity at the time. This is not to say that biology was at an end (although some attempted just this claim), but rather there was an implicit commitment that not much time would be spent on hypotheses which contradicted statements of the physical sciences.

The switch is by no means intuitive—the organism, after all, had for centuries been seen as the paradigm with the solar system as a mirror of an organicistic system. What goes into this kind of shift? Our understanding will be increased only if we can get beneath the broad sweep of generalizations about mechanism and vitalism to some of the incidents and activities that propelled biologists along the way.

Let me turn to problems surrounding Hermann von Helmholtz, who will be the focus of the remainder of this paper, and begin with a little of the prehistory of Helmholtz. It is in this context that we have to understand the role of "Naturphilosophie." The "Naturphilosophen" had adopted a way of looking at nature which had become particularly prominent on the German scientific scene. They carried over into science the romantic philosophy of Herder, Fichte and Schelling. I will not dwell on the ideas of polarities, transcendental forces and things of this sort. They did, however, provoke a very sharp response when the response finally came. It is the form of this rejection that I will turn to for the moment, for I think the form of the rejection gives insight into the extremism of the physicalism and the materialism that arose in Germany and with it the distrust of general laws that clearly permeated the scientific community.

This accounts for one important aspect of the difference between the French reaction to vitalistic ideas and the German reaction to vitalism at just this same period in history—the accompanying societal aspect I have already alluded to above. The point I would make is that the "Naturphilosophen" in their search for a unity in Nature, made a series of tacit assumptions about underlying common forces in Nature. They

expressed the claim that there was no dichotomy between nature and spirit, between body and soul, and that, therefore, the fields opened for study were broadened, such as physiology of the senses (including mind) and interconvertibility of forces.

The "Naturphilosophie" itself we know was rejected. In part, the rejection was brought by the same political-philosophical reform movements of the 1840's that witnessed a widespread development of a new materialism. The case of Virchow brings this out with force and clarity.

Among the most interesting figures in this shift is Johannes Müller and, because he sits almost at the "eye of the storm," he is one of the paradoxes in the history of science. Müller began his own scientific career as a staunch adherent of the "Naturphilosophie" and by it was led to do work in fields which would otherwise have been closed to an experimental scientist. He rejected "Naturphilosophie" in his early 30's, but continued both as an expert experimenter and a believer in extra-physical forces, or what we would generally call a vitalist.²³

We know that Müller was the most important teacher in physiology in the first half of the 19th century. Most of the men who held chairs in German universities in physiology from 1838 onward, were trained in his Berlin laboratories.²⁴ This fact in itself gives some idea of the kind of spread of a point of view which can occur through a single school. It is in his laboratory, on problems that he originally suggested, that the key figures in physical reductionism began their work. Remember Müller's was a newly founded teaching laboratory in a new scientific discipline in which attempts were being made to tease out what this new field should adopt as its special mode of operation as differentiated from other fields.

The students included Schwann, DuBois-Reymond, Helmholtz, Carl Ludwig, and Rudolf Virchow among others. Müller, it seemed, was able to provoke new ideas without taking offense to them, although he must have been sorely tried by Virchow and the other reformers when he served his term as Rector of the University of Berlin in 1848! His laboratory, we know, was the scene of the development of a very sophis-

²³The fullest biographical study is still that prepared by Emil DuBois-Reymond, "Gedächtnissrede auf Johannes Müller (1858)," *Reden*, Leipzig, 1887, vol. 2, pp. 143-334.

²⁴See Karl Rothschuh, *Geschichte der Physiologie*, Berlin, 1953, pp. 123-150.

ticated physical experimentation on physiological problems. It was the scene of the development of new instruments, physical instruments for physiology, which had not existed before, but which were becoming characteristic of this German group of physiologists. Approaches which Müller himself conscientiously avoided using were, nonetheless, adopted by his students. Theodor Schwann, somewhat older than the other men I mentioned, came first as a student to Müller in the early 1830's, and remained on as Müller's assistant in the laboratory until early 1839. While most often remembered for his cell studies, more important for my focus is the third section of his volume of microscopical researches published in 1839.²⁵ This volume concludes with a long philosophical section, attempting to explain the nature of biology and, thereby, to propose a new theory of cells, how they come into being, and how they work.²⁶ What he does though in this work is to propose a biochemical origin of all new cells, in a process directly analogous to crystallization. Indeed, he insists that explanation in biology should be physical until proven to the contrary. So he has taken the system and turned it on its head; he is groping for a new program for physiology. This new physical program was part of the ambience of Müller's laboratory in the early 1840's. DuBois-Reymond, who came to Müller's laboratory while Schwann was still there, refers to him as the first of the physicalists in his obituary.²⁷ In a student letter written by DuBois-Reymond while in the Müller laboratory, DuBois-Reymond spells out this reductionist approach and claims "Schwann ist fur mich ein halb Gott."²⁸ The strength of the claim is there—this intellectual source of the impetus for reductionism can be traced fairly clearly. The manner in which it was taken up by the younger members of the laboratory and the way in which it was transformed into a scientific-social credo is important to bear in mind.

When we turn to Helmholtz himself, we can watch him attempt to

²⁵Theodore Schwann, *Mikroskopische Untersuchungen über die Übereinstimmung in der Struktur und dem Wachsthum der Thiere und Pflanzen*, Berlin, 1839. See also Mendelsohn, "Physical Models", *loc. cit., passim*, and also E. Mendelsohn, "Schwann's Mistake," *Actes 10th Congr. Int'l. d'Hist. Sci.*, pp. 967-970.

²⁶T. Schwann, *Mikroskopische Untersuchungen*; see the translation by Henry Smith published by the Sydenham Society, London, 1847, pp. 161-215.

²⁷E. DuBois-Reymond, "Gedächtnisse," *op. cit.*, pp. 206-219.

²⁸Estelle DuBois-Reymond, *Jugendbriefe von Emil DuBois-Reymond an Eduard Hallmann*, Berlin, 1918, p. 88.

very consciously alter—radically alter—the philosophy of biology. Helmholtz was part of the Müller laboratory; he was one of the group who wrote their manifesto of 1847: "We four imagined [they looked back saying] that we should constitute physiology on a chemical-physical foundation and give it equal scientific rank with physics."²⁹ This is clearly an attempt to create a new field and to consciously establish for it a "central dogma". Brücke, DuBois-Reymond, Helmholtz and Ludwig are the four who, working together, set out to propagate a new philosophy for physiology. Each became a statesman for a program—really for a radical scientific policy not unconnected, as I have indicated, with the other radicalism of the 1840's. The range of response can be seen in a comparison of the overtly political statements of Virchow with the more philosophically clad words of the quadrupvirate.

Helmholtz opened his 1847 paper: "Über die Erhaltung der Kraft" with a clear indication of how he believes science, to be science, must work. "Theoretical natural science, if it is not to rest contented with half views of things, must bring its notions into harmony with the expressed requirements as to the nature of simple forces and with the consequences which flow from them. The work will be ended only when the reduction of natural phenomena to simple forces is complete and the proof given that this is the only reduction of which the phenomena are capable. If that reduction were then proved to be the necessary conceptual form for an interpretation of nature, objective truth could also be ascribed to it."³⁰

The task of science, as he sets it out, is clear—all phenomena are to be reduced to attractive and repulsive central forces, the intensity dependent solely upon the intervening distance. Analytical mechanics then, particles in motion, became the paradigm for the sciences. They are all assumed to be reducible to a material base. Helmholtz had taken on the job of providing the theoretical underpinning for an outlook in physiology (and now all the sciences) that was being enunciated programmatically by his Berlin compatriots.

²⁹Cited by O. Temkin, "Materialism in French and German Physiology of the Early Nineteenth Century," *Bull. Hist. Med.*, 20 (1946).

³⁰Hermann von Helmholtz, *Über die Erhaltung der Kraft*, Berlin, 1847; reprinted in *Wissenschaftliche Abhandlungen*, Leipzig, 2 (1882-3), 12-75; translation by John Tyndall in Taylor ed., *Scientific Memoirs*, London, 1854, vol. 1, pt. II, p. 114.

In July of 1847, when Helmholtz read his paper, he was a twenty-six-year-old medical graduate student. Virchow had presented his mechanistic challenge just two years earlier. Helmholtz read the paper to the Physikalische Gesellschaft of Berlin. His only previous scientific papers were in physiology.³¹ Let me underscore here the name of the society, the Physical Society, to which he read his paper as a young medical physiologist. For as we examine his training—his letters and his notes—and recognize the political and social milieu in which he was working, we can find something of the path by which Helmholtz came to his generalizations. Through the study of a physiological problem—animal heat—which, incidentally, is the same path taken by Julius Robert Mayer) Helmholtz was led directly to his study of conservation. There is a conscious attempt in his first papers to make this problem—animal heat—conform to the physics and chemistry of the 1840's. The new attitudes he developed are clear, and they were nourished in the milieu of the Müller Laboratory surrounded by this group of budding young physiologists who were physicalists in outlook.

It is important to recognize that it was three young physiologists—DuBois-Reymond, Brücke and Helmholtz, who were instrumental in founding the Berlin Physikalische Gesellschaft, together with the physicists Clausius, Siemens, Kirchoff, Karsten, Heintz and Knoblauch.³² With a new program in mind for physiology, they took the appropriate steps to institutionalize it. Just as Virchow founded a journal, they joined in establishing a scientific society which would exemplify their approach.³³ For them, physiology was a simple extension of physics into another realm, and they lived their attitude by taking physiological papers to this society. Virchow, after returning from the Silesian visit of 1847 and early

³¹Hermann von Helmholtz, "Bericht über die Theorie der physiologischen Wärmeerscheinungen für 1845", *Fortschritte der Physik*, 1845, pp. 346–355 (1847), which was a commentary on the work of J. Davy and Justus von Liebig; also "Wärme, physiologisch" from *Encyklopädisches Handwörterbuch der medicinischen Wissenschaften*, Berlin, 1845, reprinted in *Wiss. Abh.*, vol. II, pp. 680–725; "Über den Stoffverbrauch bei Muskelaction", *Arch. Anat. u. Physiol.*, 1845, pp. 72–83.

³²See *Die Fortschritte der Physik im Jahre 1845, dargestellt von der physikalischen Gesellschaft zu Berlin*. The editor is G. Karsten; the publisher is the same Georg Reimer who published Virchow's new journals. The Vorbericht of the first number sets out the claims of the Society and also lists the initial members, see pp. iii–x.

³³*Ibid.*

1848, became a member of the Physikalische Gesellschaft.³⁴ In 1849, when he accepted a post at Würzburg, Virchow was instrumental in founding a physico-medical society.³⁵

It is characteristic of all Helmholtz's early physiological work that it reflected the attitude of physiology as being an extension of physics. In 1845, while still a medical student with Müller, Helmholtz prepared a report on physiological heat, probably at Müller's suggestion.³⁶ Here he directly confronted the problem of vital force that had been raised by Liebig's recent recalculations of the sources of animal heat.³⁷ The question was whether the mechanical motion and heat produced by an animal could result fully from an animal's own metabolism, or was there some vital force component necessary? He provided a lengthy examination of the literature, going into the question in some detail, and including some of his own experiments. He then turned to tackle what to him was the key problem—the fact that a vital force should not be there.

First, Helmholtz claimed it is clear that the force contained in food, the ingesta, can only produce a certain amount of heat, and, regardless of the complications of its actions, it always produces exactly the same amount. Why should this be so? Helmholtz proceeds to answer: "This is because it is an established rule of mechanics that a certain quantity of motive force, whatever the complications of its mechanism, can always produce only the same quantity of motion." Helmholtz's thought processes are neatly emerging here—he is linking two fields of study, applying limited results of the conservation of one to the much greater ambiguity of the other. He has clearly picked up the recent thermochemical studies of G. H. Hess and adapted them to his immediate purposes.³⁸ It is a physical analogy to which he is willing to turn, since it fits his con-

³⁴*Fortschritte der Physik im Jahre 1848*, pp. vi–vii (1852).

³⁵Ackerknecht, Virchow, pp. 20–21. Their journal was entitled *Verh. der physikalisch-medicinischen Gesellschaft in Würzburg*, which began publication in 1850.

³⁶See note 31 above. Müller was the editor of both the *Encyclopädisches Handwörterbuch* and the *Archiv für Anatomie und Physiologie*. The other brief review of Davy & Liebig was presented to the Physikalische Gesellschaft and printed in their *Fortschritte*.

³⁷For an excellent summary of Liebig's work in this area, see F. L. Holme's "Introduction" to the reprint of Justus Liebig, *Animal Chemistry*, Johnson Reprint Corp., New York, 1964, especially pp. lxxiii–xc.

³⁸Helmholtz, "Wärme, physiologisch", *op. cit.*, pp. 699–700. See J. R. Partington, *A History of Chemistry*, London, 1964, vol. 4, pp. 608–9, for a review of work of G. H. Hess in thermochemistry.

ception of what constitutes a proper explanation in biology. His aim is clear—"since no known mechanical, electrical or other forces enter the organism in addition to the ingesta, the only alternative assumption [as to the source of animal heat] is that a peculiar force of organic bodies, the so-called vital force, directly produces natural forces *ad infinitum*. This assumption contradicts all the logical laws of mechanical science." Here he is calling for legitimation of a widely held assumption. He then proceeded to criticize those who put forward such a peculiar view: "To it we can theoretically oppose nothing in refutation of those physiologists who take the very essence of life to be precisely its incomprehensibility." Helmholtz had adopted, toward his assumed adversaries, the same sharply critical attitude that marked Virchow's 1845 lecture. To be a mechanist meant to be on the attack!

Helmholtz had chosen firm grounds upon which to make his first philosophical stand. His problem was to find the origin for the force in an animal. This is the examination in the problem of physiology which led him two years later to his formulation of a formalized principle of conservation of force. What Helmholtz was endeavoring to demonstrate is that all forces in a living system behave as physical and chemical forces do—exactly the claim that Virchow had made in the same year. And that, therefore, short of creating a *perpetuum mobile*—a horror to all other laws of natural science—no such thing as a vital force can exist. In his spare and remarkably sophisticated paper of 1847, he presented his arguments and his calculations for the principle of the conservation of force (energy).

Helmholtz had moved from the study of a limited biological problem to a generalization meant to include all of nature, animate and inanimate. But that is precisely the task he set himself in the introduction of his paper on conservation. He concluded the famous 1847 tract with a brief and modest attempt to demonstrate the idea of energy exchange in the chain linking the sun to plants, and thence to animals.

The paper, as he prepared it, was for presentation to the Physikalische Gesellschaft, and it is clear from his correspondence that the prospective audience was affecting the tone. In a letter to DuBois-Reymond in February 1847, Helmholtz writing from the military hospital at Potsdam, where he was doing his service, notes that he is forwarding a draft of his new paper, ". . . not because I think it is ready, for even in

reading it over I see that most likely none of it can stand, but because I do not yet see how many times I shall have to rewrite it before it is done, and I do want to know if you think the style in which it is written is one that will go down with the physicists. I pulled myself together at the last reading and threw everything overboard that savored of philosophy. . . ."³⁹ It seems fairly certain that the original philosophical introduction was much modified and that which was finally printed was written with an eye to the reaction that Helmholtz could expect from a scientific community grown shy of any attempts at discussing transcendental forces or unifying powers. That is to say Helmholtz responded to the physicists' fear of straying from strict empiricism into those areas which reminded them of the discredited "Naturphilosophie."

