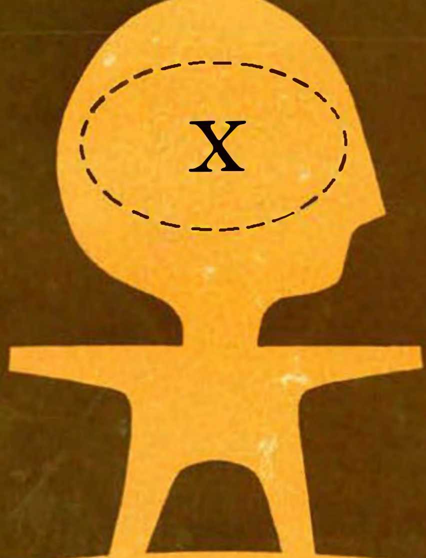
1960

The Nature of Physical Knowledge

L. W. Friedrich S.J.



THE NATURE OF PHYSICAL KNOWLEDGE

edited by

L. W. FRIEDRICH, S. J.

contributors

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INDIANA UNIVERSITY PRESS
Bloomington

MARQUETTE UNIVERSITY PRESS
Milwaukee

MANUFACTURED IN THE UNITED STATES OF AMERICA
LIBRARY OF CONGRESS CATALOG CARD NUMBER: 60-13219

Preface

PERIODICALLY, the thoughtful scientist will take pause from his research activity to reflect upon the kind of description the facts he is uncovering are constraining him to make of the universe in which he lives—indeed, even of himself. He asks himself about the quality of the knowledge he is acquiring. He asks himself about the certainty of that knowledge. Some scientists answer the question about certainty simply and devastatingly. They deny that there is anything certain about knowledge. Their skepticism renders any attempt to draw conclusions from the raw data of experiment an operation that merely increases uncertainty. There is a school of thought which likes to vent its disdain for all it considers inferior knowledge upon that much-abused and misunderstood word, mysticism.

As the physicist ponders over the insight his findings are giving him into "objective reality" he finds himself looking into his own mind, and the material instrument through which mind works, the brain. How well does this mind get into contact with things outside itself? Indeed, does it make such contact at all, or does it merely fabricate "evidence"? The chronic doubter is inclined to embrace this fabrication theory. He keeps asking himself whether he is dreaming, or insane. In so doing, is he not supposing that there is something in him, ultra-real, not depending on his senses for its knowledge, which sits in judgment over all the avenues by which knowledge comes into his conscious, or subconscious, self?

Scientists know that their research has made contributions to many areas of human endeavor. The fruits of research have made it possible for man's body to travel faster than the sound of his voice. They have lengthened life. They have entered the field of language and are making translation by machine a reality. They have come to the aid of the paleontologist to help him in the difficult task of dating his archeological findings. Can it be possible, then, that science may have an important contribution to make to our everyday deportment, the "ought" in our lives? Do the laws of nature have any bearing on the laws of behavior?

A study of the nature of knowledge demands that we inquire whether there is knowledge, or whether there are knowledges—all valid, yet different. This could lead to the further question whether what is "assumed" in one knowledge may not be an evidence in another.

Simplicity, transsubjectivity, and intersubjectivity are among the criteria accepted by scientists for testing the validity of the knowledge of science. How sound are these? Do they assume something more fundamental, which is neither self-evident nor demonstrable? If demonstration is needed, this itself becomes a subject of our concern. We need to inquire whether deductive reasoning is excessively susceptible to the fallacy of the consequent. Inductive reasoning, fruitful though it is, appears incapable of yielding conclusions that are any better than probable. Only unswerving consistency maintained throughout the performance of all possible experiments, quantitative and qualitative, on a particular phenomenon in nature could bring true certainty. Quantitatively, approach to infinity in number of experiments would usually seem to be required for absolute certitude.

The advent of quantum mechanics has compounded the epistemological problems of the physicist. He is less certain than ever about the ultimate unit of material reality, since reality seems to manifest itself to him both as a particle and as a wave. Is one of these an illusion? Is the wave in quantum mechanics no more than a probability amplitude? Have we unwittingly assigned the same objective reality to a statistical table as we assign to the events that supplied the data for the table? Is nature quantal? Is it dualistic?

These are some of the questions and problems upon which the participants in this symposium have touched directly, or indirectly. They are fundamental. They are important. Physicists want to broaden their outlook. They want to evaluate their work in terms of a broad and valid insight into all of reality. This symposium was organized in the hope that some contribution would be made toward an all-embracing view of reality, which may some day serve as a frame of reference for evaluating the scientist's concepts of the real.

Professor Henry Margenau of Yale University suggested this symposium in March of 1958 and agreed to participate in it. The Council of the American Physical Society consented to placing it on the program of its 1959 "Summer Meeting in the East" at Milwaukee. Professor Margenau's aid in organizing the symposium and in securing participants was invaluable. Gratitude is due to Professor Frank Collingwood of Marquette University, who aided in or-

ganizing the subject matter, and who graciously agreed to being moved from his initially assigned role of discussant to that of one of the principal lecturers, thus to take the place of Professor Philipp Frank, who had been scheduled as a principal lecturer but found it impossible to attend the meeting. Valuable financial help was kindly contributed by *The Milwaukee Journal* and the Falk Corporation.

Portions of Chapter 3 are used by permission of Yale University Press, publishers of Professor Henry Margenau's *Open Vistas: Philosophical Perspectives of Modern Science.*

Some of the papers presented at the meeting have been revised by their authors for publication.

It is the hope of those who participated in this symposium that it has made at least some contribution to the physicist's understanding of the nature of his knowledge, and that it has helped to narrow, even if only slightly, the gap of misunderstanding which exists between so many physicists and philosophers.

L. W. FRIEDRICH, S.J.

Marquette University
November 6, 1959

Contents

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I

The Nature of Physical "Knowledge"

P. W. BRIDGMAN

IT IS desirable to begin by trying to make more precise the meaning of our terms and the scope of the proposed analysis. As first formulated, the title proposed for this symposium was *The Nature of Physical Knowledge*. The physicist was not explicitly mentioned in this formulation. In its general usage I think the word *physical* implies something broader than physics or the physicist, and suggests all the so-called physical sciences, including physics, chemistry, astronomy, geology, and practically all biology as practiced by the biologist today. Not included in the implications are the social sciences and the humanities. The status of psychology is perhaps more doubtful, for it has aspects both physical and humanitarian. In what I have to say I shall be concerned, not narrowly with physics, but with what would by general understanding be considered the physical aspects of any of the several sciences.

The originally proposed title was subject to some discussion by correspondence with several of the participants and at one time appeared in the form *The Nature of the "Knowledge" of the Physicist,* a form that I suggested and particularly approved. This wording, however, got lost somewhere in the shuffle, and the final formulation that appears on the program is the same as the original except that there the word *knowledge* appeared unadorned, whereas in the final version it appears in quotation marks. This change was made at my suggestion in order to avoid what seemed to me the implication in the original formulation that there is such a thing as knowledge in general, and that the physical scientist is concerned with only a special kind of this general knowledge, of which there may be other kinds, such, perhaps, as the knowledge of the mystic. We here encounter distinctions which to a certain extent are only verbal, but in any event the operational background of the so-called knowledge of the physical scientist is so different from the operational background of the so-called knowledge of the mystic that it seemed to me that only confusion and misunderstanding could result from applying the same word, *knowledge,* to the activities of both scientist and mystic. I wanted to underline this situation by putting *knowledge* in quotation marks, to indicate that some clarification was necessary in the proposed usage. In anything that I have to say it is to be understood that I am addressing myself to the situations presented by the physical sciences unless the context indicates that other sorts of situations are contemplated. I should also like to stress that what I have to say in the following expresses only my personal attitude, and I have no doubt that many physicists as well as philosophers will disagree with much of it. It seems that physicists, when they talk on matters with a philosophical tinge, are no more likely to agree than the philosophers themselves.

A preliminary word is also desirable with regard to what

I shall understand to be implied in the word *nature* in our title. I shall not try to find the nature of the knowledge of the physicist by asking what that knowledge *is*. In general, whenever we ask what anything *is* we are inviting confusion by throwing the doors open to all sorts of philosophical issues about which there has been notorious disagreement for the last three thousand years. It seems to me that we shall do well to limit the scope of the discussion as narrowly as we can, and apply the principle of Occam's razor to strip away the unessentials. In particular, I want to avoid the implication of the existence of knowledge in the abstract, after the fashion of a Platonic idea, and reduce the whole matter to as concrete terms as possible. I propose merely to ask under what circumstances the word *knowledge* is used in connection with the activities of the physical scientist. If I can find the answer I shall have all that I need or can use in answering the question of what is the "nature" of this knowledge.

It is to be noticed in the first place that the word *knowledge* is used very seldom by the physical scientist or the physicist himself in describing what he does or in describing his experience. *Knowledge* is a general word, most commonly used by the outsider in describing what he sees the physicist do, whereas the physicist himself is mostly concerned with more specific situations for which he has more special words. The directions in a laboratory manual for elementary mechanics would, for example, never read, "Acquire a knowledge of the velocity of the falling body after it has fallen one meter," but would instead be, "Measure the velocity of the body after it has fallen one meter." Neither would the physicist, in describing the result of his experiments with falling bodies, say that he had acquired a "knowledge" of the law of falling bodies; more probably he would say simply that he had found the law. The physicist is, nevertheless, human, and he does on occasion use the language of daily life; and in discussing his experiences he may well say that he "knows" something or other. What does the physicist have in the back of his head when he thus permits himself to say that he "knows" this or that? There is, in the first place, I believe, always the implication of truth. If what the physicist "knows" is not true, then he does not know it. Now truth does not proclaim itself, but has to be established by some method; that is, it has to be "verified." One of the functions of verification is to guard against mistakes—illusions or faulty observation or faulty memory or unjustified inferences as to fact. There is no unique method of verification, but any operation may be pressed into service which has any bearing, direct or indirect, on assuring ourselves that the situation is as we think it is. For the physical scientist, one of the most effective methods of verification is repetition. If our observation repeats, we presume that our original observation was correct and that we may say that we are in possession of legitimate knowledge. But this method is applicable only in special contexts; that is, it is applicable only to the type of situation that can be made to repeat itself. Furthermore, there must be some way of being sure that the conditions necessary for the repetition of the situation have in fact been fulfilled. This latter usually involves some sort of theoretical understanding on our part.