As it turned out, there was good reason for Helmholtz' hesitancy. Having presented his paper to the Society on 23 July 1847, he forwarded it to his former teacher—the Berlin physicist, Magnus—requesting that it be sent to Poggendorf for publication in the *Annalen der Physik*.⁴⁰ Although the paper had been received enthusiastically by the young physicists and physiologists, while DuBois-Reymond referred to it as "an historical document of great scientific impact for all time", Poggendorf rejected it as not being sufficiently experimental. At the urging of DuBois-Reymond, the paper was sent to Georg Reimer, the publisher who accepted it and brought it out as a separate pamphlet.

The meaning of Helmholtz' achievement was not lost on those involved in the campaign to propagate a mechanical and/or material view of life and the living. The very next year, DuBois-Reymond, in the Preface to his *Thierische Electricität*, was able to call upon Helmholtz' success as support for his efforts to dissolve biology into organic physics and organic chemistry.⁴¹ Carl Ludwig made full use of the Law of Conservation of force in his influential text *Lehrbuch der Physiologie des Men-*

³⁹Helmholtz to DuBois-Reymond, 2 February 1847. The letters are part of a collection of Helmholtz materials in the Staatsbibliothek formerly housed at Marburg and recently moved to Berlin. Many of the Helmholtz letters are partially cited in Leo Koenigsberger, *Hermann von Helmholtz* (3 vols.), Braunschweig, 1903; an abridged translation by F. A. Welby was published by Clarendon Press, Oxford, 1906 and republished by Dover, New York, 1965. See p. 37 of the latter.

⁴⁰Helmholtz to DuBois-Reymond, 6 August 1847. See also Koenigsberger, p. 42.

⁴¹Emil DuBois-Reymond, *Untersuchungen über thierische Elektricität*, Berlin, 1848.

*schen.*⁴² Finally, the new doctrine of the conservation of interconvertible forces became a cornerstone in Louis Büchner's polemical treatise *Kraft und Stoff*.⁴³

There remains little doubt but that the concept of physical reducibility had a very significant influence in the development of physiology, (and other scientific disciplines as well), albeit that the intensity of its claims diminished some with time. But the concept had further influence, far beyond science, when linked, as it so often was, to materialist claims. What I have attempted to demonstrate is the manner in which this significant new "mode of thought" was generated. It is clear that it had demonstrable intellectual roots (most ideas do); but it seems equally evident that the ideas of physical mechanical reductionism came sharply into focus as part of a process of securing institutional identity for physiology within the German context. And it is the German context of the 1840's, a period of intense social and political crisis and reconstruction, that provided the note of urgency, imperative for the newly proclaimed mechanical philosophy of science. In the hands of Hermann von Helmholtz, the concepts were shaped into a strong theoretical structure. His immediate compatriots—DuBois-Reymond, Brücke, and Ludwig—turned it into a programmatic campaign for methodological reform within science. And Rudolf Virchow exemplified the role of the man of science "engaged." The reformation he proclaimed was one of method and man, of both science and society.⁴⁴

⁴²Carl Ludwig, *Lehrbuch der Physiologie des Menschen* 2 vols., Heidelberg, 1852–54. This preface has been translated and annotated by M. H. Frank and J. J. Weiss, "The Introduction to Carl Ludwig's *Textbook of Human Physiology*", *Med. Hist.*, 10 (1966), 76–86.

⁴³Louis Büchner, *Kraft und Stoff*, Frankfurt a. Main, 1855. An English translation was made from the last German edition by J. Frederick Collingwood, *Force and Matter*, London, 1864; it includes the author's rejoinder to criticisms and an updating of arguments.

⁴⁴My own longer and more detailed study of the development of physiological explanation in the nineteenth century, in which comparisons are made among the several approaches to handling the relationship with the physical sciences, is still in progress.

DISCUSSION

On papers by J. AGASSI and E. MENDELSONH

LAKATOS: I should like to make an effort to define "methodology," a term so frequently used in Mendelsohn's paper. "Methodology" in the seventeenth century denoted the discipline which set out to find a universal machine which would answer any question whatsoever in a computable—"scientific" way: Leibniz dreamt of a machine in which, if one programmed a question, would flash up the answer. Today "methodology" or "logic of discovery" means—at least for Tarski or Popper—only a modest doctrine which sets out to find (not necessarily computable) criteria for appraisal of the relative scientific merits of different *already available theories*. Several such criteria have been proposed, e.g. simplicity, falsifiability, progress in research programmes. But all these criteria agree in sharply separating the appraisal of theories, *as they are published*, from the process which led to the articulated result.

For instance, if I want to judge the merits of Einstein's 1905 paper, it does not matter at all that it was written by Einstein. It would have had exactly the same value if typed out by a monkey. (One can easily calculate the probability with which a monkey, once taught to type, would type out Einstein's 1905 paper.) Thus one can, and I think one should, have "rationality" in *methodology*, while giving way to "irrationality" in discovery, in the creative process. In the latter I agree with Popper, Polanyi and Kuhn. (Few people have appreciated that Popper mentions Bergson with approval in his *Logik der Forschung*, while, say, Russell abhors Bergson as being irrationalist.)

Now let me add a footnote to all this. There is much less probability that the monkey should type out, say, Newton's *Principia* than just the inverse square law—and this is why Newton is superior to Hooke. This is why it is more rational to take as unit of appraisal a whole *research programme* rather than the simple idea formulated in the hard core. Or in the case of Einstein, a monkey, with relatively high probability, can type out the sentence: *Time and space are somehow interrelated*. But

it is *much less* probable that the monkey should type out the entire special, let alone the general relativity theory.

HIEBERT: It seems to me that yesterday Elkana gave us a start to a programme of examining the history of science by means of the philosophy of science—in this instance using Lakatos' particular version of the latter—and that Lakatos rather smiled and implied "Yes, we shall meet somewhere." Then Everett Mendelsohn embraced Lakatos and said, "Let's work together." But it seems to me that today Mendelsohn has practically ruined Elkana's abolition of the Lakatos internal-external dichotomy. And the reason I say that is because it seems to me that Mendelsohn himself has used the term "methodology" in much too loose a fashion. Lakatos uses methodology not as something that has to be employed in constructing theories, but as an appraisal of results, whether they are typed out by a monkey or by Einstein. This meaning of methodology is rather specific.

It seems to me now that what Everett is saying leaves us with no real quarrel at all. Everett would have no real quarrel with Lakatos because he touches primarily on those matters which Lakatos would label as external. So it seems to me that one is going to have to be more explicit about the meaning of "methodology," otherwise there can be no cross-verbalization between the history of science of Mendelsohn's type and the philosophy of science of Lakatos' type.

I am saying really that Lakatos has put his case on the line concerning what he means by methodology, while you Everett effectively do not. This permits you to smuggle in all kinds of things without stipulating what you mean by "methodology," so that we are left with a very poor tool to analyze the situation.

MENDELSON: It might be just as well to get right back at this question. It is quite all right if you want to limit the concept "methodology" in 1970 to a very narrow domain. But this certainly is not the only way the word has been used historically—it has been used very differently in different periods in time and has generally been taken to refer to a much wider series of activities than today's methodologists imply.

Indeed, this 1970 logician's methodology has a very limited amount to do with what people do when they do science. When they do science, they do a large number of things—they walk into laboratories, they order equipment, they sit down at their tables, they teach, they preach. There

is a whole series of activities which a scientist goes through, which in various times people have attempted to bring together and loosely called one aspect or another of the given scientist's methodology. I clearly used the word in that way. I made no implication that I was using it in the very restricted modern logician's meaning and I think the distinction is quite clear.

The point that I am making is that to understand the approach that a scientist takes—what he does—we have got to go further than the narrow meaning. I am willing, if people want, to accept for 1970 purposes that the only use of "methodology" will be the restricted one. But if we adopt this then we shall immediately have to go and rewrite history backwards, substituting another word for "methodology" than that used in the 19th century.

TOULMIN: This debate is clearly going to begin by discussing what we mean by the term "methodology." If we are going to discuss this, we must not be faced with only two extreme alternatives. There are many views about what "method" is, even at the present time.

ELKANA: I think that what Everett said in various places is very much in line with what I believe, but I think still Erwin Hiebert's criticism is justified, insofar that if we want to give a broader meaning to "methodology" we have to make it much more specific by showing as exactly as we can what things go into it. But the examples which Everett brought did not take us far in this direction—that is I think what Erwin meant, and I think he is right. It is an important task, because if we are going to take a scientist's work and eliminate from it everything except what could equally well be done by the monkey (that is: the text of the finished product)—what remains is not really interesting. This approach has been developed very nicely in modern logic and is extremely interesting when appraising the logical consistency of theories. But those things in which we are really interested nowadays—what was happening and how was science being done—are not taken into account at all. So the narrower concept of methodology certainly has to be somewhat broadened.

Our most important historical task is still that of finding the connection between what goes into a piece of scientific work and what comes out as a finished product. The moment we don't have to take into account in the output what the input was, the way we try to understand how science is being done stops being interesting. It is in this sense that

it is important whether a paper was written by Einstein or a monkey. In other words, what we can do is to try to rationalize scientific creativity. This will not give us a complete account of how the creation was made, of the act of creation, but we can collect quite a wide range of not sufficient but necessary conditions. We can ask what things have to have happened rationally in the intellectual make-up of the scientist for the creation to have taken place. This then becomes very important for understanding the final product, and this you would lose the moment you claimed that it is not important who or what wrote it.

LAKATOS: May I ask you, is it important for judging a paper whether it was written by a minor scientist or by a genius? And if it is not important, why should we exclude a monkey or a computer?

ELKANA: This is a good counter-example for there is a qualitative difference. If you also allow a monkey or a computer, it means that by definition you cannot take into account any rational considerations of the input, while in the case of the minor scientist and the genius these considerations are relevant and should be taken into account—and that is the difference.

ROSENFELD: I take it you are discussing methodology *sub specie aeternitatis*—I think that is a dangerous path to take. There is no physics *sub specie aeternitatis*—the physics of the 17th century, of the 18th century and after it, those are quite different things, for good reasons, and the same is true of methods. The methods of the 17th or the 19th century could not be the same as ours and vice versa. If by some operation we could meet those 19th century giants and explain to them our methods, they would look very curiously at us and it would take them some effort to learn all that we learnt in the meantime, to understand what we are about.

ELKANA: If I could specify your comment more—to which of the at least three of four opinions already expressed or implied here do you think it is most relevant?

ROSENFELD: I think Lakatos is the most extreme—he will even extend methodology to monkeys which I think is a physiological impossibility. Science is attached to mankind because no brain except the human brain is a strong enough computer to make science—a monkey can't.

I think any scientific problem can only be really analyzed and understood methodologically within the context of the time, which means

against the background of the then existing knowledge. All we do in our time, when we discuss unsolved problems, is just to try to find arguments from what we know that may lead us in some direction. We don't know beforehand whether it will be the right one or not, but at least the only thing that our existing knowledge can give us is some guidance towards it. But formal logic can lead us nowhere—of that I am absolutely certain.

LAKATOS: I am a bit puzzled by the lack of understanding of what I said—of course I did not say that in order to understand how science works, we need to understand the psychology of Albert Einstein. This is a different matter. What I claim is simply that if an editor receives a paper which has the text of Einstein's 1905 paper, then for that editor it has absolutely no relevance who wrote the paper and even whether it was a human being, a divine being or a machine. But I never denied that in order to decide whether a paper is a genuine achievement or not, one has to know, say, whether the same paper was not copied out from the same periodical of a year ago. Of course one has to know the extant state of objective knowledge in order to appraise a new paper. But I am afraid that Rosenfeld's position is a sort of epistemological relativism, namely, that Newtonian standards are different from Einsteinian standards and there is no objective method to decide which are better, which are worse.

MENDELSON: It may help to quote an example coming from the period that I talked about. In 1847 Helmholtz submitted his paper to Poggendorff's *Annalen*. Poggendorff rejected it, in all probability because of the framework within which it was cast. In other words, it was not the specific formulas which were being responded to—Helmholtz speaks by the way in his letters about why he thinks it was rejected and I am conjecturing along with Helmholtz on this—but the fact that it was a species of argument which physicists were very much afraid of, because it carried with it older views. The point I make is this—that a paper, in a sense, does carry with it something of the intellectual, contextual background out of which it was produced and in this case, very clearly, this element was responded to by an editor. Helmholtz indeed thought that it was his method that was being rejected.

ROSENFELD: I might also illustrate what I have in mind by a concrete case. Take Maxwell's theory of electromagnetism. Compare Maxwell's papers in which he describes his theory with the account of the same

theory (what we *call* the same theory, in a sense to be analyzed), in modern textbooks—in Föppl's textbook and then in Abraham's and finally in Becker's and Sauter's, who made the last rewriting of this textbook. Now why did Becker or the contemporaries of Becker feel the need for rewriting this textbook?

It is the same physical theory that is dealt with in every edition of the book—it describes the same physical phenomena. The physical phenomena have not changed and this theory gives an objective description of them, in the sense that this description can be understood by all physicists who learn the language in which it is written; and if these physicists repeat experiments or draw consequences from the theory and test them, the theory will still be shown to be absolutely correct—because it is a good theory.

The answer to my question is that successive editors felt obliged to diverge from the method followed by Maxwell in order to get to these results which are eternal in their objectivity. (There is no relativity here: I mean that applied to coils and so on, Maxwell's results are eternal truths.) The method followed by Maxwell was radically different from the method which is now followed by every teacher in order to present those phenomena, and the change in method is due to what has happened in between—in the way of looking at those phenomena. We have found a better way of looking at the same phenomena, or a better code, if you like, in which to codify our knowledge of them.

So you see there is a dialectic here—there is a constant element which is given by nature and there is the evolution of our approach to nature which is built up all the time by increasing knowledge and increasing certainty.

HOLTON: Perhaps I might make some remarks stimulated by Agassi's paper.

It seems to me that there are two things that it would be worst to do about this complex and puzzling business of the methods scientists employ—one of them is not to work on it seriously, and the other one is to confuse the discussion with meaningless and puzzling things which only derail the attention. I am glad that Agassi attended to the first and avoided the second, and it seems to me one must take this point of view—that in fact we do not know enough of what is going on among these men that seem to some to be mere "sleepwalkers". It may merely be that

while they know very well what they are doing we don't have the language for it. Einstein often points to this possibility by saying that there comes the moment when he has to leap across a gulf from his knowledge of the "facts" of science to the theory, guided by an intuitive feeling for where safe ground lies; and he adds apologetically that he cannot put flesh on this meagre description, partly because he is not good in discussing such matters, as he always insists, but surely also because the state of this branch of scholarship, the study of the genius and development of ideas in the scientist's mind, has not advanced very much.