Another powerful and widely used method of verifying the correctness of our report of some situation is agreement between our report and that of other observers. Physical science concerns itself by preference with situations in which verification by public report is possible. In fact, verification by public report is so important in the physical sciences that sometimes it is incorporated into the very definition, as when it is said that one of the conditions that a body of so-called knowledge be scientific knowledge is that it be publicly confirmable. This demand can generally be met in the factual situations of physics, but even here there are special situations in which public observation or confirmation is impossible "in principle." An example is afforded by situations in which the intensity of the radiation with which the observations are made is so low that we are dealing with individual photons. In some of the nonfactual situations of physics the attribute of publicity is even vaguer. However, in the "science" that gets written in textbooks any generalization that ranks as a "law" of physics has run the gauntlet of public acceptance. But such public acceptance is only the consensus of individual physicists, each of whom was individually convinced of the validity of the law by activities essentially private. In general, whenever one is concerned with a scientific "proof," one is concerned with something essentially private. Even physics, therefore, cannot be completely reduced to a public basis. In psychology, complete reduction to a public basis is not possible even on the factual level, for the behavioral psychologist is driven to recognize the existence of phenomena "accessible only to a single individual," as, for example, my toothache. If I have a toothache I usually do not feel the necessity for any sort of verification that I actually have it, so that it might appear that here we have knowledge without the necessity for verification; on the other hand, I usually do not say, "I *know* that I have a toothache," but simply, "I have a toothache." In situations as immediate as this the whole concept of "knowledge" appears merely as a verbal artifact, which may be dispensed with. But I cannot say about you, "I *know* that you have a toothache," without attempting some sort of verification. And even in the apparently most immediate case of my own toothache it not infrequently happens that the dentist tells me that the tooth I think is aching is not the one that really aches. Verification in such cases has to be by some indirect method.

Verification, whether more or less direct, is always of the logical form, "Such and such *may possibly* be the case because this and that is the case," but never of the logical form, "Such and such *is certainly* the case because this and that is the case." These situations are usually dealt with on a probability basis, and there always seems to be a factor of probability in any actual verification. For this reason no knowledge can be certain. For another reason no knowledge can be certain, for knowledge is an aspect of our activity and as such can never avoid the specter of self-doubt. There is no adequate answer to the questions, "How do I know that I am not now dreaming?" or "How do I know that I have not suddenly gone insane?" All intellectual functioning is subject to the fundamental and unprovable assumption that our intellectual integrity is preserved.

For these various reasons it is not uncommon to hear it said, "All knowledge is really only probable knowledge." We may, if we like, speak of knowledge in this way; but if we do we have to be careful, for the implication is close to the surface that somewhere there is such a thing as certain knowledge but that for some adventitious reason we are not able to acquire it. There is no such thing—it is of the nature of knowledge to be uncertain. In spite of all this, it is not knowledge either unless there is some operation for checking or for verification.

Assuming now that the physical scientist has accomplished his verification, making his operations as direct as the circumstances permit, so that he is in a position to say that he knows that some statement as to a factual situation or as to a general law is a "correct" statement, what is the significance of his verification and of his knowledge? Why did he go to the trouble of making his verification? I think that he made the verification because he wants to use his knowledge, and that the significance of the verification is to be found in the range of circumstances in which the knowledge can be used. He seldom, I think, makes the verification because he has any interest in acquiring knowledge isolated from any context. Now the range of circumstances opened for the application of knowledge by any particular sort of verification depends on the sort of verification it is and has to be determined in general by experience. The *estimation* by any individual as to what is the range of permissible use of "knowledge" that was obtained by any specific method of verification will in general depend both on the past experience of the individual and on his individual temperament. A large part of the scientific experience of the human race has been devoted to acquiring more effective estimates of the range of valid application of the "knowledge" obtained by various methods of "verification." The range of fruitful application of the knowledge that the fever of one's son was caused by the evil eye of his neighbor, a knowledge verified by the statement of the witch doctor, is estimated to be greater by the savage Hottentot than by his more experienced European contemporary. Verification by the witch doctor is not esteemed by the European as a significant method of verification, but the European arrived at this conviction only on the basis of an extensive past experience. This is an extreme example, but there survive in our own society individual differences of opinion about the significance of various indirect methods of "verifying" knowledge which have some resemblance to it.

Our discussion of the range of application of the knowledge corresponding to some specific method of verification suggests at once the specific subtopic for this paper, namely, "Can it (i.e., the knowledge of the physicist) be expected to lead to a full understanding of reality?" It would be easy to make this question the start of an interminable discussion of the nature of reality, with no possibility of agreement. I think, nevertheless, that there are some things that can profitably be said without attempting any completely general philosophical discussion. Perhaps even more than for the word *knowledge* most physicists have a temperamental aversion to the word *reality*. They avoid it whenever they possibly can. There are, nevertheless, certain things that the physicist does that have a connection of sorts with some aspects of what I imagine to be the philosophical concept of "reality." The physicist ascribes a special significance to situations in which he can make readings with instruments, and he might even reluctantly consent to saying that the readings of his instruments correspond to something "real." Those physicists who say that the electric field at a point in apparently empty space is "real" do so because if they go to the point in question with an instrument the instrument gives a reading. Besides concepts corresponding to something "real" in this instrumental sense, the physicist also recognizes concepts which he describes as conventions, but which might perhaps alternatively be said to be concepts corresponding to nothing "real." For instance, there was a school of thought which maintained that the "force" of Newton's laws of motion was only a "convention" because there was only a single definition for it, namely mass times acceleration. Here the convention would disappear and "physical reality" enter if some second alternative and independent definition of force could be framed in terms of which it would be possible to subject the statement "force equals mass times acceleration" to an experimental check. In general, when there are two independent methods of getting to the terminus, the physicist thinks of the situation in ways recalling the way in which apparently the philosopher thinks of "reality."

Granting, now, that the physicist has conceptual machinery recalling the philosopher's "reality," I think the question of our topic can at once be answered, for the physicist certainly does not expect that his knowledge will ever lead to a full understanding of this "reality." His reason, however, is special to him, and not at all what the philosopher presumptively implies by the question. I think the physicist emphatically would *not* say that his knowledge presumptively will not lead to a full understanding of reality, for the reason that there are other kinds of knowledge than the knowledge in which he deals. His reason is based on his actual experience as a physicist and has almost no recognizable philosophical component. For it has been his universal experience that never has it been possible to set bounds to the knowledge that can be obtained with instruments, but always the bounds of factual knowledge can be pushed back, both in the direction of the very small and in the direction of the very large, by instrumental advances. The physicist at present sees no indication that this process of continued expansion will ever stop, although he may believe that further progress may become increasingly difficult. Furthermore, the physicist sees no present possibility, and anticipates no possibility in the future, that it will be possible to penetrate into this new territory by any other methods than an extension of those he already uses. The method of the witch doctor is ruled out in his thinking.

The development of quantum theory has, I think, materially altered the physicist's conception of the nature of his "knowledge" and the sort of hold this knowledge enables him to gain on whatever may be meant by "reality." For he has learned that the object of knowledge is not to be separated from the instrument of knowledge. We can no longer think of the object of knowledge as constituting a reality which is revealed to us by the instrument of knowledge, but the two together, object and instrument, constitute a whole so intimately knit that it is meaningless to talk of object and instrument separately. This insight is deepened by developments on the outskirts of what is traditionally considered to be physics, developments such as the construction of complicated computing machines and the advances in our understanding of brain structure and function. It now appears that the instrument of knowledge *par excellence* is the brain, and that the nature of the brain determines and limits any possible "knowledge." Furthermore, I believe it to be the temper of the times to regard this as the whole story: given a complete description in atomic terms of the constitution of the brain (perhaps expanded to include the whole nervous system), nothing more is needed, but everything, including all the immediate data of introspection, will be found somewhere concealed in the functioning of this inconceivably complicated system. Some modification in this formulation will doubtless be necessary to include quantum phenomena, but it seems to be the present thought that any such modifications will not obscure the expectation that as complete a description as possible in physical terms will tell the whole story. This attitude is not capable of any rigorous justification, but it is not for that reason to be characterized in terms of "belief" or "faith." The attitude is rather to be described in terms of a program for action. The physiologist or the psychologist regards as the most promising program for the immediate future one in which attention is given to determining everything implied in the detailed atomic structure of the brain. There is no present indication that this program is not capable of being carried out in principle. Neither is there any other sort of program that in the light of past experience offers any present prospect of success.

Given now that the brain is the ultimate instrument of knowledge, a limitation at once appears on any possible sort of contact which such an instrument can make with any so-called reality. For it is perfectly obvious that it is impossible that there should be a unique one-to-one correspondence between what happens in the brain and the aggregate of things that happen outside it. A sufficient reason is the numerical discrepancy between the number of things that can happen inside the brain and the number that can happen outside it. If "reality" is taken to include all the things that happen outside the brain, it is so obvious that the brain is incapable of even a full description of reality, to say nothing of understanding it, that I think a physical scientist would not think it worth his while even to bother to make the point. Instead, it becomes a pressing problem to understand how the brain is at all capable of dealing even moderately effectively with the complication outside it. Whatever its method, it would seem that the brain must be forced to ignore most of what is outside it and practice some method of selection. There can be no question of any similarity or resemblance between what the brain does and what provides the subject of its activity. In fact, the concept of "resemblance" or "similarity" is meaningless in this connection. The situation is admittedly highly unsatisfactory; it is even difficult to talk about it self-consistently. But in spite of this I think that the physical scientist is convinced there is no other way. The brain provides the only conceivable instrument of knowledge, and any plausible programs of action have to be drawn with this in view, and with the clear recognition that there are many unsolved problems here.

2

Is "Physical Knowledge" Limited by Its Quantitative Approach to Reality?

FRANK J. COLLINGWOOD

MY INTENTION is to approach the subject under consideration, namely, physical knowledge, or, as I would prefer to express it, *the knowledge obtained as a result of the investigations of the physical sciences,* from what is usually called a historical point of view. But it is not that point of view in the pejorative sense of a mere recounting of events fairly well fixed in space and time. Rather, I wish to examine the reasoning involved in the distinction between the mathematical sciences, the mathematicophysical sciences, and the philosophic sciences aimed at understanding whatever there is that is knowable about material being. To carry out that intention one should start with those who first set down the distinction, because one can then not only see their reasoning on the matter but also understand the later developments in the light of that of which they were developments.

Thus, where to begin in this most important and most complicated business of truth and certitude in our knowledge of physical being is not really a matter of choice. One must begin at the beginning, the philosophic and scientific beginning when men first reflected in an intelligent manner upon what they were doing, and why. No one knows the absolutely first beginning of speculative thought, but we have discovered the period when the philosophers of our Western culture first set down what they thought and won the approval of their peers. Unless we start with them, the whole attempt to say something coherent, acceptable, and true runs the risk of being invalidated by a prior knowledge, easily available to us, but of which we are ignorant. It has often happened that a truly fine analysis of a subject matter has fallen into desuetude because of a lack of intelligent appreciation only to be rediscovered centuries later as though it were being discovered for the first time. For example, Descartes, in stumbling upon the notion of a purely mathematical science of material being, did not realize that the same notion had been thoroughly discussed in ancient Athens and had been beautifully implemented by ingenious Greek scientists, who were not unaware of the limitations of such an approach to reality. There is no instance of any one of them claiming that the mathematicophysical approach to reality is the only one.

As far as we know, from the best records that we possess of the early history of Western civilization, the first kind of knowledge that was esteemed for its exactness and correctness was mathematical knowledge. The notion of demonstrating the truth of some statement by a reasoning process is seldom mentioned in the remnants of the civilizations preceding the seventh century B.C., which saw the advent of Greek philosophy and mathematics. The highest period of development of Egyptian civilization produced a rather poor mathematics, poor because it was not developed as a knowledge independent of things but only as a practical knowledge, and especially poor because the notation was as cumbersome as that of the later Roman numerals. During the same early period of the history of Western culture, the Babylonian civilization of the Sumerian peoples developed the sexagesimal mathematics that we still use in our conventional measurement of time and in trigonometry. That civilization also produced profuse and valuable recordings of an astronomical nature. When Claudius Ptolemy produced his *Syntaxis Mathematica,* commonly called the *Almagest,* around A.D. 150, his efforts superseded all previous attempts to draw up reliable and complete astronomical tables because of the incorporation of this large group of Babylonian observations which were remarkably accurate for the time in which they were made. The Babylonians also had a rudimentary algebra which was quite innocent of any desire to prove the formulas which had been taken from a plane geometry and algebraicized.

But no thoroughgoing explanation of the varieties of knowledge is to be found in the Egyptian and Babylonian civilizations from which the pre-Socratic philosophers, especially Pythagoras, inherited so much. However, there were these mathematical and astronomical treasures for the taking. Pythagoras helped himself to them and became, to use Aristotle's phrase, the founder of mathematical philosophy. His followers cultivated the mathematical heritage received from Babylonia, and in the heat of the speculative enthusiasm peculiar to the fourth through the second century B.C., they not only developed this heritage tremendously but spared no effort to render mathematics strictly scientific by giving proofs and by isolating the postulates and axioms. But the early Pythagoreans placed a peculiar stricture upon their approach to mathematics. It is true that above all they wanted to know the reason for the correctness that mathematics gave to their calculations. But at the same time they held firmly to the notion that mathematics was a science of whole quantities. I interpret this to mean they thought that their mathematics was a science of the quantity of things. The axioms and postulates of Euclidean geometry lend force to this interpretation.[1] For this reason they would not permit themselves to talk of multiplying a length by a length because such a physical action is impossible, nor would they use numbers or symbols raised above the third power, because, to their observations, only three dimensions were evident in things and to use numbers raised to the fourth or fifth power was to be talking about no thing (nothing).

The later Pythagoreans, Archimedes, for example, had no qualms about using algebra as an algebra, irrespective of whether the things being analyzed could in fact be multiplied by themselves or divided by another, or not. The reason they felt this way, I presume, is that they saw mathematics as a science of something other than material things, although it was most useful in the mechanical science of the time. Archimedes, in trying to estimate the area under a curve, by seeking to exhaust the area through dividing it into rectangles of known area, developed a notion of the infinitesimal and of how to calculate with it that predated by roughly twenty centuries the development of the infinitesimal calculus by Leibnitz and Newton. He could hardly have reasoned so brilliantly unless he were quite sure that mathematics was primarily a knowledge of quantity abstracted from all the limiting characteristics of actual material quantities. He shows no awareness of the strictures placed upon the Babylonian mathematical heritage by the earliest Pythagoreans.

Thus a distinct transition, in the estimation of what mathematics *is,* occurred between the time of Pythagoras and the time of Archimedes. That period of transition contains within it the lifetimes of Plato and Aristotle, which is quite sufficient to make it noteworthy; but it is also the period in which the discovery of the irrational, the square root of 2, for example, demanded some thought as to just what mathematics *is* and as to how it differs from other knowledge. Plato and Aristotle appear, in my opinion, to be located in the period when the transition was occurring from considering mathematics as a science of things to considering mathematics as a science of abstractions only. This being so, Aristotle had the advantage of knowing both views and of making his choice between them. That choice and its relevance will be explained in a moment. First, let us consider Aristotle's illustrious teacher and his estimation both of mathematics and of the nature of reality.

Plato held that the mathematical sciences, although they proceed with rigor and have some certainty on that account, nevertheless are based upon hypotheses, and for that reason lack apodictic certainty. The examples that Plato gives of these hypotheses include the odd and the even, the various figures, and angles. What he called hypotheses, then, are the postulates that the odd and the even exist, and that the figures and angles can be constructed. Proof in mathematics, for Plato, means that, given some statements, others may be logically deduced from them; for example (my own example), given that there are triangular figures, the property of the contained angles equaling two right angles can be deduced by the aid of a construction.

There is no appearance of hesitation on Plato's part in making the mathematicals ideal entities: "And do you know also that although they [the mathematicians] make use of the visible forms and reason about them, they are thinking not of these, but of the ideals which they resemble, not of the figures which they draw, but of the absolute square and the absolute diameter." [2] Mathematics is not a science of existing material things, for Plato. Nevertheless, in recommending its study he gives two reasons for its importance. One reason is that the study of mathematics draws our attention to the realm of the intelligible and fits the intellect for the journey into the realm of the pure intelligibles, the realm of absolute certitudes.[3] The other reason is a practical one, namely, that mathematics, although it is not a science of sensible things, is nevertheless most useful in analyzing the universe about us.

The practical usefulness of mathematics is lauded in many dialogues, the *Philebus,* the *Epinomis,* the *Laws,* the *Republic,* the *Timaeus.* In the *Republic,* mathematical knowledge is depicted as being a requisite for the soldier in the ordering of his troops, and as enabling the musician to successfully construct in numbers the harmonies that are basic to music.[4] In fact, it is employed by all arts and sciences and forms of thought.[5] Also in that dialogue, Plato chides the astronomers for star-gazing when they should be attending to the development of mathematical equations that would be capable of accurately formulating the path and motion of the planets.[6] His point is that, in spite of the endless flux of appearances in sensible reality, sufficient stability can be brought into our management of practical things, by the measuring and ordering that the use of number effects, to enable us to conduct our practical affairs successfully.[7] Furthermore, in the celestial realm there is not the same degree of mutability that there is in the sublunary sphere.[8] As a result the heavenly bodies appear to move in regular, orderly motions which will be susceptible of description by mathematical constructions and formulas.[9]

This doctrine, that mathematics can order and bring certainty into our everyday practical knowledge, and that it will eventually bring certainty to our knowledge of the motions of the heavenly bodies, strongly suggests that sensible reality is in some degree mathematically constituted. This second notion of the utility of mathematics, which depicts mathematical forms as the models, and even as the constituents, of sensible being, is given in the *Timaeus*. There air and water are seen as binding together fire and earth in the same manner as the means in a numerical proportion bind the extremes to each other.[10] And when the Maker forms the visible universe he does so by constructing the four elementary bodies out of a chaotic matter by means of the forms of two kinds of triangle, the half-square and the half-isosceles.[11] But that out of which this material world is constructed is the ultimate in disordered chaos and so is the least subjected to intelligence and order. Even though the elementary bodies are constructed of mathematical figures, they retain much of the mutability characteristic of the primordial source.[12] How modern Plato's assessment of the mutability of matter is! Change the mathematics involved, and the whole account would be as timely as that of any contemporary book on the philosophy of science.

Naturally, knowledge in terms of the elementary bodies and of composites formed from them will be no more than mere opinion. That is to say, for Plato, knowledge in terms of the affections that sensible becoming (there is no being, and therefore no intelligibility, in the sensible realm precisely as sensible) gives rise to in our senses is impossible. Opinion alone is possible concerning this realm. Any proof or demonstration apart from the mathematical is out of the question here. There can be no knowing of the substantial nature or of the properties of a material thing. It is futile in Plato's philosophy to talk of any scientific knowledge through the causes of material being. Consequently, the royal road to truth is not through the investigation of sensory phenomena; that way leads to doubt about even those things of which he at first seemed so certain.[13] The royal road to truth for Plato is in thought and through thought to the Good in itself. In this ascent to the Good, it is mathematics in its pure form, that is, considered apart from sensible things, that first satisfies the intellect's avid desire for the intelligible and thus serves as a steppingstone to the realm of pure intelligibility, which is attained not by any deductive process but by an act of vision on the part of the intellect. Once this vision is attained, once the essential nature of reality is discovered, then the material universe will be understood as it is derived from the spiritual universe.[14] This goal, set out for attainment partly by speculation of a mathematical kind and partly by pure intuition, did not prove to be as appealing to posterity as it was to Plato.