We must allow for the possibility that all the great discoveries in this particular field are still ahead of us, that we know extraordinarily little, and that what we know may be false. Comparing one man of genius with another, for example, we may overlook the possibility that completely different processes could be going on in the two different cases. Also, there may be a large gap between the genius and the very good scientist; they may have different internal equipment to work with. One may have to think of a large spectrum of scientists, and therefore a large spectrum of questions and answers. I am basically an experimental physicist, so I think of this field as an experimental science, and one in which little progress has yet been made.

When it comes to the remark about the editor not really caring, or perhaps not having to care who wrote the paper, I think that is one of those ideas of a derailing kind. You can certainly argue about it, but I have a feeling you won't get nearer to anything useful. In the particular case of the 1905 paper, in fact, the editor of the *Annalen der Physik* was Planck. He had a new assistant—von Laue, and it seems one of his first tasks was to deal with the 1905 relativity paper from Einstein. He was so excited about Einstein's work that he made arrangements to take a train to visit this man in Bern. Von Laue found him in his shirt sleeves in the patent office. Einstein later remarked. "That was the first time I saw a physicist!"

What counts—and that also is a puzzling thing—is this: Planck and von Laue sensed at once that a glorious, extraordinary paper had arrived at the editorial office, and that there is a humanity and a genius behind the written lines that is simply beyond question. So I say it is possible to argue whether they should have cared one way or the other, but I think it gets us nowhere.

ELKANA: I naturally accept from Prof. Rosenfeld that all these things are time-dependent—that goes without saying; but it has been the great achievement of methodologists that we learnt the difference between the editor and the philosopher—i.e. we stopped just giving marks: good, bad, not good enough, etc.

For this I think the Poggendorff example is very nice. As long as we just want to discuss as a historical curiosity why was Helmholtz's paper rejected, or the similarly interesting question of why was Einstein's accepted—for it should have been rejected, we may accept the Lakatos attitude that this is a typical "external" problem. But I think we can get further, and this is where I take Lakatos' methodology much more seriously than he does himself and try to use it, as far as I can, in the history of science. It is not merely a curious "external" question why Poggendorff rejected the paper—I think that Helmholtz himself knew exactly what he was doing methodologically, he knew he was breaking the rules and introducing a shift. As a rational act he was trying to legitimize a new way of doing science. That is, he was choosing a deliberately different kind of thing from the accepted standard and this had to do with the changing image of science. So this kind of issue too has to be dealt with in our historical methodology.

With regard to Gerry Holton's remarks I would just like to comment that we can understand more of these things than he is ready to allow. This still leaves enough room for that which we will never understand in a genius. I mean geniuses will continue to do physics and not philosophy of science.

TOULMIN: I think Gerry's example had an added virtue. It is clear that if, when von Laue arrived at the Swiss patent office, he had in *fact* found a monkey with a typewriter, his recommendation to Planck would have been different! For a monkey with a typewriter would have not arrived at Einstein's result in anything that can be described as a "methodological" manner—by the application of a "method"—whereas, in the sense in which Everett was talking about "method," this has something to do with the rational procedures employed in arriving at results, as well as the logical arguments used to explicate results previously arrived at.

HOLTON: I just want to say that you all seem very derogatory about monkeys, perhaps because we have little idea how or what monkeys think.

LORCH: About the monkeys, in 1904 Häckel wrote that one should try crossing monkeys with humans in order to see what will come out. This being 1905, there might have been someone trying to suggest that this in fact had been done.

TOULMIN: One brief addition to my remarks on Lakatos: this kind of discussion is liable to be bedevilled by his and Popper's use of the word "psychology." When we talk about how scientists arrive at results, this is not just a matter to be discussed under the heading of "individual psychology." But for Lakatos it is simply to be dismissed in this way, and so he claims we are not interested in the question of the personal aspects of how Einstein arrived at this result, this not being relevant.

LAKATOS: We are not interested in the question from the special methodological point of view!

TOULMIN: What I want to say, by contrast, is that there *are* considerations which are not considerations of logic, but which have to do with the manner in which scientists arrive at their results and which—if they are part of psychology at all—are the concern only of "collective cognitive psychology." Now to my mind the discussion of rational procedures overlaps the area of collective cognitive psychology; and once this is admitted, one cannot any longer insist on the Tarski/Carnap/Popper/Lakatos distinction between "logic" on the one hand and the individual idiosyncrasies of scientists on the other hand.

HIEBERT: I am afraid that my earlier comments may have been taken as a defense of Lakatos' particular position in methodology—quite to the contrary. I would like to state my position more clearly now vis-à-vis his position.

As you by now know, I don't believe that the history of science is worth doing in *vacuo*—I just don't have any interest for setting a record straight or something of that kind. So the kind of history of science it seems to me worth doing is the kind in which you do ask interesting philosophical questions, interesting sociological questions, interesting political questions, interesting aesthetic questions.

Now the only reason I thought we ought to embrace Lakatos is not because of a particular programme that he presents, but because of his intent—he is willing to communicate, he is willing to cooperate. I think, however, that this particular scheme is much too grandiose an idea. So the value of carrying out his particular programme is not all that im-

portant for us. If I may put it in a sort of belittling way—he is as it were simply a laboratory assistant of ours. We have to call in sociologists and aestheticians and people in political science and so forth, because it seems to me that the most interesting question that the historian of science can raise is: when a scientist says he is doing science—what is he doing? That is the really interesting question. And of course part of shedding light upon that question has to do with the appraisal of the results.

EZRAHI: Prof. Hiebert—is it enough to ask what a scientist is doing when he says he is doing science? If we do not look at the scientist as an individual, but rather look at science as a cooperative enterprise, then I think the ways in which scientists perceive, and describe what they are doing, must have significant influence upon the ways in which other scientists interpret or evaluate the results of what they were really doing. If you look at science as a social activity, which requires interaction among scientists, then for example if the interpretation given by one scientist of what he was really doing—be it wrong or not—delayed the perception by other scientists of some conclusions that could be drawn from the results that he arrived at, this is extremely significant for the development of science.

TOULMIN: In a sense what this shows is that the study of methodology is important even at the level of posing the questions to which the sociologist will then attend. Because the sociologist has previously to make a normative judgment, in deciding that a particular piece of scientific behavior was *in fact* "holding back" an intellectual transition, which ought to have been made at a particular time.

ROSENFELD: There does indeed exist this social compulsion—not of society at large, but of the society of scientists—on the way in which scientific work is carried on. Poggendorf's resistance to the new tendencies in physics is an example of that: he was very strongly under the spell of the reigning ideology, rather narrow-minded, and that was the reason for his rejection of Helmholtz's paper—for not even understanding the point of the paper.

But the new direction that Mendelsohn described so well, and that he called reductionist—not a nice word, perhaps, but one which nevertheless describes a profound reality—very rapidly became itself exclusive and one can cite victims of it, one of them being Robert Mayer, who had what we now regard in retrospect as a much more profound view of

the situation. Even in physics, he insisted that the transformation of thermal energy into mechanical energy was not at all identical with the statement that heat was a mode of motion: he had only established a law of transformation from the one to the other. This was a more profound standpoint for instance than Joule's, who thought that he had proved the kinetic theory. Mayer was victimized, in particular by Helmholtz, who only belatedly and grudgingly acknowledged Mayer's priority. There was an incident at a congress at which Mayer gave a talk—a rather weak one, I must say; anyhow, he was outrageously lampooned for it, with the result that he had a relapse into his mental sickness. So this was a case of rather violent compulsion going out from the circle of the scientists following the accepted standards.

Another case was Schwann: he was a victim of himself, so to speak, in the sense that there was a conflict within him, between his religious belief and his materialistic views in physiology; the religious got the upperhand with the result that he gave up scientific research for the rest of his life. So there is here a very complicated interaction between the inner community of the scientist and the more extended social conflicts in the society to which he belongs.

LORCH: Schwann at the age of 28 left science forever—at the age of 28 after a terrible nervous breakdown. While Schleiden, after twice attempting to take his own life, turned from law to botany. Anyway, Schleiden went into botany and his immense success and the veritable impact he had on biology is not without interest, because his botany and all his great conclusions were entirely wrong—which did not prevent him or his pupils from getting first prizes from academies just because the drawings were so beautiful!

Schleiden's impact and his confidence were due to his preoccupation with philosophy and he writes that he had learnt from Kant and Kant's pupils more botany than from all botanists put together—this is also an indication of how versed he was in botany.

MENDELSON: But what distinguishes him is a specific concern with the method of botany. This is why he says he is learning botany only from Kant—he is going to great lengths to indicate the definition of a new methodology.

LORCH: But his contribution was wrong. He says that the cell is important, that reproduction is important—but whatever he says about the

cell, and he says it with the same confidence as that with which he prescribes the method by which things ought to be done, is entirely wrong. So how could he have made such an impact on all his contemporary biologists—because of his preoccupation with philosophy?

This I think is something that has not been enough pointed out—the enormous preoccupation of people at that time with philosophy, the amount of time they spent—at least in Germany—listening to philosophy lectures and reading philosophy.

LOWE: It seems to me that the view of Professor Lakatos concerning the nature of methodology is immune to some of the attacks that have been made upon it here, provided that one realizes that a certain kind of thesis lies behind it. Moreover, the example of the monkey with the typewriter is meant to give support precisely to this thesis, although Prof. Lakatos did not explicitly state this.

When Prof. Lakatos asserts that methodology deals only with ready-made products, the results of a scientist's work, he appears to be making a paradoxical assertion, because traditionally, as previous contributors to the present discussion have alluded, methodology concerns not merely the ways in which a scientist appraises his results, but also the process by which he proceeds.

Now, it may be that a scientist uses methodological criteria in the process of arriving at his results, and yet, nonetheless, the rational aspects of the process by which he arrives at his results are not significantly distinct from the criteria which he uses to appraise his results. This would be so if, in fact, the rational aspects of the process of discovery consisted simply in the scientist's directing his activity in such a way as to produce results which will satisfy certain kinds of criteria. If this is what the rational aspects of the process of discovery are, then a statement of the criteria by which results are to be appraised *implies automatically* a statement of the rational aspects of the process of discovery. This is the unstated thesis behind the view of Lakatos, and the example of the monkey with the typewriter is intended to support just this thesis and has to be evaluated in this light—namely, any objections to the typewriter example are irrelevant if they fail to weaken its support for the thesis that there is no significant difference between rationality of the process of discovery and rationality of its results.

TOULMIN: I think this is really directed at my comments on Lakatos. I

think the case you are making out would be a very strong case, if it were still the accepted convention for scientific papers to be written in an autobiographical manner, about what you in fact did and how you in fact got to where you did. Then indeed you could show the "reasonableness" and/or "rationality" of procedures by which you had arrived at this result in the course of reporting the experiments and the considerations.

LOWE: My point is that this is just what you don't need to have in it, according to Lakatos, because inasmuch as you did anything rational, this should be visible in a presentation of your argument and experimental results that is independent of your personal history. And so Lakatos uses "methodology" in a sense which at first sounds strange, because of what lies behind it: the thesis, that the rational aspects of the process of discovery consist in directing discovery towards a result which can be appraised in a certain manner.

TOULMIN: If this were the case, because in a certain sense the question is whether it is the case.

LOWE: The typewriter example is supposed to establish that this is the case. Moreover, the behavior of von Laue if anything bears Lakatos out—von Laue rushed to see Einstein not in order to check that a human being had written the paper, but because from *just reading* the paper he realized that it was a great contribution to physics.

TOULMIN: I think we have had a very useful discussion on this topic arising from Mendelsohn's paper, and this last point is a helpful way to clarify the issues. Perhaps there are some more comments directed towards Agassi?

ELKANA: If I view Agassi's lecture with normative eyes—then I see two points that raise a question. The first is: can you imagine arriving even at partial algorithms without attempting a full algorithm? This is a psychological question. The second is: Let us say that your description of partial algorithms is accepted, what do you think can one learn from it normatively qua historian of science and qua science teacher?

AGASSI: In order to deal with these points, let me start with something that Hiebert was saying. He said we should call on the logician as an expert, should call on Lakatos as an expert and so on. I think that is quite all right. There was a debate yesterday about whether Kepler's theory and Newton's theory contradict each other. This is a strict logical

question if the word "contradict" is understood in the logical sense, and the answer is, yes, Newton's theory contradicts Kepler's. Now, if the word "contradicts" is understood differently then it may need bringing in other experts. For my part, I have found one who gave a different meaning of "contradict" according to which Kepler and Newton do not contradict each other.

Karl Popper in his *Logic of Scientific Discovery* spoke of appraisals—we have a theory, we can appraise its status—is it testable? Does it explain facts? Is the test that is being performed a risk, i.e. can't it turn out to be a falsification of the theory, in case it is false? The result of the test, does it accord with the theory or not? All these are questions of appraisal and at the end of the appraisal you can say: "An experimental result as recorded in book such and such, contradicts the theory and thereby refutes it, or it does not and therefore it corroborates it." These are all appraisals and here we have what I would call the narrow definition of methodology, which I think is perfectly all right—it is not the one that I am much interested in these days, but it is perfectly all right.

There is also a broader definition of methodology, which we all were concerned with today except for Lakatos. The source of his dissent is a hypothesis lurking in his talk that I want to state explicitly because I think it is false and dangerous. It is the hypothesis that methodology in the broad sense is the methodology in the narrow sense plus some old-wives' tales, plus hot air plus some moralizing and so on. Now I know that there is literature which conforms well to this description, I know quite a few books on the history of science that are just confirmations of the hypothesis that methodology in the broad sense is just methodology in the narrow sense plus. But this is not the kind of history of science that we are interested in.

If we accept that there is methodology in the broad sense rather than the narrow sense or on top of the narrow sense, then we can for instance discuss the questions relating to partial algorithms, which brings me to Yehuda's first question.

Partial algorithms have to be understood in two ways. First, they are partial in the sense that they don't cover the whole field in which they are supposed to apply but only parts of the field. Hence, they are not reliable. Secondly, they are not as strict as to be formalized. Hence, they are not too clear. I don't mean only formalized for a computer

but even axiomatized—partial algorithms are not sufficiently clear. If you take for instance chemistry, say the radical theory, people who invented the radical theory knew that it didn't apply everywhere—benzene was discovered in 1824, and the radical theory which was invented in the eighteen fifties or thereabouts did not apply to it. So they knew it was not enough to cover everything, but they tried to cover as many chemical reactions as they could with the help of the radical theory.

You ask now the question: can you imagine arriving even at a partial algorithm without the idea of a full one—without aiming at a full one? Certainly. Let me give you a straightforward historical example—John Dalton. One of the reasons he is so misunderstood by historians is that he published the first partial algorithm in chemistry in which he said, "try to assume as few atoms of a given element as you can in a given compound, and if it fails try to increase the number of atoms of one element, and so on"—he gave a detailed prescription though he was not interested in full algorithms.

I can even imagine people arriving at partial algorithms and being prodded to extend the techniques further and who refuse. Social anthropology is one example. Social anthropologists were repeatedly advised to broaden their techniques to cover literate societies—not only primitive societies—and some (though others resisted) even did it. It is a fact that some people, against the intentions of Malinowsky, applied his partial algorithms to, say, London slums.

The second question that Yehuda asked is what is the corollary for historians of science from the existence of partial algorithms. Now I would say it is a question of appraisal. In my opinion, historians of science appraise all the time; they can do it crudely, by giving medals and bad marks to past scientists, or they can do it nicely. Koyré said already that every historian is a bit of a hagiographer and I think this is true. The very fact that you write three volumes about Galileo, shows that you don't think that Galileo is any Tom, Dick or Harry.

When we appraise, we can appraise things in many different ways. We can say, for instance, like one historian I have read, that Euler solved certain integration problems with the most incredible substitutions that would not have occurred to any one around who only utilized existing partial algorithms for solving problems in integration. Some people have criticized this historian, saying that his facts are wrong, so perhaps he

was making a valid appraisal on a mistaken statement of fact in this example. If we say: John Herschel went to South Africa and prepared a catalogue of stars as big and rich as he could and we are grateful to him for his patience—not for his genius—this is also an appraisal. Here is, strictly speaking, a partial algorithm, a research programme, a very dull research programme which requires little genius and Herschel performed it as a labor of love. We know that Herschel was a genius, a small genius perhaps but still a genius, and he went to do this work because he felt that science involves a lot of tedious work—he wrote this in his *Preliminary Discourse to the Study of Natural Philosophy*. He believed that every man could make a simple, honest grey contribution to science and he made his grey contribution by going south and compiling the catalogue.

Now the very fact that he compiled the catalogue draws our attention, because we appraise it in certain ways. We say, there is not much genius in it, but there was patience in it; we say, it did or did not push astronomy further. But when we take Young's *Dictionary of Medicine*—we say this is a very particular thing—this is not just compiling all medicine that was known, but here is a man undertaking a certain task for a very specific reason. He is in fact the first to do anything like it, which had a certain flavor of what we call the external history of science. So we do appraisal all the time as historians and just like patent testers, we can say—or should be able to say—how ingenious the patent is. We could be wrong about it, like patent testers of course.

TOULMIN: Could I again throw in a distinction to try to prevent cross purposes—it does seem to me that there is a danger in your use of the phrase "partial algorithms" which suggests that there are criteria of completeness which can themselves be stated beforehand. Now what is very important is that the criteria of completeness are themselves to some extent at the mercy of history.

In the 19th century for instance, people never really asked why the sodium "D" lines should have the color they had, and be in the spectrum where they are—this was a question that did not arise within physical science as it was then conceived. That any particular element had its characteristic spectral lines was a recognized fact. But the idea of producing an algorithm from which you could derive these frequencies was not part of current research—was not allowed for in the current research programme.

Perhaps what I am saying is that if you speak of partial algorithms, you have to avoid making anachronistic judgments. You have to decide what is partial, what is complete, once again with an eye to what the legitimate horizons of understanding were at that stage. And those horizons are themselves an element in the history of the science.

AGASSI: I would agree with those who say you can apply hindsight when you search for your own problem as an historian. For example, you can use hindsight and say we know that a certain research programme is very limited in application; this may lead you to a historical problem: how did the ones who advocated it look at it in their own period? Whatever research programme we have, we find its limitation when we exhaust it sufficiently to our taste. And things do change. If you take the sodium "D" lines, the problem that Balfour Stewart and Kirchhoff raised is what can we learn from absorption about emission. Because they received from the sun only absorption spectra and they had to translate it into emission spectra before they could do solar chemistry. This problem was never exhausted because Einstein's paper on emission and absorption, though an attempt to go back to Kirchhoff's law, somehow fizzled out because it led to another research programme which was much more interesting and which related to emission mechanisms and absorption mechanisms—I mean that of Niels Bohr.

TOULMIN: I personally wish to hear the reactions of Shmuel Sambursky to all of this.

SAMBURSKY: I believe that methodology cannot be formalized to comprise the whole of scientific discovery. Referring to Agassi's account on "sleepwalkers" and to several occasions when the role of the genius was mentioned, I want to come back to what Schelling said in 1799. He defined the genius and if you have not read the last chapter of his *System des transzendentalen Idealismus*—I recommend to you to read this chapter, because he anticipates this idea of sleepwalkers in a very interesting and much better way.

He says that creative activity at its peak, on its highest level, is identical in science and in art—that is, outside scientific method. Further, we have to rely upon the luck that geniuses outwardly have—we don't know how—and that a genius is actually only understood by hindsight, by prophecy backwards so to say. What he says also applies to the case we were discussing—namely Einstein, because of course Einstein is the foremost paradigm of a genius.

Schelling says that a genius actually unconsciously discovers an infinity of possibilities and aspects, whereas consciously he limits himself to a certain partial aspect and he is restricted to the ideas and methods of his time. His own example was of course not Einstein (though it applies to him too), but the genius of the Greek people. And he was not thinking of their contribution to science, he did not know much about that, but rather thinking of Greek mythology as the product of the genius of a people, because now we see the infinite aspects of this mythology, whereas the Greeks were restricted to their own time and to their own method and to their own concepts.

I think it is important to know that there are limits—every systematization has its limits—and it is interesting and I think significant that at the highest level of creativity, science and art are absolutely equal. Beethoven was restricted to his technique in creating his last quartets, Van Gogh was restricted to his technique in painting, Kafka was restricted in writing. But what came out was really unconsciously an infinity of aspects which last forever—just like what Rosenfeld said about Maxwell.

THE CHARISMA OF CRYSTALS IN BIOLOGY

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“Dass ich erkenne was die Welt im
Innersten zusammenhält”

J. W. Goethe

Schriften zur Botanik, p. 227

Simple models have always had a pervasive and persuasive magic. Their effect on modes of looking at things and interpreting them is perhaps nowhere more strikingly demonstrated than in the following story, which attempts to trace the multivarious ramifications in biology of discoveries and theories in crystallography.

Those intangible qualities of various substances which determine their form must have puzzled our first ancestor when he watched salt crystallizing from brine, or plants growing from their seeds.

Lucretius,¹ contemplating the curious fact that each kind of tree regularly produces identical fruits, concluded that everything that grows, grows from a specific seed by virtue of a *secreta facultas*, an occult faculty, so that the business of nature becomes little more than that of a farmer who superintends the growth of his crops or of a shepherd tending his flock. Small wonder that when, in due course, final causes began to fall into ill repute, the *natura genetrix* of some of the Stoics—the *spermatic rules* or *spermatic powers*—provided a convenient starting point for presumed causal explanations of organization and form.

Sir Th. Browne pokes fun at “the common opinion that Crystal is nothing else but Ice or Snow concreted, and by duration of time, congealed beyond liquation.”² “Few opinions,” he adds, “have found so many friends, or been so popularly received, through all Professions and Ages.”

¹Lucretius, *De Rerum Naturae*, Bk. I, 11. 167, 220–224.

²Th. Browne, *Pseudodoxia Epidemica*, 1646, in G. Keynes ed., *The Works of Sir Th. Browne*, London, 1928, vol. 2, p. 87.

By elaborate *a priori* reasoning Browne reinstates the "Mineral spirit," the "lapidifical principle." The example of Alum, Salt-petre or Tartar affords him ready proof that congelation is not primarily due to cold, "but an intrinsical induration from themselves, and a retreat into their proper solidities which were absorbed by the liquor and lost in a full imbibition thereof."³

All this, in contrast to ice which to him "is only water congealed by the frigidity of the air, whereby it acquireth no new form." Its condition (of fluidity) is thereby lost, but its essence is not changed. In this context, Browne mentions Aristotle's test of the fertility of human seed by an experiment on its congelation.

Beyond the clumsy language, there is always the same vague intuition that seminal principles are severally responsible for the shapes of crystals, as well as of all other bodies.

Maupertuis seems to have been the first writer explicitly to draw a parallel between processes of development in living beings and non-living substances. He compared the formation of the foetus with the growth of Arbor Diana, formed by the mixture of silver and spirits of nitre with mercury and water.⁴ Maupertuis proceeds from the cubic grain of sea salt, consisting of an infinite number of other cubes, to plants and other organisational bodies, composed of other exactly similar organisational bodies whose constituent parts, by reason and analogy, are likewise organisational. According to him there exists in nature an infinity of organisational living parts, of the same substance with the bodies which they constitute. For, as perhaps thousands of thousands of small saline cubes are accumulated, to form one tangible crystal of salt, so thousands of thousands of organisational parts, exactly similar to the whole, are required to form a single bud of an elm. In drawing such parallels, Maupertuis was merely presenting another aspect of his views on the necessity of the laws of nature: "But one could say that although the laws of rest and movement were until now demonstrated by hypotheses and experiments, they are perhaps the necessary consequences of the nature of substances . . . all things being ordained in such manner, that a blind and necessary

³Ibid., p. 88.

⁴P. L. M. de Maupertuis, *Venus Physique*, La Haye, 1746, 5th ed. 1748. A study by L. Bourget, *Lettres philosophiques sur la génération des plantes et des animaux*, Amsterdam, 1729, was not available to me.

Mathematics executes that which the most enlightened and free intelligence prescribes."⁵

Tonelli has pointed out that reacting to the reception of such ideas, Maupertuis remarked already in 1751 in his *Essai de Cosmologie* that this doctrine was pure hypothesis and not acceptable to him: "For it is not in Mechanics that I shall search for these laws, but in the wisdom of the Supreme Being."⁶

An early critic of Maupertuis is Alston, who takes umbrage at Maupertuis' tedious and intricate detail of words, an attitude which must have been—and is to this day—quite prevalent among English readers struggling with French philosophical texts. But beyond this, he finds it difficult to tell what is meant by these organisational parts, "because they are all, either always of the same determinate figure, or constitute equally well an elm, or a cabbage, and then they differ in no respect from the atoms of the Epicureans." Alston then castigates the supposed incorruptibility of these organisational parts and their living nature. For are not the parts of a single bud "disposed in one, certain and definite order, of the infinite possible orders . . . to effect which, evidently another energy and design, more than human reason, is requisite, which the author I scarce expect will find in his organisational parts, though he should feign that they both feel and think."⁷

Yet Alston's criticism, a vague groping for a "Gestalt" principle in each plant, failed to counteract the excitement about crystals as models for the study of organic phenomena.

When trying to evaluate such analogies as proposed by Maupertuis, one must of course keep in mind the attitude prevailing in those days, that the different "kingdoms" do not have clearcut limits. Linnaeus, in his slowly changing views, and Wallerius, a contemporary student of petrifactions, provided ample evidence for such attitudes. Thus Linnaeus: "Natura non facit saltum";⁸ and Wallerius: "However, it also should be noted in this connection that Nature in her division and placing of the

⁵P. L. M. de Maupertuis, *Les lois du mouvement et du repos déduites d'un principe métaphysique*, Histoire de l'Acad. Sc. et Belles Lettres, Berlin, 1746 (1748), quoted after G. Tonelli, "La nécessité des lois de la nature au XVIII siècle et chez Kant en 1762," Centre International de Synthèse, 12 (1959), 225–241.

⁶P. L. M. de Maupertuis, *Oeuvres*, Lyon, 1756, vol. 1, p. 35.

⁷Ch. Alston, *A Dissertation on Botany*, London, 1754, 136 pp. (p. 29).

⁸C. Linnaeus, *Philosophia botanica*, Stockholm, 1751.

borderlines of her realm as elsewhere, does not leave any gaps: but there are bodies which, as it were, are ambiguous between two of these realms of Nature, and are like means or something between, by means of which Nature gradually rises to its height from the one realm to the other. Thus it is found that the Natural mineral Liquids in a certain way belong to the Water kingdom, and will here below be included in our Water kingdom; but in a certain way they also belong to the Mineral kingdom; *Lithophyta* or Stone plants are dealt with in both the Vegetable and the Mineral kingdoms; *Zoophyta* or Plant animals are not widely different from other plants.”⁹

Also, any attempt to discover the implications of the study of crystals for the study of plants must be read against the straggling development of microscopy and anatomy. Thus, about a century after Malpighi's and Grew's pioneer studies of plant anatomy, Gleichen could not report much progress. Yet he concludes that “in the formation of a vegetable embryo and a crystal, identical forces are involved and both processes proceed according to one and the same law of nature.”¹⁰ Vicq d'Azyr has expressed a similar view in his study of new growth in stems and crystals.¹¹

The grandeur of the subject of crystals was first exhaustively presented by René-Just Haüy. Born February 22, 1743, he was in 1783 appointed as adjoint of botany at the Académie des Sciences, replacing A. L. de Jussieu, who had just been promoted.

Cuvier, in his necrologue, points out “the extraordinary care and neatness with which Haüy prepared his herbarium, which also provided him with a first introduction to the employment of method.”¹² Thus, when at a later stage the system of minerals inspired views on the classification of plants, these may well have been inspired by a methodology

⁹L. Wallerius, *Förberedelse . . .*, quoted after G. Regnell, “In the position of palaeontology and historical geology in Sweden before 1800,” *Arch. Min. Geol.*, 1 (1), 1–59 (p. 47). See also J. Lorch, “The history of theories on the nature of corals,” *Vie et Milieu*, Supplement 19 (1965), 337–345.

¹⁰W. F. Freyherr von Gleichen, genannt Russworm, *Auserlesene mikroskopische Entdeckungen bei den Pflanzen, Blumen und Blüthen*, Nürnberg, 1777, p. 28. See also J. Hedwig, *Sammlung seiner zerstreuten Abhandlungen und Beobachtungen*, vol. I, Leipzig, 1793.

¹¹Vicq d'Azyr, *Oeuvres* (6 vols. + atlas), Paris an XIII (1805), vol. 4, p. 232.

¹²Baron Cuvier, “Eloge historique,” *Mem. de l'Institut Physique et Mathématiques*, 8 (1829), 144–178 (p. 149).

initially acquired in the classification of plants and projected unto the world of minerals.¹³

In 1785 Haüy was employed as adjoint in mineralogy and in 1792 he became adjoint to the perpetual secretary of the Académie des Sciences, Condorcet. In the same year he was appointed to the Academy to replace Fourcroy.¹⁴

Haüy's first account of the science of crystal dates from 1784,¹⁵ but his great impact is due to his 4-volume study published in 1801.¹⁶

Haüy's ideas and preconception have been succinctly outlined by Goodman. Initially, Haüy seems to have been attracted by the simplicity of the relationships between linear dimensions and angles in crystals (the modern “law of rational indices”). He believed that the regular solids found in some crystals were the archetypes of all. Since the dimensions of the former involved ratios of square roots of small whole numbers, this property of the models was to be looked for in the irregular crystals.¹⁷ Haüy is taken by “This idea, so satisfying and so true, that in general nature tends to uniformity and simplicity. The ratios between the dimensions of the limiting forms possess this last property in a remarkable way.”¹⁸

Fortunately for Haüy, his measurements were imprecise enough to leave him scope for adjustments, and faith. But his Keplerian trust in simplicity was such that even improved instruments and more accurate, incompatible data, such as those obtained by Wollaston, failed to move him. The same obstinacy, indeed, we will come up against more than once in the sequel.