I would not wish to overstate Plato's position on the use of mathematics. Nowadays, when the utility of mathematics as a practical science is beyond question, there is a temptation to look back to Plato and to see in him the person who successfully championed the use of mathematics in the elaboration of physical science. This is not exactly the truth. What he praised was the study of mathematics as an intellectual exercise and as a study which aided in obtaining the vision of the Good. Thus he championed each of the four branches of mathematics, numbers, geometry (plane), stereometry (solid geometry), and astronomy, in the same manner; that is, while allowing that they were indispensable in practical affairs, nevertheless he actually praised them only inasmuch as they were helpful in obtaining knowledge of the Good.

One might more correctly credit Aristotle as being the man who championed the use of mathematics in the elaboration of the sciences of things. He too considered astronomy and harmonics, as well as the other sciences universally recognized as mathematical, as being branches of mathematics. But he also added optics and mechanics to the list of *the more physical branches of mathematics.* In these four areas of investigation the knowledge of the proven fact is given by the mathematical astronomer or optician or musician or physicist. By the use of mathematics such men give proof. In these subjects, "It is the business of the empirical observers to know the fact, of the mathematicians to know the proven fact; for the latter are in possession of the demonstrations giving the causes, and are often ignorant of the fact." [15] Aristotle probably means, by the latter part of his statement, that they are not aware of all the possibilities of using their mathematics in the analysis of things. Thus, because natural bodies contain points, lines, planes, and volumes, they manifest quantitative aspects and so admit of treatment by mathematical theorems. Geometry knows only the abstract form of the straight line, and this form has no existence in itself. The straight is found in stones and wood and in all bodily things, even in air; it cannot be isolated physically, but only mentally. Whereas the geometer considers the straight apart from things, the student of optics considers straight lines in the air.[16] Thus are the applied parts of mathematics distinguished from the speculative parts.

As for speculative mathematics, the pursuit of the study of the mathematicals in themselves, Aristotle explains what Plato had left undiscussed. Plato had maintained that the mathematician uses the visible forms in order to think about the mathematicals. But his explanation of the origin of the mathematicals is unsatisfactory. Aristotle holds that the mathematician investigates abstractions. "For before beginning his investigation he strips off all the sensible qualities, e.g., weight and lightness, hardness and its contrary, and also heat and cold and the other sensible contrarieties, and leaves only the quantitative and continuous, sometimes in one, sometimes in two, sometimes in three dimensions, and the attributes of these qua quantitative and continuous, and does not consider them in any other respect, and examines the relative positions of some and the attributes of these, and the commensurabilities and incommensurabilities of others, and the ratios of others." [17]

The abstractions are representations of the quantified aspects of material things. In order that the purely quantitative aspects of such things may be grasped more clearly, their other features are left out of consideration. Thus, by abstraction, the features not relevant to *quantity considered in itself* are left aside, and the remaining content of cognition is then analyzed. Aristotle made no claim to being a mathematician, and he made no contribution to the development of Greek mathematics. Therefore in his explanation of mathematical abstraction we are not surprised to find only the most elementary notions used as examples. The basic notion of quantity is that of the continuum, the extended-in-three-dimensions. By subtraction (in thought only), a continuum in two dimensions, and a continuum in one dimension only, are isolated. The Greeks defined solids, plane surfaces, and lines in this manner. The possible arrangements of these would include all the figures of plane geometry. The commensurabilities would yield whole numbers and proportions. Thus, a smaller length taken three times would measure a greater length. The two lengths are said to be commensurable because they have a common measure.

Although these notions are very elementary they serve to indicate the nature of mathematical abstraction. Over and above being the intellectual process that gives stability in thought to the notion of continuum, abstraction is a process of *subtracting* various quantitative aspects of that conceived continuum. Consider for the moment only three of those subtractions. Think of them quite apart from the abstractive process necessary to attain to any object of scientific analysis. The first subtraction takes away all colors, sounds, odors, etc. It leaves an utterly blank continuum having only two characteristics, size and shape. The second subtraction deletes the various aspects of size and shape. In one case depth is omitted, in another width, in a third length, leaving an absolutely featureless continuum, which could not be imagined, but which could be thought of as empty space. It would have to be defined as a mere possibility of extension.

The third subtraction leaves aside the characteristics of quantity as it is found existing in actual quantified things. It substitutes symbols that are appropriate for reasoning about quanta that have a very indeterminate character. These quanta are as near to pure potency as the mind can conceive. With these symbols unknown quantities can be reasoned about. For example, a ratio can represent the comparison of a length to a length: one side of a table is to another as 4 is to 3; or it can represent a comparison of areas; or it can represent simply a ratio of numbers; or it can represent a ratio of unknowns, a/b as b/c. Although Aristotle did not carry his analysis beyond arithmetic to the consideration of a purely symbolic science, he nevertheless saw why mathematics was useful in ordering and measuring and calculating about things. It is because the abstractions are far more wieldy in thought than the quanta are in actuality. Corresponding to the orders and relations of quantities that exist in mathematical systems is the possibility of approximately the same orders and relations existing among actual quantities. It is not the precision and exactness of mathematics that are chiefly responsible for the practical usefulness of some of its parts. Actual quantity is an attribute of matter, and it shares the vagaries and mutability of all material things. Therefore, it cannot be measured and analyzed with the absolute precision that is characteristic of mathematical analysis. There is no such thing as an exact measurement of actual quantities; measurements are only as exact as our measuring devices make them. A mathematical yard contains exactly 36 mathematical inches, but no such accuracy can be found in measuring the lengths of a yard and its inches in actual quantity.[18]

So long as a mathematics treats of that which has some basis in things, no matter how slight that basis may be, the actual order in the mathematics may be sufficient to bring to light the hidden order in the realm of quantity in sensible things. This is the factor in practical mathematics that makes it successful. The agent in act in such cases is the human intellect perfected by mathematical knowledge and possessed of the incommunicable knack of seeing where the possibility of application lies.[19]

But, going beyond Aristotle's elementary teaching on abstraction, why is it that mathematics is based upon hypotheses? If one starts his investigation with sensibles, he accepts them as given and then proceeds. He knows why he starts there; it is because he has something given whose being present he does not need to question. He does not say, if there are sensibles then I define them as such and such. The sensibles are, and their definitions are contained in their very being. The problem of the investigator is to analyze such being into its definitions which are either speculative or operational depending on what the aim of the investigator is. But if one starts with symbols which represent subtractions from natural things, which do not mirror their qualities, nor their actual determinate shapes or sizes, nor any of their powers or dispositions, the possibility of finding among such symbols an object of knowledge, as absolute and as independent of the thought of man for its existence as is the sensible universe, is reduced to the vanishing point. And mathematics is fundamentally the art of manipulating symbols of this kind, whether they be lines and surfaces, or the unknowns of algebraic systems, or whatever. There is a minimal being remaining to the symbol after the abstraction of all determinations so that its nature is close to being a mere possibility. If one says, let *p* stand for any instance of anything, and let *q* be in some manner different from *p;* then it is evident that both *p* and *q* are in the highest degree indeterminate. The manipulator of the most abstract of these symbols has the power to give an intelligible content to his symbols which the symbols do not yet possess, actually, but which they are capable of receiving. The restrictions upon this human creator reflect only his purely human capabilities intellectually and his dependence upon the sensible universe for meanings to be given to the symbols. His dependence upon the sensible universe is reflected in this that he considers as valid operations with symbols, operations that he sees in matter, or in his own mental acts apprehending the material world. Thus addition and subtraction are operations, for manipulating the quantities of things, and such operations are allowed in the realm of symbols. The if-then notion is based upon concomitances discovered in nature; the notion of "function and variable," if it is really different from the "if-then" notion, is based upon concomitances discovered in the variable aspects of nature; and so on for the remainder of the operations, disjunction and the like. The one exception that occurs to me is negation, for its analogue is found not in nature, nor in apprehension, but in judgment.

If this analysis, of what mathematics in its simplest form is, be correct, then one would be very surprised indeed if it started with anything other than hypotheses. One might expect a purely formal science, such as abstract mathematics or symbolic logic, to be based upon the most certain foundations, as this would seem to be consistent with its abstractness from the realm of mutable matter and with its formal clarity. But, upon investigation of the nature of symbol itself, upon seeing its neutrality to intelligible meaning before it is used, one clearly sees that any operation performed and any meaning given to symbol will be at the whim of the artificer who wishes to construct a meaning in symbols.

I have endeavored to show that the very nature of symbolic reasoning is such that it has to start with hypotheses because of the abstraction from meaningfulness that is involved in obtaining the symbolic notion. But this does not explain why the physical sciences too are based upon hypotheses. It is not enough to say that they make use of mathematics as an instrument and that the science suffers from the imperfection of the instrument. This is true in the sense that mathematics is an ideal science dealing with straight lines, or curved if you prefer, plane and curved surfaces, regular functions, and the like, which do not occur in nature in the way in which they are defined. This causes difficulty, but not anything insuperable to human ingenuity. The basic difficulty in the attempt to find absolutely certain first principles in the physical sciences lies in the starting point that must be adopted in the practical investigation of sensible being. To have scientific certitude one needs a knowledge of cause. In the physical sciences the search has been predominantly for material causes, the ultimate constituents of things. Although there have been efforts to formulate formal causes, the manner in which the constituents are arranged and interrelated, they have usually taken the form of laws of action. Perhaps formal causes are inexpressible in any other way than by listing physical properties and describing activities. Whatever the case may be for formal causes, the pursuit of ultimate material causes has been unabated since the four-element theory was laid to rest by the analysis of combustion. But even today the ultimate material causes still elude us. As a result, knowledge in terms of the basic material components of being is not yet possible. Therefore the attempt to explain in terms of ultimate material causes is thwarted by our failure so far to discern them. The only available substitute for the desired proofs regarding the ultimate nature of physical reality is hypotheses about physical reality. Thus the modern sciences of nature supplement their descriptions of the visible world with suppositions as to the ultimate nature of both visible and invisible material being.

The two obvious restrictions upon physical knowledge as possessed by present-day sciences of matter are reliance upon symbolic instruments in thinking about things and the use of hypotheses in place of knowledge of the actual ultimate material elements. Does it follow then that, if there is this uncertainty about the facts of physical being, the being that we are most readily acquainted with, the same uncertainty will necessarily be found in any other knowledge that attempts a correct account of creation? To rephrase my opening question: Is "physical knowledge," which is limited by its quantitative approach to reality, the only knowledge of reality?

To answer the question I should like to present a brief analysis of Aristotle's account of both knowledge and reality. Knowledge on both the sensory and intellectual levels is the production within our consciousness of a representation of something. In the representation the thing is "seen" under some aspect or other; for example, sight sees it as being colored and shaped, and intellect goes beyond this, seeing it as being a self-existent or an accident. It is this process of "seeing" what is enduring among the changeable aspects of things that enables one to obtain something enduring in knowledge.[20] The something enduring in knowledge is information of what a thing is primarily, or for the most part. Such knowledge endures even when that of which it is knowledge does not endure. For conceptual knowledge to be true it does not have to conform to the mode of reality possessed by that of which it is knowledge. In fact, the kind of reality that is being considered at a given moment, whether it be a thing apparent to the senses, or a hypothesis, or an ideal such as a theory, will be thought over and apprehended by the intellect without there being necessarily any reference to the status of the thing being known. This latter kind of knowledge, of how what is being thought of exists, is attained by intellectual judgment. To understand the indeterminacy principle, or the ergodic hypothesis, is not too difficult; but to judge truthfully whether they are valid and to what extent they are valid is another and more difficult matter, requiring the decision in advance as to what criteria are to be used in deciding what mode of existence is possessed by an object known. The one criterion accepted by the layman is this: If the thing being thought of is one that I can have sensory awareness of in my waking state, then it has more to it than simply my thought of it; it is a thing. This is the criterion used in everyday knowing. Unfortunately it will not work for Professor Bridgman; his remarks imply that he cannot tell whether he is awake or asleep.

Some things are evident, and some are not. When one is fully conscious that he is seeing a colored thing, he takes the coloredness of the thing to be evident to sight. He never attempts to prove what is evident; only what is not evident demands proof. Other things over and above sense qualities are evident; for example, quantity and change are two other fully discernible aspects of the universe. The relation between the color seen, and that in things which gives rise to it, is not evident and so provokes an investigation both of things and of sensing. Similarly, the connection between minimal quantity and the nature of a material thing is not evident and requires investigation. When such investigations turn up the truth of the matter, the truth is called scientific. This truth, as was previously mentioned, can be expressed in quantitative terms or it can be expressed in nonquantitative terms. When it is expressed in nonquantitative terms it lacks the approximative precision and accuracy that the use of measurement gives, but it is certainly true knowledge. If I pass light through a diffraction grating and attempt to measure the path of the particle, or wave, that travels from the grating to a screen behind it, undoubtedly I am dealing with measurables: distance, frequency of light, and extent of fringes on the screen. But the question as to the path of the individual whatever-you-call-it of light is saved from being utterly ridiculous only by the fact that I, and any other observer who cares to, "see" light entering the grating and light appearing on the screen. The unverifiable assumption that it is the same light that strikes the grating and that strikes the screen afterward causes me no greater concern than the unverifiable assumption that the sunlight that caused my sunburn is the same light that left the sun eight minutes earlier.

Philosophers like Aristotle have been of the opinion that it is better to start with what is evident, and from that vantage point to proceed to investigate what is obscure. The precision of an approximative measurement is not required for such a procedure; nor are the assumptions that there is gravitational *force*, and that the *velocity of light is constant*, prerequisites to knowing that water flows down to the oceans and that the sun warms things. Knowledge of this latter kind is presupposed to all science of whatsoever type, quantitative or nonquantitative. If absolute precision were a prerequisite for truth, only speculative mathematics would be true. But speculative mathematics is obviously based on assumptions (the use of natural numbers, 2, 3, 4, etc., excepted). To hold that absolute precision is the essential prerequisite for truth would put us in the peculiar position of holding that man, who cannot make a single thing (he can only rearrange what he finds), can, in elaborating mathematical systems, make the only truth there is.

All sciences, including the philosophy of science itself, have had to avoid the exclusive use of merely quantitative expressions in order to talk of qualities and activities which are more than the measurements that can be made of them. I can, by using terms standing for qualities, quantities, substances, etc., tell what mathematics is; but no mathematics can tell what it is. Nor can any mathematics tell what a substance, or a quantity, or a quality, is. In fact, a hasty induction from only ten books purporting to tell what mathematics is (written by authors who understood mathematics, I presume) left me with the opinion (not very approximative, I am sure) that mathematicians, integrated into one spokesman who resolved all their opinions, could not tell anyone what mathematics is. But enough of this raillery. Mathematicians must know what they are talking about; how else could they disagree?

Since mathematics, when used by physics, is only a method and not an explanation, it can be dispensed with to some extent when a true explanation is achieved. For example, if force did exist as the cause of the orbits of the solar planets, their orbital motions would be understood in terms of it. Kepler's laws would add nothing to this basic understanding. They would add something to the knowledge of *how* the planets follow their orbital motions, but would add nothing to the *why*. Speculative physical science has very little need of mathematics in expressing the qualitative and substantial nature of things. Mathematics is an instrument of analysis; it is not a constituent of things. Only the perverse way of looking at things that is characteristic of positivism could hold that an instrument of analysis is more real than what it analyzes. But no one could possibly believe in positivism any more!

Aristotle held that knowledge of the quantitative, qualitative, and activity differences of material things gives knowledge of their forms. The essential form of a thing is defined as that factor which determines the actuality of a thing. It is not a sophism. It is a way of speaking that corresponds with reality. For example, the reason why a tree and a cat appear to be different to us is that the basic elements of both are formed in a different manner. If you put a portion of both through qualitative analysis you would find carbon as a constituent of each. But it does not seem reasonable to say that both the cat and the tree are carbon compounds having inexplainable differences in appearance. It seemed reasonable to Aristotle to say, in the beginning, that, whatever the ultimate constituents of things turn out to be, those things that appear to be radically different are *formed* differently. Thus, for him, the essential form of a thing is that which accounts for a thing's being the kind of thing that it is.

When a tree or a cat is reduced to its elementary parts, there is no point in searching for the original form. The act of killing and dissolving needed to get at the parts has destroyed the organization characteristic of a whole tree and a whole cat. Although no form is isolable this does not mean that forms are mere devices to explain an organization of matter that we do not understand. We do not understand this organization, but it is obvious that matter is organized. The term form is used to indicate this fact, and it is indispensable to both our everyday and our philosophic vocabulary. For sciences whose greatest concern is the isolating of material causes, it can be dispensed with in matters of analysis, but something like it, terms such as "organic whole," "synthetic whole," have to be used to express the togetherness of parts that is characteristic of actually existing material things.

If human knowledge could grasp the essential form of a thing it would understand all the characteristics of that thing. Obviously no one can say of any material thing that he understands it completely. Qualities and activities are signs of the essential natures of things. To banish these from discourse because some aspect of them is measurable is to make thought easy at the expense of making it empty. Although the reduction of qualities to quantity has often been attempted and has often been claimed successful, the simple fact of the matter is that no quantity can be known, either directly or by instruments, without its being known by means of qualities that have an effect upon our senses. The universe is quantified, but that is not all that it is. It exhibits qualities that are constant—the orange color of copper, for example; and without these qualities, sense qualities, and the activities of material things, nothing would be knowable. If the qualitative aspects of things by which we know them are erroneous, then so much more so are the quantitative aspects, for they are known only through the senses which detect qualities primarily and quantities only secondarily. If you deny the reality of quality, you must deny the possibility of knowing objective quantities.

To conclude, things are quantitative and qualitative and substantial. A knowledge of things from one of these points of view, the quantitative, requires the elimination of the ordinary qualitative aspects, the color, the sound, etc., that are associated with it. But to banish these from consideration in order to have a nice clear-cut precision, from the purely quantitative point of view, is not to banish them from reality. They will remain and will still inform us of "what things are up to" after the present distress over the upsetting effect to the latest physical theory is long forgotten. Remember, a practical science, in order to be successful, does not need to know what things *are;* it only needs to know what they are likely to do, or, failing that, what we can do to them. But a speculative science, and the philosophy of science and metaphysics are such, must have reasonable certitude before it can progress; and in the realm of matter such certitude is difficult to obtain. The philosophy of science, it seems to me, must accept this account of things, and, directing its attention to the kind of thing that mathematics is and to the difficulty involved in the analysis of matter, must attempt to get an over-all view of the quantitative-investigation field and say something intelligent about what is going on in it. It does not belong to it to say anything about matters that are not clearly in its domain, unless in talking about such matters it takes full cognizance of the viewpoint by which they are to be properly apprehended. Everything involved in speech is capable of being analyzed quantitatively except the way in which its reception by hearing affects the intellect of the hearer. No quantitative analysis can predict this effect, or measure it, or explain it away. In the beginning there was *mind,* and *matter,* and *forms,* in Aristotle's language; they are still here challenging us to know them, more and more fully, and forbidding us to exclude either matter or any one of the various forms, quantitative, qualitative, etc., from a true account of things.

3

Does Physical "Knowledge" Require A Priori *or Undemonstrable Presuppositions?*

HENRY MARGENAU*

1. *The Logical Forms of Scientific Demonstration*

To KNOW, as the title of my discourse requires, what is meant by an undemonstrable proposition, it seems necessary to achieve clarity and perhaps agreement with respect to the meaning of "demonstration" in physics. The word is used in a great variety of contexts. It denotes methods of conviction or persuasion ranging all the way from deductive mathematical proof to incidental exhibition of specific items of evidence. Literally, it means "showdown," and its meaning centers in the presentation of crucial or striking *sensory* confirmation of a proposition.

I shall first accept the wider sense of demonstration, allowing the word to stand for any experience that has a large measure of suasive power or cogency relative to a physical proposition, supposing, however, (*a*) that the experience is of the direct or perceptory type (not merely the

* Aided by a grant from the National Science Foundation.

recognition of logical or mathematical consistency) and (*b*) that the proposition is sufficiently general to be called a hypothesis or a law or a theory. Under these conditions one encounters two large classes of demonstrations in a science like physics. The first may be called inductive or, better, *correlational;* the second, deductive or, for reasons that will soon be given, *exact.*

Let Boyle's law be such a proposition. To demonstrate it may mean making a very large number of measurements of the pressure and the volume of a gas at a given temperature, showing that they all very nearly satisfy the law. The logical situation here is this. The experimenter has obtained n values for the pressure P, and n corresponding values for the volume V. Except by an unwarrantable and logically illicit extrapolation, these $2n$ experimental values cannot establish the belief that in every possible measurement, past and future, P equals c/V. What they do imply is that the *correlation coefficient*, k, of the P_i with the c/V_i is very nearly 1, and this entails, via laws of induction which are progressively being clarified,[1] that future observations will satisfy Boyle's law with a computable probability, a probability that is a function of both n and k.