In the first quarter of the 19th century, Haüy's studies appear to have attracted considerable attention, even outside scientific circles,

¹³J. Lorch, “The natural system in biology,” *Phil. Sc.*, 28 (1961), 282–295; Note especially J. S. Mill's views on “distinction of kinds.”

¹⁴Cf. Commemorative Volume, *Bull. Soc. Fr. de Mineral.*, 67 (1944).

¹⁵R. J. Haüy, *Essai d'une théorie sur la structure des cristaux*, Paris, 1784.

¹⁶R. J. Haüy, *Traité de Minéralogie* (4 vols.: 494 pp., 617 pp., 588 pp., 591 pp. + atlas, 86 pls.), Paris, 1801. See also P. Groth, *Entwickelungsgeschichte der mineralogischen Wissenschaften*, 265 pp., 1926; J. G. Burke, *Origins of the Science of Crystals*, Berkeley and Los Angeles, 1966.

¹⁷D. C. Goodman, “Problems in crystallography in the early nineteenth century,” *Ambix*, 16 (1969), 152–166 (p. 153). See also D. C. Goodman, “Wollaston and the atomic theory of Dalton,” in *Historical Studies in the Physical Sciences*, Ed. McCormach, Vol. I, 1969, pp. 37–61.

¹⁸R. J. Haüy, *Traité de Crystallographie*, Paris, 1822, vol. II, p. 343.

properly speaking. A relevant document is K. C. von Leonhard's lengthy report of 1816 on the situation of mineralogy. Nature is there presented as having a universally uniform course, proceeding from a principle of necessity, even where it appears haphazard and devoid of law. In order to grasp it, one must follow its genesis from the deep calm of crystalline formations to the fully formed organic creation, "this fruit of the first freedom which brought about a partial victory over (existing) limits."¹⁹

Leonhard described at some length the transition, from the first symmetrical inorganic body—the sphere—to the crystal, in which raw matter attains regular formation. "The effort to achieve certain forms reveals a superior law which governs the forces of extension and tension and which produces a variety of phenomena by virtue of combinations and separations." This in turn leads Leonhard to speak of pure crystal-force ("Krystallkraft") which lets life submerge in the maintenance of shape. Though Romé de l'Islen first recognized the permanence of crystalline angles and its value to the classification of crystals, he never properly developed the geometrical method. Indeed, it was Hauy's merit to have related differences in the shape of crystals to often slight differences in the mixture of materials, whereas widely divergent substances may produce similar crystals.²⁰ Leonhard concludes with a philosophical sigh: "but darkly we surmise the magic hidden in the secrets of these liquids, while we must renounce the rational, the knowledge of causes."²¹

Goethe, preoccupied as he was with problems of organization in animals and plants, wrote at some length on "The laws of organization in general, inasmuch as we must keep them in mind in the construction of the type." This is what he has to say: "In order to clarify the concept of organic being, let us look at mineral bodies. Fixed and unshakeable in their various elementary parts, they appear in combinations which, though formed according to laws, present neither order nor limits. The parts separate readily, to form new combinations which may in turn be eliminated, so that the body, which appeared destroyed, again presents itself in its perfection."²²

¹⁹K. C. von Leonhard, *Bedeutung und Stand der Mineralogie*, Frankfurt, 1816, pp. 83 ff.

²⁰Ibid., p. 87.

²¹Ibid., p. 89.

²²W. Goethe, *Morphologie*, in *Sämtliche Werke* (10 vols.), Cotta, Stuttgart, 1875, vol. IX, p. 493.

Goethe goes on to mention that, once the water of crystallization is lost, beautiful translucent crystals turn to powder, "just as—if a more remote example be permitted—iron filings lined up by a magnet into hairs and bristles disintegrate, once the strong, binding influence is withdrawn. . . . How different are organic beings, even imperfect ones! They elaborate their food—or rather a part of it, the rest being secreted—into several clearcut organs."²³ Once destroyed, a unique living form produced in this manner cannot be reconstituted from its ingredients.

Though Goethe distinguishes the lowly nature of plants from the higher nature of animals, his sensitivity to the "Gestalt" of the entire complicated organism and his general philosophical attitude immunize him against the charms of analogies derived from a study of crystals.

This brings us to Link, a botanist and the author of the voluminous *Elementa philosophiae botanicae*, whose views were praised by Leonhard. Link focuses on aspects of symmetry, and throughout treats crystals, plants and animals as differing above all in degrees. Whereas Hauy initially studied plants, subsequently shifting his interest to crystals, Link, a botanist by profession, extended his theories to cover crystallography.²⁴

Again and again, one comes across a peculiar longing to ascribe to crystals and organisms identical living, directive forces. Ehrenberg and Raspail are two of the authors who share such views.²⁵

When cells became an increasingly attractive subject of study, and their structure was gradually unravelled, the charisma of the crystal model became especially evident. The more so as at first cells were believed to grow from non-cellular initials.²⁶ Schwann, one of the pioneers in this field, often referred to crystals. His views on the matter have been briefly dealt with by Mendelsohn.²⁷ The views of Schleiden, whose vol-

²³Ibid., p. 493.

²⁴H. F. Link, *Elementa philosophiae botanicae*, 486 pp., Berlin, 1824. Link's "objective" attitude is perhaps best illustrated by his recommendation that illustrations of botanical preparations should be made by artists not acquainted with botany. He entirely fails to appreciate that such amateurs will draw bubbles of air and scratches on slides as ardently as cell walls. See also H. F. Link, "Über Wachsen und Anwachsen im Pflanzenreiche," *Verh. Ver. Bef. Gartenb. König. preuss. Staat.*, 40 (1850), 183–196, (p. 183).

²⁵Ch. G. Ehrenberg, "Über Krystallisierungsverhältnisse," *Poggendorf Ann.*, 42 (1835), 237; J. Raspail, *Nouveau Système de Chimie Organique*, Paris, 1833.

²⁶See the fascinating discovery of the structure and activity of the cambium: J. Lorch, "The elusive cambium," *Arch. int. d'hist. sc.*, 20 (1967), 253–286.

²⁷E. Mendelsohn, "Cell theory and the development of general physiology," *Arch. int. d'hist. sc.*, 16 (1963), 419–429 (pp. 422–423).

minous *Introduction to Scientific Botany* was surely the most influential textbook of botany at the time (three editions were published between 1842/43 and 1850), were in rather close agreement with those of Schwann.

Schleiden had a very strong bias for philosophy. (He transferred to botany from law). His particular admiration was for Kant, or rather for Kant's pupil Fries: "The theory applying to the laws discovered by Kant was subsequently provided by Kant's only true pupil and successor, Fries."²⁸ Elsewhere, he speaks of "the sane clarity which I owe to the philosophy of my teacher Fries, from whose logic I have learned as much botany as from all botanical writings taken together."²⁹

Small wonder, then, that to him "the view that organisms are nothing but the form under which substances capable of imbibition crystallize, appears to be compatible with the most important phenomena of organic life, and may be so far admitted, that it is a possible hypothesis or attempt to put forward an explanation of these phenomena."³⁰

Schleiden deserves to be quoted at some length: "This view will undoubtedly prove to be very fertile, inasmuch as even now it shows that the apparent hiatus between inorganic and organic form is not impassable. Yet I must raise one further point, which concerns an important difference overlooked by Schwann. The matter of the crystal exists in the liquid, already formed, and it suffices to withdraw the solvent in order to force its appearance in fixed form; in cells, at least in those of plants, this is not so. There, the substance organically precipitated ('auskristallisierend') as cell—if I may so express myself—does not exist in the cytoplasm, but is only produced in the instant in which it assumes its form, from other, necessarily present, materials. Indeed, this form appears to be determined by the circumstance that the newly-produced substance is at least relatively insoluble."³¹

Schleiden goes on to criticize Link's theory of crystallization. He considers it a mistake on the part of Link, to have proceeded from the observation of saturated solutions under conditions of cooling or evapo-

²⁸M. J. Schleiden, *Über den Materialismus der neueren deutschen Naturwissenschaft, sein Wesen und seine Geschichte*, Leipzig, 1863, 57 pp. (p. 43).

²⁹M. J. Schleiden, *Grundzüge der wissenschaftlichen Botanik*, 3rd. ed., 2 vols., Leipzig, 1850, vol. II, p. 115.

³⁰Ibid., vol. I, p. 214.

³¹Ibid., vol. I, p. 215.

tion. In the former case, Schleiden claims to have observed the formation of a membrane, between two solutions brought into contact: "Closer observation shows this to consist entirely of crystals . . . if the liquid is not disturbed, those grow into the solutions on both sides." Though it is difficult to imagine how he conducted these studies, his "careful and varied observations" led Schleiden to the conclusion "that all inorganic substances, when passing undisturbed into the solid state, form crystals instantaneously."³²

A somewhat more guarded, yet equally optimistic presentation of Schwann's theory, states the following: "Even if one is not prepared to accept Schwann's analogy between cell and crystal and considers this as for the time being entirely unfounded, there is nevertheless available in this clever exposition the undeniable possibility, that one day science will successfully explain the cell as an equally necessary form, in a relatively solid state, of a permeable (assimilated, organic) substance transformed according to law, just as crystals are transformed from impermeable (inorganic) substances. All organisms which are formed or reproduced as individual single cells would then represent particular species of organic crystallization. . . . Between these and specifically determined forms there would be a great lacuna, which would justify us in placing the former as a separate class between crystals on the one hand and plants and animals on the other."³³

Concluding the relevant discussion, Schleiden states that, as far as he can see, "there is at least one species of cells which is most probably not formed from organic germs . . . the yeasts." In line with these considerations, the yeasts should be regarded "neither as fungi nor as a definite group of plants, but as organic crystallizations."³⁴

I have elsewhere analyzed at some length the immense impact which Schleiden's *Grundzüge* had on his contemporaries.³⁵ Surely it is Schleiden's merit to have accorded the study of the cell an unprecedented importance, which it was never to lose again. Yet his blind admiration for philosophy in general, and for such beautiful theories as those on

³²Ibid., vol. II, p. 515.

³³Ibid., vol. II, p. 519.

³⁴Ibid., vol. II, p. 519.

³⁵J. Lorch, in Introduction to M. J. Schleiden (E. Lankester, tr.), *Principles of Scientific Botany*, London, 1849 (reprinted New York and London, 1969), pp. ix-xxxv.

crystallization, led him into deep error. His unfortunate theory on cell formation, according to which the nuclei of new cells are formed from existing cells by a process of crystallization, and the new cells themselves subsequently grow from these, shows some of the pitfalls which over-enthusiastic theorizing presents to unwary observers with but inadequate means of observation at their disposal.

Unger, in his *Foundations of the Anatomy and Physiology of Plants* (1846) generally follows Schleiden, even in his considerable ingenuity in *a priori* reasoning. "The powers (of plants) by virtue of which they function, cannot be special powers, but only such as are obtained with matter absorbed from the outside, or else awakened by such uptake."³⁶

In the introduction to his treatment of the activities of the cell he states that as yet the formation of cells without the direct influence of already formed cells has only been observed in fermentation: "The formation of cells has been referred to as organic crystallization of matter capable of being imbibed ('imbibitionsfähiger Stoffe'). Yet both processes (i.e. organic and inorganic crystallization) have but this in common, that they are formative processes ('Gestaltungsprozesse')."³⁷ In his treatment of organic crystallization, Unger dwells on the uniqueness of the cell. "All considered, one could regard as crystals the nucleus and other solid ingredients of the cell contents, such as starch-grains, various exudates or mucilaginous particles; the cell itself, however, should in any case be regarded as a formation of a higher grade."³⁸ In line with this, Unger refers to the *cytoblastema*, an early term for protoplasm, as "*Mutterlauge*" the term generally applied to solutions of crystalline substances. "In the present state of knowledge it hardly seems defensible to claim that there exist sharp boundaries between the nature of crystal and plants on the one hand, and of plant and animal on the other."

By 1866, Unger sounds even more confident: "The formative power ('Bildungskraft') of the cell is identical ('ist Eins mit') with the formative power ('Gestaltungskraft') of a crystal. Just as the latter acts only in certain directions, so the formative power of cells has its predetermined modes, within which it raises itself from step to step to greater perfe-

³⁶F. Unger, *Grundzüge der Anatomie und Physiologie der Pflanzen*, Wien, 1846, 131 pp. (p. 61).

³⁷Ibid., p. 64.

³⁸Ibid., p. 64.

tion. Like the crystal, the cell forms itself from the liquid state, and like it, it grows by apposition of new ingredients. Yet, this growth is not always confined to the surface of the liquid and solid phases (apposition), but—by virtue of the molecular condition of the organic substance—also throughout the entire body of the solid substance (intususception)."³⁹

Taken with an extreme bias in favor of the crystal paradigm, Unger thus managed to turn the heated arguments of those days on growth by apposition vs. growth by intususception into an internal quibble and thus deprive them of their anti-paradigm sting.

The attitude here presented must be borne in mind by anyone trying to appreciate the motives which led C. Nägeli—surely one of the most influential German botanists of last century—to devote a volume of 624 pp. to the starch grain.⁴⁰

Nägeli explains his choice in the introduction as follows: "... I am convinced that the starch grain is certainly the most suitable, perhaps even the only object of all vegetable—if not of all organic—creations, which gives hope for the establishment of a new discipline, which may be referred to as the molecular mechanics of organized bodies ('Molekulärmechanic der organisirten Körper')."⁴¹ His book contains a vast array of microscopic details, though without any proper discussion of the limitations of the microscope.⁴² Much space is also devoted to observations of the growth of starch grains. Evidently, Nägeli felt as present-day electron-microscopists have occasionally tended to feel, that their chosen instrument was a reliable means to reach the ultimate limits of knowledge.

It is not without interest, that owing to the initial influence of Schleiden and the subsequent studies of Nägeli, the starch-grains have for many years—here and there until this day—been the subject of a quite exaggerated attention in botanical studies and in the training of micro-

³⁹F. Unger, *Grundlinien der Anatomie und Physiologie der Pflanzen*, Wien, 1866, 178 pp. ill.

⁴⁰C. Nägeli, "Die Stärkekörner," in C. Nägeli and C. Cramer, *Pflanzenphysiologische Untersuchungen*, vol. II, Zurich, 1858, 624 pp., 26 pls.

⁴¹Ibid., p. ii.

⁴²C. Nägeli subsequently coauthored a book on microscopes: C. Nägeli and K. Schwendener, *Das Mikroskop*, last German edition 1877, second English edition 1892.

copists, even though such translucent objects of often indistinct laminate structure are really rather ill-suited to train beginners in microscopy.

Yet the charisma of the crystal-analogy extended beyond the development of cells or yeasts, to the growth of organisms, societies and species.