Here then is the character of an inductive demonstration: it changes n into $n+1$, increasing the probability in question. The psychological force of an inductive demonstration, however, is enormously greater than its logical force. If a student to whom the law is a novelty sees a few positive instances of the correlation between P and $1/V$, he is greatly impressed and takes the demonstration as final proof in the same vein and with the same satisfaction as a proof of Pythagoras' theorem. This is partly valid, for the probability is raised from 0 to a value not far from 1 by only two or three positive instances, whereas many further confirmations cause it to crawl toward 1 very slowly; it is partly wrong, because the novice mistakes the element of surprise for cogency.

For the more advanced student a demonstration of Boyle's law requires a great deal more. He will think of the law as *implied* by, or as a special case of, a more general proposition called the perfect gas law, or as a consequence of the equation of state for real gases, and he will see even this in the framework of the more embracive kinetic theory or statistical mechanics. Having already confronted situations which led him to accept the validity of the laws of particle mechanics, and regarding the passage from particle mechanics to statistical mechanics as a simple and reasonable one, he thinks of the analytic consequences of that theory as *true*, and his *a priori* expectation, when confirmed by a very small number of positive instances supporting Boyle's law, engenders in him an assurance concerning the outcome of future experiments that is far beyond justification by the inductive probabilities just mentioned. The point is that the coherence of the logical texture in which the proposition to be tested is embedded produces its own evidence, and this evidence makes reliance on correlations less severe and less important. Never, of course, does a physicist dispense with empirical confirmation, nor can a theory create empirical data out of purely rational ingredients—Eddington was, in my opinion, quite wrong methodologically when he suggested that the constants of nature are reflections of tautological human procedures. What the physicist entertains here is healthy respect for the positive feedback that takes place between purely inductive evidence and the *a priori* expectation that flows from the logical entailment of a given statement by more general propositions already confirmed. In practice, he couples correlational demonstration with deductive demonstration. A science that has attained success and stability between these procedures is called *exact,* and for this reason I have labeled the deductive method itself exact. Let us study it more carefully.

Its use amounts to what is ordinarily called an explanation, a feat which is characteristic of the deductive process and has no meaning in any correlational pursuit. Explanation starts with some very general affirmation, such as is contained, for example, in Newton's laws together with the ergodic hypothesis. Let me call this set of premises S_0. They alone do not imply Boyle's law. Hence, one introduces a set of further assumptions of a less general sort concerning which there is some empirical evidence. They may include the supposition that the forces acting between individual molecules are additive, or central, or indeed zero. These suppositions will be called S_1. From the conjunction of S_0 and S_1, theorems $T_1, T_2 \cdots$ can be derived by logical procedures; among these theorems are the general gas law and its special case, Boyle's law. But as a theorem the statement is still indefinite and empty, for it merely contains the symbols P and V whose reference to observation needs to be inserted. It is at this place that operational definitions [2] enter, and by their intervention empirical manipulations can engage the symbols in concrete fashion, leading to a climax which logicians call confirmation or disconfirmation.

$$\text{In symbols: } S_0 \cdot S_1 \supset (T_1, T_2, T_3 \cdots)$$

One of these theorems, say T_i, functionally relates P and V. P and V in turn are connected to numerical values P' and V' by rules of correspondence [3] of which operational definitions are a special and important class. If P' and V' are found in observation, T_i is said to be demonstrated and to be true (with certain reservations). And if all T_i are confirmed, preferably many times and by numerous observers, S_0 and S_1 are demonstrated.

Some may feel that the word *demonstrate* in this connection is ill chosen. I confess to some sympathy with this sentiment, because the manner in which the S's are verified is rather indirect and lacks the "ad oculos" quality expected of demonstrations. Nevertheless, no more direct way to ascertain the truth of abstract propositions is available, and if the word *demonstrate* is to have any significance at all with respect to such general principles as the basic laws of mechanics, of thermodynamics, or the ergodic hypothesis, which enter into our example, it must reside in the transition from S to P' and V' which has been sketched.

Accepting this meaning of demonstration, we ask what measure of certainty it confers upon S_0 or, to be more specific, upon $S_0 \cdot S_1$. In common language, $S_0 \cdot S_1$, henceforth simply written as S, is called the explanation of, or the reason for, Boyle's law. Every explanation in science is an act of logical inclusion—a chain of reasoning that allows a particular proposition known as a fact to be seen as the consequence of a more inclusive set of propositions. There is often a series of explanations, as in the case of gravitational motion, where a "fact," like the fall of a stone, is explained by Galileo's "law" of constant acceleration; this itself can be explained in terms of Newton's law of universal gravitation, and *this*, once more, as a special case of Einstein's law of gravitation. Here we have to stop, for at the present stage of physics there is no more general theory which yields Einstein's (or some other, perhaps more successful) formulation of general relativity as a deductive consequence. A proposition forming the logical starting point of an explanatory chain is called a *postulate* or, as a carryover from the days when first principles were regarded as indubitable, an *axiom*. In our example of Boyle's law the chain has but a single link; at least, one may think of it in that way. Strictly speaking, the number of links is not countable: as in all deductive situations, one can interpose between first premise and final conclusion an arbitrary number of intermediate though usually uninteresting steps. Here, for simplicity, we shall regard the passage from S (basic principles) to T (Boyle's law) as one.

The demonstration in question has this form:

$$\begin{array}{c} S \supset T \\ T \\ \therefore S \end{array}$$

Every student of elementary logic will at once recognize in this conclusion the famous form of the fallacy of the consequent. One is really not entitled to affirm S if its consequent, T, is true. The physicist knows this, too, for he is aware of the circumstance that Boyle's law may very well be also the consequent of postulates quite different from S, perhaps not yet discovered.

The history of science is full of instances where accepted implication relations of the form $S \supset T$ though still valid have been abandoned as part of science. This may happen for several reasons, among them the following. Sometimes, later, more refined experiments prove a given consequence of S, T, false. In this case S usually has to be changed. This change is uninteresting from my present point of view, for it could have been effected even if the reverse relations were true, i.e., if T implied S. The usual case, however, is this. Further experimentation shows that T remains true, but new observations become possible, observations expressible, let us say, in the form of a different theorem T'. Now T and T' are quite compatible since they deal with different sorts of phenomena, but T' is usually not implied by S. If the relation $S \supset T$ were reversible, so that $T \supset S$ and $T' \supset S'$, S and S' would contradict each other, and we should be developing a kind of physics in which each set of phenomena must be explained by its own set of theories, simplicity and cohesion being lost. It is the *ir*reversibility of the implication relation, $S \supset T$, that saves the day; for T and T' can both be implied by a different and wider set of postulates. That irreversibility, however, forces science to affirm the consequent.

Hence follows the important methodological result that physics can never be certain of its postulates. This is the price it pays for its dynamism, for its facility of self-correction, for its impressive rate of growth. And as for deductive demonstration, we see that it, too, can never reach certainty of its premises.

Yet the lack of certainty encountered here is altogether different from that which afflicts correlational demonstrations. They could be expressed in terms of probabilities. By probability the physicist means a relative frequency in some well-defined ensemble. Such an ensemble is available when Boyle's law is tested empirically: one can clearly specify and observe the relative frequency of volume measurements falling into a range about V_i when the pressure has a value in the neighborhood of P_i. But what about the relative frequency of a theory (postulate, hypothesis) S?

It seems to me, in view of the practice of scientists and in view of the logical situation just discussed, that a search for a "probability index" of theories is unprofitable. We do not speak of theories and postulates as probable or improbable, but as correct or incorrect relative to a given state of scientific knowledge, or perhaps as approximations to a more exacting theory, either known or not yet known. In applications of a theory we make allowance not for probabilities of hypotheses but for errors of numerical results. There is no ensemble of theories in which favorable and unfavorable ones can be counted, and this is because theories, like ideas, are not subject to arithmetic; two theories may be one, or many, or indeed none if they are contradictory. Hence we conclude: *Inductive* or correlational demonstration involves uncertainties capable of numerical test as probabilities. *Deductive* demonstration of theories involves a different, intrinsically logical kind of uncertainty which arises from the inevitable fallacy of the consequent inherent in it. To distinguish them, let us use the names inductive and deductive uncertainty.

2. *Meta-Principles of Science*

Deductive uncertainty means radical freedom of choice in the construction of hypotheses. For, while a given finite set of empirical data (e.g., P_i' and T_i') allows the calculation of a most probable or "true" set of values for these quantities, no similar unique method for specifying a "true" hypothesis or a most probable hypothesis in the face of the data (P_i' and T_i') exists. The absence of uniqueness is especially serious for the kind of terminal hypotheses called postulates, and it arises quite clearly from the presence of "fallacies of the consequent" in the chain of entailments connecting postulates with observations.

The agreement among scientists with respect to acceptable explanations remains therefore an astonishing historical fact so long as only logical concerns are allowed to govern our inquiry. When the view is shifted to the actual practice of physicists, however, a new and highly revealing element emerges: the deductive uncertainty is held in bounds by important habits of reasoning, by pre-empirical commitments to certain forms of theory—in short, by factors not imposed and frequently not even suggested by the facts themselves. Philosophers have spoken of them as categories of thought, as razors that shear away irrelevancies of explanation, and as injunctions enforced by a *lumen nat-*

urale or by divine revelation; physicists have used phrases like economy of thought, simplicity, and elegance of explanation, in describing them. Whatever the name, their analysis is of importance at every stage of science. In today's physics they clamor for attention with greater vehemence than ever because a major point of controversy, the so-called causal interpretation of quantum mechanics, involves precisely these items of transempirical commitment; to seek the solution of this problem in the field of data and of detailed mathematical analysis, as is sometimes done, must be recognized as a misguided and futile endeavor.

In a traditional sense of the word metaphysical, the paralogical, nonempirical principles affecting the choice of hypotheses should be called metaphysical. To avoid misunderstanding, may I say that I do not include in that word the ontological suffusion of absoluteness which it sometimes carries, or the self-assurance made notorious by thinkers of Deussen's [4] school; my precedence lies in the usage of principled philosophers like Kant, to whom metaphysics meant (in part, at least) the theory of scientific knowledge with its primary task of elucidating the way in which such knowledge is made acceptable and objective. And by nonempirical I simply mean procedures that have a character apart from the coerciveness of observational experience, even though they have no business and no significance without such experience.

An extended survey of the metaphysical principles has been conducted elsewhere.[3] Terms like logical fecundity, extensibility of constructs, the requirement of multiple connections among constructs, their permanence, causality, simplicity, and conceptual elegance were employed to suggest a spectrum of functions which these requirements perform. To my knowledge a complete logical analysis of this vague assemblage has not been made, nor is it certain that it would be fruitful or that it could be achieved. There is perhaps an advantage in the restraint which leaves these principles unanalyzed, for any complete logical structure, once established, produces a stability, a rigor often close to stagnancy, and the principles under consideration, if viable, are in flux.

This last admission shows at once an insufficiency of the Kantian doctrine, upon which the present study leans. For Kant, the categories, which may be regarded as the forerunners of the metaphysical principles under inspection here, were eternal forms of thought, wholly of *a priori* origin. The history of physics since his day belies this allegation; it shows that the principles are pragmatic devices of great scope, established by an impressive crescendo of scientific successes but never exempt from careful scrutiny and modification or, indeed, rejection. We have seen what has happened to causality in our time, and nobody can guarantee the quality of simplicity in theories dealing with nuclear forces, although we still hope for it. Despite this concession of mortality, however, one cannot fail to be astonished by the longevity of metaphysical principles: their lives are reckoned in millennia, whereas physical theories nowadays live decades, and facts may die in months.

Suffice it here to show briefly how the principles in question [5] operate on the contemporary stage of physics. As our first example, we choose the history of the neutrino. It started out as a metaphysical gleam in Pauli's eye, springing from the hope that the constructs of nuclear theory, in particular of beta emission, might prove consistent (logically fertile) and extensible. But questions arose: Is the neutrino an insular construct, or is it multiply connected? In particular, are there rules of correspondence which give it empirical status beyond the demand for consistency in the face of conservation of energy and momentum? The issue here was not one of direct sensory confirmation, which was ruled out by the very qualities assigned to the neutrino itself; nobody expected the particle to be seen or to manifest itself in cloud chambers. Physicists felt uneasy because there were not *enough* connections between the postulated entity and other constructs, connections which would make a difference in the empirical domain. The principle of multiple connections has now been satisfied, even though the neutrino has not been seen directly.

The negative proton was expected on other grounds: it had to exist if nature is symmetric. Here an esthetic requirement enters the scene, something, perhaps, that comes under the heading of elegance. In a way it contradicts simplicity and shows that different principles sometimes compete in application. The force of the symmetry postulate was great, inducing theorists to incorporate the negative proton in their calculations and to predict how it might manifest itself in experiments, all according to the pattern $S \supset T$ with appropriate elaboration. The metaphysical principles generated S; empirical verification established T.

These brief allusions, which can hardly portray adequately, or do justice to, the interesting play of metaphysical principles on the scene of present physics, must suffice here. Some of the most fascinating problems arise because we defy positivism and believe in them; otherwise we should not be worried about negative matter, geons, chronons, hodons, or, for that matter, the elusive plasma oscillations.

We have said that metaphysical principles often suggest general laws S; empirical verification then establishes a T which is implied by S. This is the pattern of many discoveries, and it stands in sharp contrast to the accidental or shotgun method of science which probes the facts in assiduous and never-tiring fashion with the hope that new discoveries, unrelated to present theory, will suggest new understanding. Actually, science is a two-way transaction which flourishes when equal emphasis is placed on both approaches. We are perhaps in danger today of overstressing the shotgun attack, chiefly because ideas promoting elegance in theory are rare and money for research that turns up stones to see what is under them is plentiful. But to return from a digression. Our examples indicate that metaphysical principles are as important today as they have been throughout the history of physical science.

However, they never act in isolation. As has been more carefully set forth [8] elsewhere, after constructs are selected in accordance with metaphysical requirements, they are then tested by an establishment of "circuits of empirical verification," and only those constructs are retained as valid, i.e., as verifacts, which are embedded in a network of successful circuits. It is the neglect of this verifying phase of scientific method that accounts for the pathology of Deussen's metaphysics.

Our journey into the territory of scientific method has now brought us to an elevation from which the subject of this article takes on added interest, or at least added complexity. Should we count the metaphysical principles as undemonstrable presuppositions? Are they *a priori*? Does physical knowledge truly require them? The last question I should now affirm definitely, and offer in evidence the preceding excerpts from recent physics, in addition to uncountable examples of the past. The other two questions can hardly be settled so easily, because they are subject to different possible interpretations, and we must return to them. First, however, it is well to deal with some challenges and afterthoughts that have come up in connection with the list of metaphysical *principles* as published.[3]

Professor Frank [6] suggests that scientific method is highly sensitive to cultural and social factors, and presumably he would accord to them a rank comparable with causality, simplicity, and so forth. Hence we should inquire whether our list needs to be enlarged, and whether it should not contain, besides the present socially neutral items, certain factors linking the basic procedures of science with the structure of society or even the political system in which science is practiced.

Then there is another point that should be discussed. To be objective and generally acceptable, it is often claimed, scientific truth may not be private truth. It must be public, inasmuch as every normal person with adequate training can acquire that truth. This view equates objectivity with intersubjectivity and insists on communality of demonstration. Again, one may ask whether here is not an important principle to be added to our earlier list. The next section is devoted to these matters. Section 4 deals with an allied problem, posed by the growing insistence with which some physicists demand the resolution of quantum-mechanical probabilities into mechanistic artifacts, or hidden variables, and the like. The interest that attaches to this last "principle" justifies our treating it in a section by itself, although its status is not different from that of the others.

3. *Compatibility with Cultural Norms and Communality of Evidence as Controlling Factors of Physical Knowledge*

It is impossible to deny that moral and political considerations affect the course of science, and Frank offers a number of examples to prove the point. Our own time presents ample evidence for it. Hence the occurrence of such influences need not be argued and cannot be questioned. What matters here, however, is whether a patent historical connection between morals, religion, and politics on the one hand, and the methods employed by science on the other, justify the inclusion of a *requirement of compatibility with cultural norms* among the regulative principles of science, coordinate with logical fertility, simplicity, causality, and so forth. Do we in fact rely on our conditioning by the cultural milieu in the same way as we rely on simplicity of explanation?

First, let it be admitted that cultural conditioning, while present, is also inescapable. It is a commonplace. One cannot think thoughts, scientific or otherwise, for which training has not prepared one. Language determines in large measure our forms of reasoning, and it may well be that principles like causality, which operate with states and systems that are doubtless descendants of Aristotle's attributes and substances, are historical consequences of the subject-predicate relation dominating Indo-European speech. The influence of political systems, though less direct and affecting mostly the rate of growth of science, is likewise inevitable, since what can destroy life can also destroy science. And here we approach the main issue. Is cultural conditioning to be compared with the metaphysical principles we have listed, or does it belong in the same class with other commonplace factors that also affect science, like the training we receive, the language we speak, the friends we have, and the food we eat?

I should class it among the latter and therefore refuse moral and political considerations an acknowledged regulative role in the formation of scientific postulates and hypotheses. To some extent, this is shutting one's eyes to reality in the hope of keeping science clean. But there are cogent reasons in favor of this stand. For even those who practice political, religious, or moral disciplining of science deny that they are doing it; they still insist that they base their reasoning on "objective" grounds. Moreover, the accounts that inform us of the incidence and the inevitability of such moral-political mortgaging of truth themselves never fail to convey a measure of condemnation of this very circumstance, thus showing that a distinction between good and bad regulative principles is fairly universal. Finally, and this is the most interesting and the most telling point, history somehow *exposes* instances in which scientific theory has been influenced by nonscientific considerations and proves them wrong, ill-conceived, or diversionary; science heals the flaws that political conditioning sometimes leaves in the wake of its advance. For all these reasons, it would seem that affinity of science with culture cannot be demanded as a metaphysical principle commensurate with the others.

The story is different with respect to communality or intersubjectivity as a controlling factor of scientific method. Here we encounter a requirement which is wholly operative and acceptable, and which might indeed be added to our former list. It was omitted because, I feel, it follows from the others.

Science is certainly an intersubjective, a communal affair, so far as that is possible. Every experience, if it is to be relevant, must be sharable or communicable; isolated experiences, *if they can be repeated*, do not count. Some isolated experiences, however, cannot be repeated. Astronomical observations of the past, a measurement of the velocity of light in 1910, are admitted as scientific evidence when cosmological theories perhaps involving a variable velocity of light are to be tested. It is seen, therefore, that repeatability by other persons is a kind of *secondary* requirement which is sometimes waived. Dependence on the testimony of others can be contradicted by the equally scientific attitude of individuals who will accept no evidence save their own. And there have been mass hallucinations.

Nevertheless, scientific knowledge attains its stability by relying on the experiences of many. But this, it seems to me, must be regarded as a special consequence of the more basic requirements of logical fertility and multiple connections, already cited. For a construct that is not communicable, or one that is communicated but not believed, is clearly sterile. Hence the requirement that scientific constructs should be logically fertile, coupled with the fact that there are other people, at once implies communality of knowledge and belief.

Again, if a construct is to be multiply connected, it cannot reside merely within that island universe which is my personal experience. It has to make reference to the universes we call other minds, and if the connection fails, it must be rejected. This requires intersubjective agreement.