In his *Genesis of Species* Mivart, the Catholic critic of Darwin's *Origin of Species*, claims that the same innate powers evident in chemical atoms, gemmules and physiological units should be attributed to each individual organism. This statement was subsequently discussed by C. L. Morgan and Th. H. Huxley. Huxley believed Mivart's position to be based on considerations other than scientific, leading to an analogy of very doubtful validity. "Yes, Sir," replied Morgan, "save in this that both invite us to distinguish between an internal factor and the incidence of external conditions."⁴⁴

Herbert Spencer, renowned for his dynamic, often unbridled imagination, also constantly has crystallizing processes working at the back of his mind. Phenomena of growth seem to have had a peculiar attraction for him: "Perhaps the widest and most familiar induction of Biology is that organisms grow. . . . Crystals grow. . . . Growth is indeed a concomitant of Evolution. . . . The essential community of nature between organic growth and inorganic growth is . . . that they both result in the same way." "The deposit of a crystal from a solution is a differentiation of the previously mixed atoms."⁴⁵ In both crystals and living organisms, this growth is presented as "but an instance of a universal tendency toward the union of like substances and the parting of unlike substances."⁴⁶

The following excerpt should help to make clear his way of thinking:

"It has been shown of particular salts, A and B, coexisting in a solution not sufficiently concentrated to crystallize, that if a crystal of the salt A be put into the solution, it will increase by uniting with itself the dissolved atoms of the salt A; and that similarly, though there otherwise

⁴³K. Goebel, *Gedächtnisrede auf Karl von Nägeli*, 21 March, 1893, 19 pp. (p. 11)

⁴⁴St. G. Mivart, *The Genesis of Species*, 2nd ed., London, 1871. See also Th. H. Huxley, "Mr. Darwin's Critics", in *Darwiniana*, New York, 1898, vol. II, pp. 120-186. The meeting is described in C. L. Morgan, *Evolution*.

⁴⁵H. Spencer, *Principles of Biology*, American Home Library, New York, 1902, vol. I.

⁴⁶Ibid., p. 150.

takes place no description of the salt B, yet if a crystal of the salt B is placed in the solution, it will exercise a coercive force on the diffused atoms of this salt, and grow at their expense. No doubt, much organic assimilation occurs in the same way."⁴⁷

The intricate and intriguing phenomena of regeneration, which are the last resort of convinced vitalists, are treated by Spencer with somewhat less confidence: the "deductive interpretation of the phenomenon of repair, is by no means (so) easy . . . since organs are in part made up of units that do not exist as such in the circulating fluids."⁴⁸ But then "groups of compound units have a certain power of moulding adjacent fit materials into units of this form."⁴⁹ Here follows an ingenious argument, based on the property of immunity that is shown by blood: "May we not reasonably suspect that the more or less specialized molecules of each organ have, in like manner, the power of moulding the materials which the blood brings to them into similarly specialized molecules? . . . The repair of a wasted tissue may, therefore, be considered as due to forces analogous to those by which a crystal reproduces its lost apex, when placed in a solution like that from which it was formed. In either case, massed units of a given kind show a power of integrating with themselves diffused units of the same kind, the only difference being, that the organic mass of units arranges the diffused units into special compound forms, before integrating them with itself. In the case of the crystal, this reintegration is ascribed to polarity—a power of whose nature we know nothing. Whatever be its nature, however, it appears probable that the power by which organs repair themselves from the nutritive matters circulating through them, is of the same order."⁵⁰

"If in the case of the crystal, we say that the whole aggregate exerts over its parts a force which constrains the newly-integrated atoms to take a certain definite form we must, in the case of the organism, assume an analogous force. This is . . . a generalized expression of the facts." After some examples of regeneration, Spencer remarks that "the form of each species of organism is determined by a peculiarity in the

⁴⁷Ibid., p. 151.

⁴⁸Ibid., p. 177. See also Pasteur's remarks on "mutilated" crystals and the subsequent "rétablissement de la régularité dans la partie mutilée", in *Compt. rend. Acad. S.*, 2(17) (Oct. 1856), 195.

⁴⁹Spencer, *op. cit.*, p. 177.

⁵⁰Ibid., p. 179.

constitution of its units, that these have a special structure in which they tend to arrange themselves; just as have the simpler units of inorganic matter."⁵¹

Interestingly enough, Spencer's thread was picked up by August Weismann, father of the theory of germ-plasm. This is his summary of Spencer's view: "Spencer considers, on the one hand, that the whole organism is composed of these units, which are all alike in kind, and on the other, that the germ cells also contain small groups of them. The former supposition makes regeneration possible to each sufficiently large portion of the body, while the latter gives the germ-cell the power of reproducing the whole: inasmuch as the 'polarity' of the units leads to their arrangement in such a way that the whole 'crystal'—the organism—is restored, or even formed anew. The mere *difference in the arrangement* of units, alike in kind, determines the diversity of the *parts of the body*, while the distinction between different species and that between different *individuals* is due to a diversity in the constitution of the units."⁵²

Earlier, Weismann had written: "The living organism has already been often compared with a crystal, and the comparison is, *mutatis mutandis*, justifiable. As in the growing crystal the single molecules cannot become joined together at pleasure, but only in a fixed manner, so are the parts of an organism governed in their respective distribution. In the crystal, where nothing but homogeneous parts become grouped together, their resulting combination is likewise homogeneous, and it is obvious that they offer but very little possibility of modification, so that the governing laws thus appear restricted and immutable. In the organism, whether regarded microscopically or not, various parts become combined, and these therefore offer numerous possibilities of modification, so that the governing laws are more complex, and appear less restricted and unchangeable. In neither instance do we know the final causes which always lead to a given state of equilibrium; in the case of a crystal it has not occurred to anybody to ascribe the harmonious disposition of the parts to a teleological power; why then should we assume such a force in the organism, and thus discontinue the attempt, which has al-

⁵¹Ibid., pp. 179–180.

⁵²A. Weisman (N. Parker tr.), *The Germ-Plasm*, New York, 1893, Introduction, § 2.

ready been commenced, to refer to its natural causes that harmony of parts which is here certainly present and equally conformable to law?

"On these grounds the assertion that the theory of selection is not an attempt at a 'mechanical' explanation of organic development appears to me to be incorrect. Variability and heredity, as well as correlation, admit of being conceived as purely mechanical, and must be thus regarded so long as no more cogent reasons can be adduced for believing that some force other than physico-chemical lies concealed therein."⁵³

With this attitude governing his thoughts, Weismann obviously finds Spencer's "physiologically variable units," which presumably act in every case in such manner as the whole demands, unsatisfactory even as an explanation of the differentiation of organs in ontogeny. Incidentally, he claims that the existence of similar "Elementar-Organismen" (referring to the organization of cells, quite distinct from the molecular structure of the organic compounds) had been suggested before Spencer by Ernst Brücke.⁵⁴

An attitude similar to Spencer's led Pringsheim, a pioneer in the study of algae, to make the claim that: "The insight we have obtained into the regularity of plant structure, into this new architecture or crystallography of these plants . . . represents one of the most beautiful achievements of human intelligence in the domain of nature-study."⁵⁵ Strasburger, another outstanding botanist of the late nineteenth century, similarly refers to the development of new tissues as a "mathematically necessary" consequence of antecedent events.⁵⁶

We have seen how unknown forces of the kind which hold together and produce regularity of pattern and growth in crystals were variously postulated as responsible for regularity in cells and organisms. The charisma of the simplicity and elegance assumed by Maupertuis and Häuy to govern the development of crystal structures was such, that its adherents remained undaunted even by the difficulties presented by liv-

⁵³A. Weisman, *Studies in the Theory of Descent*, London, 1882, 718 pp., vol. 2, p. 674.

⁵⁴A. Weisman, *op. cit.* in note 52 (Introduction 2). Cf. E. Brücke, *Wiener Sitzungsberichte*, 44(ii) (10 October 1861) 381.

⁵⁵N. Pringsheim, "Über Richtung und Erfolge des cryptogamischen Studien" (Inaugural address, University of Jena, 26 October 1864), in *Gesammelte Abhandlungen*, vol. III, pp. 307–320 (p. 315).

⁵⁶E. Strasburger, *Neue Untersuchungen über den Befruchtungsvorgang bei den Phanerogamen etc.*, Jena, 1884, 176 pp. ill.

ing organic forms. In fact, the fascination of the crystal model even extended to the discussion of the nature of species, races and nations.

It is perhaps no accident that it was a professor of chemistry, the well-known W. Ostwald of Leipzig, who, about the beginning of the First World War, in an interview granted to a visiting journalist, made the arrogant claim that the Germans were superior to the French because Germany had discovered the secret of organization. Or, in Ostwald's own words: "We, or rather the Germanic race, have discovered the factor of organization. Other peoples still live under the regime of individualism, whereas we live under the regime of organization."⁵⁷

It is surprising, indeed, that vitalistic thinkers have repeatedly found their way to fascist attitudes. Overwhelmed by their romantic notions, and unable or undisciplined enough to recognize the differences for what they are, they were only too prone to project ideas of perfection, in purity and organization, onto the social and historical scene.

Woodger, in one of his more lucid passages, presents the case as follows: "Just as the crystal is an exemplification of a chemical substance in the 'secondary meaning' of substance in Aristotle, so an organism is an exemplification of a secondary substance—a 'biological substance'—namely the race to which it belongs. And just as the chemist distinguishes pure and compound substances, so also the geneticist recognizes pure and mixed races."

According to Woodger, the comparison may be carried a step further: "A specimen of an elementary chemical substance cannot be altered. And similarly, although the geneticist can change mixed races by crossing, the pure races seem to be extremely stable. . . . Thus the problem is to know how we are to conceive this extremely stable entity whose manifestations we see in the observable characters of individual organisms. . . ."⁵⁸

One has but to compare such more or less recent statements with the view expressed by Buffon long ago, to realize that the spell of the crystal is far from broken: "It appears that the Supreme Being wished to employ but a single idea, and to vary it simultaneously in all possible

⁵⁷For details and numerous reactions, including a statement by E. Heriot see J. Labadie ed., *L'Allemagne, a-t-elle le secret de l'organisation?* Paris, 1916, 5th ed., 281 pp.

⁵⁸J. H. Woodger, *Biological Principles: a critical study*, London, 1929, 496 pp. (p. 346).

ways, so that man shall be able to admire both the magnificence of the execution and the simplicity of the design."⁵⁹ Like Buffon, we too are prone to be attracted by a persuasive hidden resemblance, more wonderful than all apparent differences, which we must do our best to overcome.

In conclusion, I wish to add my hearty congratulations to Professor Sambursky who introduced me to the fascination of science while I was a student, and subsequently helped me to discover the excitements of its history when I joined the staff of his budding department.

⁵⁹Quoted after M. Flourens, *Eloge Historique d'Etienne G. Saint-Hilaire*, Institut National de France, 1852, p. 16. See also A. D. Lovejoy, *The Great Chain of Being*, 1936. There, crystals are not given any particular attention.

SCIENCE AND PHILOSOPHY IN THE PROBLEM
OF THE DIMENSIONALITY OF SPACE

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The problem of the dimensionality of physical space is as old as the philosophy of space and time. It is also a problem in which philosophic reflection and scientific deliberation interacted to a large extent.

Only in recent times has it become clear that this problem consists of three different, though interrelated, questions: (1) What, precisely, is meant by saying that space has a certain number of dimensions? (2) How do we know that it really has, as generally claimed, three dimensions? (3) Is there any *raison d'être*, an inherent reason, for such a specific dimensionality?

As to (1), it is well known that for mathematical spaces, and topological spaces in particular, the answer was fully clarified some decades ago through the work of Poincaré, Brouwer, Menger, Urysohn and others. If, therefore, the abstract model of mathematical space, be it Euclidean or not, can be applied for physical space, question (1) has a clear-cut answer. Since it can however be shown that up to sets of zero Lebesgue measure a continuous one-to-one mapping between spaces of different dimensionalities, as defined in pure mathematics, can be established and since such null-sets may well be physically meaningless, the legitimacy of applying the mathematical model for physical space can be questioned. In this case the dimensionality of physical space is still an undefined notion.

That the dimensionality of space had to be accepted as an accidental feature, justified only by experience in so far as just three coordinates, to use the modern term, determine unambiguously the location of a point-object in physical space, was already in antiquity regarded as a serious deficiency in any deductive theory of space. Aristotle considered this problem worthy of a detailed discussion in the opening chapters of his *De Caelo*. His treatment of this question was greatly influenced by the Pythagorean doctrine of the "perfection" of the number three.

Aristotle's "derivation" of the three-dimensionality of space has rarely, if ever, been criticized by his commentators. In fact, the earliest refutation was probably given by Galileo in his *Two Principal Systems of the World*. Galileo's own "proof" of the three-dimensionality is based on the alleged impossibility of constructing, in a given point, more than three mutually perpendicular lines.

It became the standard argument for centuries to come, though at best it merely shows the limitation of our imagination to visualize space of higher dimensionality. True, in the course of the following centuries many more demonstrations of increasing sophistication have been given; but if scrutinized they are all based on the limitation of our visualizability. Hermann Lotze's famous "proof," expounded in his *Metaphysik* (Book 3, Chapter 2) and refuted by Bertrand Russell in his *Essay on the Foundations of Geometry*, and Paul Natorp's argument, proposed in his study of *Die logischen Grundlagen der exakten Wissenschaften* and disproved by Max Jammer in his *Concepts of Space*, are classic examples of this fact. That also Kant's attempt to reduce the three-dimensionality of space to the peculiarities of Newtonian dynamics and in particular to the law of universal attraction was untenable has been shown by Trendelenburg's *Logische Untersuchungen*.

In the *System des transzendentalen Idealismus* Schelling tried to deduce matter and its three-dimensionality from the existence of three forces, magnetism, electricity, and chemical force. Though obviously incompatible with the results of modern physics—there are more than three forces or kinds of interaction in nature and only one of them, the electromagnetic interaction, may be said to have been referred to by Schelling—his correlation between the number of basic physical data and the dimensionality of space may be regarded as an anticipation of a modern approach to solve the problem, as we shall see.

At the beginning of the present century it seems to have been generally recognized by philosophers and scientists alike that no *a priori* logical or mathematical proof of the empirically acknowledged three-dimensionality of physical space could be maintained. Emphasis thus shifted to *a posteriori* demonstrations or, more precisely, to the study of the specificities of well-established physical laws with respect to the dimensionality problem.

In fact, the earliest paper of importance along this line, which how-

ever attracted little attention at the time of its publication, expressed the shift already in its very title. It was Paul Ehrenfest's article "In what way does it become manifest in the fundamental laws of physics that space has three dimensions?," published in the *Proceedings of the Amsterdam Academy*, volume 20, in 1917. Ehrenfest, following J. Bertrand and H. Lamb, showed that in a space of more than three dimensions the laws of physics do not admit stable orbits in planetary systems such as our solar system or the Bohr model of the atom. The case of one- or two-dimensionality required in Ehrenfest's treatment special considerations. It is however possible, as can be shown in full detail, to improve Ehrenfest's calculations so as to obtain the unambiguous answer of three-dimensionality as a necessary condition for orbital stability. In addition, referring to Hadamard's investigations of the Huygens' principle for wave propagation Ehrenfest pointed out that electromagnetic signals could be transmitted in an undistorted manner only in odd-dimensional spaces.

Ehrenfest's arguments were further elaborated by Whitrow and Büchel and, in particular, by Tangherlini who generalized the result for relativistic Keplerian orbits. Tangherlini derived the dimensionality of space from what he called the "bound state postulate," namely the assumption that "there shall exist stable bound orbits or 'states' for bodies interacting through fields which asymptotically approach a constant value at large distances."

In spite of the interesting aspects of Tangherlini's work, especially from the mathematical point of view, we do not think that the reduction of three-dimensionality (or four-dimensionality for space-time) to orbital stability is a philosophically satisfactory procedure. Stability is an expression of conservation (at least, conservation of the geometrical configuration), and conservation, in turn, is the expression of a certain symmetry property of the system under discussion.

If these associations are correct then the development of our problem, which started with Kant's reduction of dimensionality to the law of forces and their effects on motion and ended with Tangherlini's "bound state postulate," is intimately connected with another development that likewise had its origin with Kant, viz., the existence of enantiomorphous objects (that is, symmetrical but not congruent objects, such as left and right hands) and certain philosophical conclusions drawn

from their existence with respect to the dimensionality problem. Thus, F. Zöllner derived from their existence the necessity of higher-dimensions while P. Milau based upon it his proof of the three-dimensionality of space. More productive in this respect were the symmetries underlying the special and general theories of relativity (Lorentz invariance, general covariance).

Indeed, with the development of the theory of relativity and its concomitant investigations in differential geometry it might have been expected that new lines of approach toward a solution of our problem should have been advanced. If according to Einstein's original conception of Mach's principle the geometry of physical space should depend upon the distribution of matter (or energy), one could have expected that not only the metric but also the topology and dimensionality of space should be derivable from other principles. However, conventional general relativity, it seems, did not find it possible to represent matter and energy distributions in a sufficiently general manner without an *a priori* assumption of spatial three-dimensionality or space-time four-dimensionality. In any formulation of the energy-momentum tensor ever proposed, which in virtue of the field equations was to be the determinative factor for the geometry of space, it was tacitly assumed that space is three-dimensional (or space-time four-dimensional). The very idea that also the dimensionality of space should be subjected to some kind of Mach principle seems never to have been suggested.

And yet, in certain respects relativity sheds new light upon our problem. An early indication in this direction is the fact, noticed already by Ehrenfest, that only in a three-dimensional space does the polar vector of the electric field have the same number of components as the axial vector (or more precisely, antisymmetric tensor) of the magnetic field, a fact necessary for their common participation and interchangeability in the Lorentz transformation. But as this early example also suggests, relativity, whether special or general, seems to contribute little to our problem as long as only pure kinematics or dynamics are involved. Its relevance to our problem, it seems, becomes apparent only when, possibly in addition to gravitation, also electromagnetism or other fields are considered.

The classical example to corroborate this thesis is of course Hermann Weyl's celebrated paper of 1918 on "Gravitation and Electricity," originally published in the *Sitzungsberichte der Berliner Akademie der*

Wissenschaften, in which his interpretation of a gauge vector field as the vector potential of the electromagnetic field led to a unified theory of gravitation and electricity with the conclusion that "the possibility of the Maxwell theory is restricted to the case of four dimensions". Fritz London's strange result obtained in his 1927 paper (*Zeitschrift für Physik*, vol. 42), seems to open a way to connect Tangherlini's postulate with the Weyl theory.

An even more profound argumentation in favor of the four-dimensionality of the space-time continuum can be established, following Einstein and R. Penney, if the notion of "the strength of field determination," introduced on p. 133 of the fifth edition of Einstein's *The Meaning of Relativity*, is adduced. For it can be shown that the Maxwell equations of the electromagnetic field, the Einstein equations of the metric field, and the Weyl equations of the free neutrino field have precisely the same "strength" if and only if the space-time continuum is four-dimensional. If we recall that the Maxwell photon, the Einstein graviton and the Weyl neutrino are precisely the three massless long-range particles which describe the physics of empty space, and that most probably a singularity-free metric cannot be established in such a space unless these three fields have the same determinative "strengths," we see that this argument may be regarded as a modern scientific version of Schelling's "proof" in his *Naturphilosophie*.

Additional field-theoretic arguments along these lines can be adduced through a judicious analysis of the CTP-theorem. In fact, modern physics abounds in indications that space is three-dimensional. To quote only one more example we refer to the theory of phase transitions: from liquid-gas and ferromagnetic transitions we know with certainty that for molecules having interactions of finite range, phase transitions are possible only if the number of dimensions is greater than one; but ferromagnetic to non-magnetic transitions involve magnetism, that is, a phenomenon which is due to spin magnetic moments, and spin is a three-dimensional phenomenon, a necessary conclusion from group-theoretic considerations.

In view of these results we may say that modern physics produces a number of *indicia*, based on rather general principles, to the effect that physical space-time is four-dimensional. But we must also admit that a *cogent proof* has not yet been established. Thus far the physicist.

For the philosopher (or philosopher-physicist) an additional ap-

proach may be available. For reasons which we shall not explain at present, Maxwell's equations are much more fundamental than the equations of mechanics or of the Einstein field—already in virtue of the fact that they can be formulated without any recourse to a metric. If, moreover, we admit that all our information in physics is obtained via the electromagnetic field—the “seeing” or “touching” of objects in the ordinary macrophysical realm, the “bubble chamber tracks” in the microphysical realm, and the exchange of light-signals in the megophysical (cosmological) realm are nothing but electromagnetic phenomena—we may be tempted to conclude as follows: *Since* Maxwell's theory is to be formulated, if not necessarily so at least most conveniently, in terms of a four-dimensional space-time structure, and *since* all our knowledge about the physical world is obtained through the “spectacles” of electromagnetism, space-time is four-dimensional because our “spectacles” are such.

Thus philosophers and physicists, though on different grounds, arrive at the same conclusions.

STATISTICAL CAUSALITY IN ATOMIC THEORY

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Well, I shall start by explaining why I am here: my main motivation is that I was so glad to have this opportunity of meeting my old respected friend Sambursky again, especially on this festive occasion.

The subject of my talk, or rather the choice of it, is also inspired by Sambursky, namely by a paper that he has written about the curious fact that in the ancient world there was no conception of statistics, of randomness—that was completely foreign, according to the evidence he has collected, to the mentality, to the outlook of the ancients. So it is a modern concept and even in modern science it has only appeared—the physicists, so to speak, dared to use it with apology—in the 19th century. Now we have come to the point of being forced to realize that statistical causality is *the* form of causal link that dominates practically the whole of our description of the material world and very probably also of the organic world, and this turn of outlook has come about through the atomic theory—through the representation of the world as a huge system of atoms. If I may describe it like this—we have passed from the system of representation of deterministic trajectories of large bodies with which Newton was very much concerned, to the random walk of tiny atoms. The God of Newton held a firm hand over the planets—the God of the atomic physicist is intoxicated.

When one takes atoms seriously, one is necessarily led to a kind of consideration borrowed, as Maxwell put it, from the department of statistics, because it is quite essential—this is a point which is not always sufficiently emphasized—it is essential, for a rational atomic representation of the phenomena, to assume that the atoms are identical in structure, or at least that they consist of a few species of identical structures. Otherwise it would be quite impossible to make any coherent description of macroscopic bodies.

On the other hand they must be so small that they are inaccessible

to direct observation. This creates a new epistemological situation, because we have somehow to describe the motions of the atoms and the forces with which they interact. With the refinement of technology, as you know, we are now able to make observations of individual atomic processes, but of course any such observation always implies an amplification device, in order to bring the atomic phenomenon that we want to investigate to our level of observation, which is macroscopic. Therefore, what we actually observe are interactions between single atoms or atomic systems and macroscopic bodies or instruments. I prefer to speak in terms of fully automatic registration, so that we keep out the human observer and all the problems that are connected with that aspect of the process. This is a quite novel situation in physics, and I need not remind you of the tremendous consequences for epistemology that have arisen out of it and which are expressed by the code word of "complementarity", introduced by Niels Bohr to remind us of them.

This complementarity at the level of individual atomic processes introduces a dichotomy between the two fundamental aspects of 19th century physics, according to which we can describe phenomena: the one being the very old and primitive one of space-time location (which by itself raises epistemologic problems—but let us accept it as a well-defined mode of description, always on the macroscopic scale), the other being the energetic aspect, which is a 19th century creation—the general conception that the various forms of energy are interchangeable and that there is a conservation of the energy (and, related with it, of the momentum) of any closed system.

Those two aspects are complementary, which means that when we concentrate on one aspect, we completely exclude the possibility of defining the other. Now this complementarity is of course not the result of an arbitrary choice on our part; it is based on a law of nature, namely, the existence of an element of discontinuity in the interactions between the atoms and the macroscopic instruments—the existence of the quantum of action, which limits the applicability of the features of continuity in the classical description: there can be no question of a trajectory, since a continuous variation of position would imply changes of action less than one quantum.

One very often hears the question: is this complementarity a property of the atoms themselves, or is it imposed by us on the atoms? This

is a badly formulated question: the two partners—the atoms and ourselves—are inseparable. We are in the queer position of being ourselves macroscopic systems of atoms, and the only concepts with which we can communicate experience are concepts adapted to our scale, macroscopic concepts; we have also to describe, by means of these, experience derived from the interaction of those tiny atoms with our macroscopic receptors. To find fault with this situation is no help—we have to live with it. The concept of complementarity just reminds us of the necessity of always taking due account of the conditions in which complementary concepts may be used in the description of atomic phenomena. These conditions are embodied in a perfectly consistent mathematical theory, which allows us to cope with every possible type of phenomenon. Certainly, this formalism is adapted to observational situations "only"—well, there are no other situations that we can speak about intelligibly.

Thus, quantum mechanics always refers to conditions of observation, which enter into the equations in two ways: first as what we call *external parameters*—the intensity of a magnetic field, for instance, which is generated by a big electro-magnet and measured in the usual, classical way; then as *boundary conditions*, by which we select that solution of the equations which is adapted to the experimental arrangement: this is always describable entirely in terms of classical concepts.

Here there arises a problem of logical consistency. Because our instruments are themselves systems of atoms, their behavior during the observation of an atomic process must also be describable by means of Schrödinger's equation, if we introduce in this equation a complete atomic description of the experimental arrangement and its interaction with the atom that we are observing. In principle we can find the solution of this problem, and it should lead to the same result as the procedure that I have just described, involving the summary description of the conditions of observation by external parameters and boundary conditions.

Clearly, this does not mean that we could dispense with the classical concepts, or "reduce" them to quantal operators, since we already put them into the interpretation of the symbols with which we work in the Schrödinger equation: in order to interpret the solution for the whole system we have to use the same prescriptions that we use when we are interested in the behavior of a single atom. All we can achieve is to demonstrate that the relation established in quantum mechanics between

the formalism and the classical concepts describing macroscopic observation is *consistent*; this is a necessary ingredient in the formulation of this relation, since after all it is our duty to show that whatever results we express in classical terms are consistent with the formalism that we claim is capable of giving us those results.

Now, this brings us then to a definite problem in quantum mechanics, namely, that of the time evolution of systems with many degrees of freedom. This problem was first treated in the early days of quantum mechanics by von Neumann, who took as a model the similar treatment of classical systems which goes back to Maxwell and Boltzmann. But von Neumann's theory is not satisfactory; its failure is due to a deep-lying difficulty, which I must try to explain.

Let me first recall the argument of the classical ergodic theory. A state of the system is represented by a point in a multidimensional phase space, whose coordinates are the whole set of canonical coordinates and momenta of all the constituent elements, and whose metric is determined by the condition of being independent of the choice of canonical coordinates and momenta, and also invariant in time: Liouville's theorem shows that these requirements are satisfied by a simple Euclidian metric. A statistical distribution of states in a domain of phase space will be deformed and spread out in a complicated way in the course of time, but it will always occupy the same total volume of phase space: its evolution is comparable to an incompressible flow. As a result, if the evolution of the system is confined to a limited region of phase space, every bundle of trajectories starting from some domain within this region, however it may spread over it in the course of time, must eventually return to the initial domain, and not only once, but an infinity of times —this is the content of Poincaré's famous theorem about the cyclic character of the time evolution of a finite system; it strikingly expresses the reversibility of its dynamical behavior.

How can we now pass from this dynamical behavior at the atomic level to the level of macroscopic observation? Obviously, by an averaging process: we do not measure the value of a physical quantity for each single element of the system, but only its average value for the whole system at some instant of our macroscopic time, which means over a time interval very large on the atomic scale, involving many interactions between the constituent elements of the system.

In fact, we may just take the average over an infinite time—which is a convenient idealization from the mathematical point of view: for we know that on the macroscopic scale the system, after a certain relaxation time, short on that scale, reaches a state of equilibrium; and it does not matter if we extend the average over the infinity of Poincaré cycles during which (ideally, of course) the evolution of the system just repeats itself. At first sight, it would seem, however, that calculating such a time average is a hopeless undertaking, since it would require solving the equations of motion for the whole system; but we could by-pass this task if we only knew—or guessed—how often, on the average, the system occupies in the course of time each of its possible configurations: the time average would then be replaced by a statistical average over the part of phase space in which the system evolves.

Suppose we take a film of the successive configurations of our system: to obtain the time average of some physical quantity, we have to measure it on each picture and take the sum (divided by the number of pictures); obviously we get the same result if we cut out the single pictures and arrange them, regardless of the times at which they occur, in groups corresponding to the same value of the physical quantity in question: we have then to count how many pictures there are in each group, multiply these numbers by the corresponding values and sum the products (again divided by the total number of pictures).

Maxwell and Boltzmann guessed that a large system of interacting elements would pass equally often through each of its possible configurations: this is what Boltzmann called the "ergodic hypothesis," and it leads, of course, to the simplest possible method for computing statistical averages. Mathematicians, however, frowned upon such a bold intuition, and at long last, after their tools were sufficiently sharpened, they managed to replace it by a forbiddingly rigorous theory, without, however, adding anything to its physical significance. Indeed, the rigorous definition of ergodicity does not provide us with any *practical* criterion helping us to decide whether a given system behaves ergodically or not.

Yet in one respect, ergodic theory did clarify our ideas. Gibbs had compared the endless winding of the trajectory in phase space to a mixing process—the stirring of milk in black coffee, leading to a mixture which has a *coarse* appearance of homogeneity: the mathematical anal-

ysis of this process, carried out by Hopf, revealed that it is logically independent of the ergodic character of the system—it only takes place if a further condition is satisfied. In fact, a mixing essentially depends, for its very definition, on the consideration of more than one trajectory—it turns out that it suffices to consider a pair of trajectories. This is conveniently done by representing a pair of phase space points, each belonging to one of the trajectories, by a single point in a “superspace” with twice as many dimensions, in such a way that the pair of trajectories will appear as a single trajectory in superspace. Then, Hopf’s mixing theorem simply states that the mixing process will occur if, and only if, the system is ergodic in superspace (as well as in the original phase space): this is a more restrictive ergodicity condition, essential for the “normal” thermodynamic behavior of the system. It is essentially a condition upon the *correlations* between pairs of configurations, and it thus emphasized the necessity of taking account of these pair correlations, in addition to the simple average distributions in phase space.

On this particular point, quantum theory has a great formal advantage over the classical description: it can operate, as von Neumann showed, with a density operator which comprehends *both* the average density distributions in the various states of the system (its diagonal elements in a matrix representation using a complete set of such states as a basis) *and* the correlation coefficients between pairs of states (its non-diagonal elements).

This density matrix is defined in terms of the state vector of the system in Hilbert space and its variation in time is expressed by a “Liouville equation” immediately deduced from the Schrödinger equation satisfied by the state vector. In a superspace defined as the direct product of the Hilbert space and its dual, the density matrix appears as a supervector, varying in time according to Liouville’s equation; the latter represents the rate of variation of the density supervector as resulting from a linear transformation of this supervector, effected by a linear superoperator L :

$$i\dot{\rho}(t) = L\rho(t).$$

The Liouville superoperator L may be expressed in terms of the Hamiltonian H of the system; to this end, let us introduce a convenient notation. Consider the special class of superoperators $O \equiv M \times N$ depending on a pair of supervectors M, N according to the definition $O\rho =$

$M\rho N$ (we call them “factorizable”); then we may write $L = H \times 1 - 1 \times H$.

The Liouville equation describes the dynamical behavior of the system on the atomic scale. In order to arrive at a description at the macroscopic level of observation, we ought—along the lines of the preceding classical argument—to introduce the analogue of the mixing process. In the classical case, this is simply done by a “coarse graining” of the phase space, i.e. by averaging the distributions on the atomic scale over finite “cells” of phase space (the shape of which is arbitrary). Here, then, is the stumbling block against which von Neumann’s attempt founded: a phase-space cell with a sharp boundary would contradict the indeterminacy relations, and the substitute von Neumann tried to formulate leads to hopeless complications. Later, it is true, a considerably better approach was found: it consists in subdividing the Hilbert space into bundles of state vectors, which play the part of the cells; it is then necessary to perform an additional averaging over all such subdivisions, and the resulting formalism is still very cumbersome.

However, the introduction of coarse graining, which is the source of this difficulty, is solely a consequence of the cyclic character of the time evolution on the atomic scale, which forces us to extend averages over infinite time and represent the mixing effect of the correlations (which actually goes on all the time) by the schematic form of statistical average we call coarse graining. Now, the cyclic type of time evolution is a feature of finite systems, confined in a finite phase space extension if they are classical, or characterized by a discrete energy spectrum in the quantal case. Infinite systems, on the other hand (i.e. systems of infinite degree of freedom and infinite extension, but such that the number of elements within a given extension has a finite limit), show no cyclic return: the period of the cycle becomes infinite, their energy spectrum is essentially continuous; they may therefore be expected to exhibit directly, in the asymptotic limit of very large, but finite, macroscopic times, the approach to a state of equilibrium which corresponds to macroscopic observation.

This consideration invites us to look for the quantal form of Boltzmann’s kinetic approach rather than for a generalization of ergodic theory: this is the line of investigation initiated by Prigogine and followed by him and his group in Brussels during the last decade. I came

into it only recently, when Prigogine told me of the conclusion he had reached—a conclusion so striking that I did not quite believe it until I had repeated the calculation by a more direct method, which had been outlined, but not fully exploited, by the Brussels group.

In exploring virgin ground, one needs to test new ideas by applying them to concrete cases, and to seek guidance from such applications for further developments: the formalism useful to this end is not generally the one suited to give a systematic account of the main features of the theory. In the present case, it turned out that a different—though of course equivalent—mode of treatment led to a very compact, synthetic presentation of the line of argument and the essential results: it is this final form of the theory, developed in common by Prigogine, George and myself, that I am going to outline to you.

My late participation in the protracted investigations of the Brussels group allows me, perhaps, to take a more detached view of the subject, and I am not infringing the code of modesty about which scientists are sometimes so prejudiced, by telling you that the outcome of these investigations really represents a major advance in the strengthening of the conceptual frame of atomic theory: we have now a powerful mathematical tool to represent and analyse in a surprisingly transparent way the relationship of complementarity between the dynamical behavior of large atomic systems and their macroscopic observation.

Our aim being to find out the long time effect of correlations, we must, to begin with, compare our system, defined by the Hamiltonian H , with a “model” system H_o , from which the interaction energy V , responsible for the correlations, is removed, in such a way that $H = H_o + V$. The eigenstates of H_o form a complete orthogonal basis of representation in Hilbert space, from which we construct a similar basis in superspace: the latter may be divided into two classes of supervectors, those built up of pairs of identical (or physically equivalent) eigenstates, and those built up of pairs of different eigenstates; they belong, respectively, to two orthogonal subspaces of superspace, characterized by projection superoperators P_o , P_c . Then, the projections $\rho_o = P_o \rho$ and $\rho_c = P_c \rho$ of the density supervector correspond, respectively, to the average distribution densities and the correlation amplitudes. Putting $L_{oo} = P_o L P_o$, $L_{oc} = P_o L P_c$, etc. we obtain for ρ_o and ρ_c the coupled Liouville equations

$$i\dot{\rho}_o = L_{oo}\rho_o + L_{oc}\rho_c, \quad i\dot{\rho}_c = L_{cc}\rho_c + L_{co}\rho_o. \quad (1)$$

The next step is to extract from these equations the asymptotic forms $\tilde{\rho}_o(t)$, $\tilde{\rho}_c(t)$ of ρ_o and ρ_c for large *positive* values of the time variable: these are expected to express our possibilities of prediction of the *future* evolution of the system on the macroscopic time scale.

We must here restrict the generality of the Liouville superoperator in order to characterize the class of systems which we expect to exhibit the “normal” asymptotic behavior, i.e. an approach to a state of equilibrium. To this end, we observe that the time evolution of the correlation density ρ_o is essentially governed by the superoperator $T_c = \exp(-iL_{cc}t)$, depending on the part of the Liouville superoperator which acts entirely in the correlation subspace. We assume accordingly that the asymptotic effect of this superoperator $T_c(t)$ upon any regular supervector which is not an invariant is to reduce this supervector to zero: $\lim_{t \rightarrow \infty} T_c(t) A = 0$; we express by this assumption the fading of the system’s “memory” of its correlations.

By means of this assumption, we readily derive from the second Liouville equation (1) the following relation between the asymptotic densities:

$$\tilde{\rho}_c(t) = \int_0^\infty d\tau e^{-iL_{cc}\tau} (-iL_{co}) \tilde{\rho}_o(t - \tau); \quad (2)$$

it has the form of an integral equation, showing how the asymptotic correlations build up by sequences of processes starting from the average situations through which the system passes in the course of time.

Let us introduce at this stage an asymptotic time evolution operator by writing $\tilde{\rho}_o(t)$ in the form

$$\tilde{\rho}_o(t) = e^{-i\theta t} \tilde{\rho}_o(0); \quad (3)$$

the variable t in this formula represents the time measured on a macroscopic scale—to be *quite* rigorous, we ought to restrict the formula to some discrete sequence of values t_1, t_2, \dots, t_n , which, however, could be chosen arbitrarily; let us forget about this restriction, which would complicate things without affording us more insight into the physical aspects. The advantage of the representation (3) is to reduce the integral equation (2) to a simple linear relation between $\tilde{\rho}_o(t)$ and $\tilde{\rho}_c(t)$ taken at the same time:

$$\tilde{\rho}_c(t) = C \tilde{\rho}_o(t), \quad C = \int_0^\infty d\tau e^{-iL_{cc}\tau} (-iL_{co}) e^{i\theta\tau}. \quad (4)$$

The first Liouville equation (1) then yields a functional equation for θ :

$$\theta = L_{oo} + L_{oc}C, \quad (5)$$

which can be solved for θ by iteration.

The total asymptotic density $\tilde{\rho} = \tilde{\rho}_o + \tilde{\rho}_c$, thus obtained has the remarkable property of being an *exact* solution of the Liouville equation. According to eq. (4), it may be written in the form $\tilde{\rho} = P_a \tilde{\rho}$, with $P_a = P_o + C$. It is still more remarkable that this superoperator P_a has the characteristic properties of a projection operator: it defines a subspace in which the asymptotic density is confined. This subspace differs from the average subspace P_o by the adjunction of a part of the correlation subspace P_c , namely that part which is specified by the superoperator C ; the latter may be interpreted as representing the building up of correlations from *asymptotic* average situations (we call it the superoperator of correlation creation)—it sorts out those correlation processes which have a long time-effect and accordingly manifest themselves at the macroscopic level.

Thus, the asymptotic density $\tilde{\rho}$ is not, as one might have expected, governed by a “kinetic equation” different from the dynamical Liouville equation: it is an exact solution of the latter, and its asymptotic character is conferred upon it by its confinement to a subspace defined by the projector P_a . The expression for the superoperator of correlation creation C , which enters in the definition of P_a , is clearly unsymmetrical in time, and gives the projector P_a the expected bias towards a preferred direction of the time-evolution. In fact, time-inversion transforms P_a into a different projector $\bar{P}_a = P_o + D$, where the superoperator

$$D = \int_0^\infty d\tau e^{i\eta\tau} (-iL_{oc}) e^{-iL_{oc}\tau}$$

is the time-inverse of C ; it contains the superoperator $i\eta$ which is the time-inverse of $i\theta$ and obeys the equation $\eta = L_{oo} + DL_{co}$ derived from eq. (5) by time-inversion. In contrast to C , the superoperator D describes sequences of “destructions” of correlations leading to asymptotic average situations. We thus see that in passing to the macroscopic level by means of the projector P_a , we have destroyed the time-reversal invariance of the dynamical behavior on the atomic scale and imposed macroscopic time-directedness upon the asymptotic time-evolution.

An important element is still missing in the picture: we must establish a link between the asymptotic density supervector $\tilde{\rho}(t)$ and the ar-

bitrarily chosen dynamical density supervector $p(0)$ from which the time-evolution is assumed to start. This is readily supplied, however, on the basis of a further remarkable property (easily derived) of the superoperator θ and its time-inverse:

$$P_a \theta = LP_a, \quad \eta \bar{P}_a = \bar{P}_a L. \quad (6)$$

With the notation $N_o = 1 + DC$, it follows from eqs. (6) and (3) that

$$N_o \tilde{\rho}_o(t) = N_o e^{-i\theta t} \tilde{\rho}_o(0) = e^{-i\eta t} N_o \tilde{\rho}_o(0)$$

and, on the other hand,

$$\bar{P}_a \rho(t) = \bar{P}_a e^{-iL_t} \rho(0) = e^{-i\eta t} \bar{P}_a \rho(0).$$

This shows that $N_o \tilde{\rho}_o(t)$ and $\bar{P}_a \rho(t)$ have the same time-evolution, governed by the superoperator $\exp(-i\eta t)$: we may therefore equate them at any instant and in this way fix the correspondence between the dynamical and the asymptotic density. This gives the quite fundamental relation, valid at any time,

$$\tilde{\rho}(t) = \tilde{\Pi} \rho(t), \quad \text{with} \quad \tilde{\Pi} = P_a N_o^{-1} \bar{P}_a, \quad (7)$$

from which follows

$$\tilde{\rho}(t) = \tilde{\Sigma}(t) \rho(0), \quad \text{with} \quad \tilde{\Sigma}(t) = \tilde{\Pi} e^{-iL_t},$$

the answer to our last question, completing the theory.

The striking feature about the superoperator $\tilde{\Pi}$ occurring in eq. (7) is that it is also a projector, and moreover one that is time-reversal invariant: we have now a time-symmetrical subspace of the superspace in which the asymptotic part of the time-evolution, starting from any given situation, remains confined, exhibiting the features observed at the macroscopic level; whereas the irregular fluctuations occurring on the atomic time-scale are contained in the complement subspace orthogonal to the asymptotic one. That such a clean separation between the two aspects of the atomic system could be effected was entirely unexpected: it could never have been found by a study of the evolution of the system in Hilbert space, for it can only be formulated in terms of the superspace formalism. The superoperator $\tilde{\Pi}$ is not factorizable: one cannot ascribe any state vector to an asymptotic situation as we have defined it, but only a density supervector $\tilde{\rho}$.

I shall leave it at this summary sketch, except for one further point I wish to mention. Not all large systems show the normal macroscopic

behavior described by thermodynamics: how can we recognize those that do? Well, for the case of homogeneous systems at any rate, theory gives us a simple criterion: we have just to turn again to the superoperator of asymptotic time-evolution θ . It is obvious that any system for which θ happens to vanish identically will not show any irreversible tendency towards equilibrium; if the system is homogeneous, it can be seen that it will exhibit this tendency provided that θ does not vanish identically. This "condition of dissipativity" is a practical one: it can be tested in concrete cases by actual computation of θ .

I need not insist on the epistemological significance of the neat, clear-cut representation we obtain for the complementarity between the two levels of description of atomic phenomena. In particular, the discussion of the measuring process in quantum mechanics can be reduced to a few words: the apparatus being a macroscopic system, the process is described in the asymptotic subspace, where any phase relations in the initial state of the total system are wiped out (i.e. they are rejected into the orthogonal subspace)—this is the simple meaning of the "reduction" of the initial state of the atomic system resulting from the measurement. As to the human observer, his interaction with the apparatus is also entirely described in the $\tilde{\Pi}$ subspace, and therefore without influence whatsoever on whatever goes on in the orthogonal subspace.

These brief indications may serve to illustrate how the disclosure of the new formal relationships, provided that their adequacy to the physical situation is severely kept under control, can throw light on the epistemological aspects of atomic theory and bring out the deep harmony we have reached in our description of atomic phenomena.

CONCLUDING WORDS

SAMBURSKY: It remains for me to put in a closing word. I might be biased of course for obvious reasons, but I would like to say that the Symposium was a very good one, and I believe I can give a reason for that according to my standards. Because the older I grow, the more I value the human element over the intellectual element and here were both—the combination of intellectual and human elements created a climate conducive to a fruitful exchange of views. So the discussions as well as the papers were good. It is also symbolic that the beginning and the end of this Symposium were given by active scientists, symbolic of the hope that the bond between active scientists and philosophers and historians of science is on the increase, which is, I think, the only hope for science.

I wish to thank all the participants who took the trouble to give four valuable days of their life to this Symposium and who took the trouble to travel quite a long way to come here. Also I wish to thank Yehuda Elkana for his splendid idea of organizing this Symposium and for making it a success. Lastly, I hope that all of you will one day come again to Israel. Thank you very much.