Perhaps the more basic concept of *ob*jectivity needs discussion at this point. Objectivity in science may mean two things: first, transsubjectivity, or the kind of documentation that gives a personal experience an internal stability, a coherence with other personal experiences which causes it to be accepted as valid. To speak more simply, when I see a spot of light in the dark sky and wish to interpret it as a star, I first make sure that it is steady and does not change its color periodically, like an airplane. If I am in company, I ask whether others see it too, but this is by no means essential. Any real doubt as to the trustworthiness of my visual sensation can be dispelled by reference to other experiences within my private possession: noticing whether the light appears where according to my knowledge there ought to be a star, watching its progression across the sky, seeing whether it reappears tomorrow. All these are acts of internal checking, of probing the consistency of my total experience when this item is added to it—in short, of employing the validating metaphysical principles (not including communality of knowledge) together with empirical verifications. The result is objectivity in the experience of seeing the star, objectivity attainable through procedures involving only private probings; it engenders a certainty that transcends the subjective sensation; it generates transsubjectivity without intersubjectivity.

The last quality, also often identified with objectivity, is what I have called communality of experience. It is a corollary to the other, to me primary, form of objectivity and arises automatically when consistency of understanding (the principle of multiple connections) is extended to those verifacts within private experience which are called other persons.

4. *The Requirement of Spatial and Temporal Abstraction*

Since Descartes, physical science has flourished by using a method he perfected. It involves the abstraction, through physical intuition, of ever smaller and finer elements from the spatiotemporal continuum. Greek science was wary of it, having been impressed by Zeno's paradoxes. Modern science has wrestled with the implications of this method in the difficult mathematics of continua; in physics, however, it did not follow the lead very far, for it invented the concept of an atom, which is intended as a ban upon the ultimate application of abstraction. To be sure, the ban was not taken altogether seriously; physicists continued with a good deal of success to probe the interior of the atom, only to find other atoms. The latter, however, take on increasingly perplexing qualities which make men wonder whether the method of spatiotemporal abstraction is being misapplied.

The style of analysis under discussion goes by other names. The Germans call it *anschaulich.* De Broglie identified it specifically with *clarté Cartesienne;* writers looking for hidden variables call it *causal.* What it seeks is invariably a determination of the spatially and temporally large by the small, and it does not shrink from infinitesimals. Our question is: Are we committed to this sort of quest; should this tendency be set down as a metaphysical requirement of physics?

Let it first be acknowledged that there is nothing intrinsically absurd in such a principle, indeed that it appears at the same methodological level as causality, though it is wholly different from it. If it is to be rejected it must be on the grounds of inadequacy to the actual state of science. Now, it seems to me that such inadequacy has been clear and present since the beginning of the century.

Perhaps most spectacularly, every annihilation process involving quantized observables defies ultimate spatiotemporal abstraction. Consider pair annihilation. Electron and positron necessarily have the rest mass m as long as they exist. This mass is converted into energy of photons, of amount $2mc^2$. How does this conversion take place if it is to be understood under the requirement of temporal abstraction? The masses must gradually melt away into photon energy, in contradiction to everything we believe. Hence, *if* temporal abstraction *must* be imposed, the transformation can only be sudden or catastrophal, and this, I take it, is actually a surrender of the method of spatiotemporal abstraction. Atomic physics presents a host of similar examples.

The defeat of abstraction had already been suggested by Bohr's early quantum theory. If an atom has truly quantized states, then the "passage," i.e., the temporal transfer in which our method forces us to believe, is not only mysterious or difficult to understand (as physicists often said); it is logically impossible. The atom can only be in one or the other of these two states; to say that it is in both at once acknowledges the fiasco of the method. Physics has, of course, given it up in this instance. Yet its desire for continuity was strong; [7] continuity could not be found through ordinary spatiotemporal abstraction, hence physics seized upon a variable that remains continuous when simple abstraction fails and introduced *probability*, the probability of the atom's appearing in one of the quantized states upon measurement as distinct from its *being* in them. One might say that probability saved continuity at the price of spatiotemporal abstraction.

The very notion of classical causality, if taken seriously, requires renunciation of the method. Whatever causality may mean in detail, it suffices here to regard it as a relation between states of a physical system which allows the prediction of future states on the basis of its present state. Suppose that an electron is a region of space filled with substance, a fluid, perhaps, to which we cannot in principle deny the possibility of complex motion. To be sure, Lorentz and Abraham got away with simplifying assumptions which gave it rigid structure, but these could at best be only first approximations. In fact, they opened the door upon an infinite regress, a never-ending abstraction of spatial domains inside the electron, each with its own causal destiny and its effects upon the others. There is no assurance that simple hydrodynamic models would work in such a situation, and the prospect is an infinite proliferation of presumed observable features which would belie predictability. Hence, ultimate spatial abstraction must be renounced *to save causality.*

With all these indications I find it difficult to concede to the tendency under review the status of a methodological requirement. True, it had the semblance of success and still makes major claims, but progress has apparently overtaken it. This conclusion also shows what attitude one should take toward attempts to interpret quantum mechanics in terms of spatiotemporal abstraction: they *may* be worth while, for science does occasionally reverse its trend. But if its progress is steady, they have two strikes against them.

5. *Conclusions*

Our quest for the meaning of demonstration in physics brought us face to face with two types of uncertainty inherent in all confirmatory procedures: If the procedures are inductive the conclusion is only probable; if deductive, they are subject to the fallacy of the consequent. In the latter case, helpful regulative principles, here called metaphysical, serve to minimize or limit the uncertainty; recognizing this important fact we reviewed the metaphysical principles and their function in recent physics. And we permitted ourselves a lengthy digression into problems of the determination of method by cultural and political concerns, intersubjectivity, and geometric abstraction—a digression that was meant to bring an earlier analysis up to date. We now return to the original question: Does physical knowledge require *a priori* or undemonstrable presuppositions?

Stated with rigor and circumspection, the answer is this. If allowance is made for the irremovable uncertainties residing in the results of demonstration, if we do not ask for more than what our analysis has proved attainable, then physical knowledge requires *no* undemonstrable presuppositions. If we ask for certainty, for explanations that are unchangeable and eternal in our search for physical knowledge, then *all* of it is based on undemonstrable propositions.

The use of the term *a priori* is a little troublesome, though natural, in this connection. Its classical sense, made famous by Kant, can hardly be maintained today.[8] If it is nonetheless adopted, the required metaphysical principles are half *a priori*, half *a posteriori*, the former because they come epistemologically (though not genetically) before observational experience; the latter because they are born pragmatically from experience and change with use. To the extent that these principles are *a priori*, then, it must be affirmed that physical knowledge requires *a priori* presuppositions.

Our first answer, however, which bespeaks the intrinsic uncertainty of all scientific knowledge, has an aspect of triviality, and one gets a feeling that it misses what this symposium is about. Granted, you may say, we can never be certain of all details of scientific objects, as for example of the *properties* which present understanding assigns to an elementary particle; nevertheless, our belief in their very *existence* is not founded on undemonstrable presuppositions. There is after all a vast gulf between belief in the existence of electrons and belief in the existence of gods, and it is this difference that our discussion intended to examine.

On the plane of principles, metaphysical or otherwise, I can see no difference between gods and electrons. The no assurance that simple hydrodynamic models would work in such a situation, and the prospect is an infinite proliferation of presumed observable features which would belie predictability. Hence, ultimate spatial abstraction must be renounced *to save causality.*

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On the plane of principles, metaphysical or otherwise, I can see no difference between gods and electrons. The demonstration of one involves the same logical movements as the demonstration of the other. There may be a difference in the degrees of success when the final necessary appeal is made to empirical observation. But the widest disparity in this comparison appears when one examines the specific manner in which the metaphysical principles are applied to one instance and to the other. Again, the requirement of multiple connections is the decisive issue. The construct electron is rich in relations to other valid constructs; it is pervaded by many validating circuits of confirmation, is a pivotal link in many chains of physical reasoning, bears numerous correspondences to the plane of perceptions. The concept God, at least in some of its connotations, is almost insular, devoid of the fullness of relations with other verifacts that are enjoyed by physical concepts. This is certainly true if it is posited, as it often is, merely for certain kinds of satisfaction which do not culminate in verification.[9]

The second and perhaps more meaningful answer to the title question, then, is available if one reconstructs the question slightly and puts it as follows: Does physical knowledge require *insular constructs,* that is, ideas, postulates, or concepts inaccessible to demonstration and unconnected with other demonstrable constructs? The answer to this is clearly no, and the reason lies, perhaps paradoxically, in the metaphysical presuppositions of science.

4

Does "Knowledge" of Physical Laws and Facts Have Relevance in the Moral and Social Realm?

GEORGE P. KLUBERTANZ, S.J.

ANY significant discussion of morality must be placed in the context of the contemporary discussions. You will forgive me if I summarize briefly the recent history of ethical theory. At the beginning of this century the British philosopher G. E. Moore examined the work of his predecessors in ethics and maintained that all of them had fallen into what he called "the naturalistic fallacy." [1] According to him, instead of investigating and clarifying what it is that men ought to do, they gave accounts of what men were doing. They confused, he said, "is" and "ought." In his view it was impossible by an examination of the facts to arrive at any moral criteria or any moral judgment. He maintained that the knowledge of the good was obtained only by intuition. No other process could ever reveal to us what good was or what a particular good was. By "intuition" he meant a direct and immediate knowledge that neither could be proved nor needed any proof.

Next on the field of ethical theory came the movement which is sometimes called logical positivism. In their examination of ethical theory the positivists agreed with Moore that it was absolutely impossible to derive an "ought" from an "is."[2] But, in addition, they attacked Moore's intuitionism, which they declared nonexistent.[3]

Hence, in their first treatments the positivists flatly maintained that all moral propositions were simply nonsense. The recent successors of the positivists, namely, the British analysts, have not taken such a rigid view. Instead they have maintained that so-called ethical propositions are really not statements of fact but statements of attitude. According to them, "This is good" means, "I like this," "I approve of it"; and it may or may not imply, "I want you to do the same."

In this country, ethical theory and ethical practice have been discussed repeatedly by pragmatists and instrumentalists. What is common to both these theories is that the good is defined "as that which is useful, successful." Let us look a little more closely at the statement, "The moral good is that which is successful, or, leads to further goods." If we expand this statement in terms of the meaning of good which it contains, then we must say that the moral good is that which leads to further goods which are goods because they lead to further goods—and so on. Now, when the very meaning of the term is thus cast into an infinite regress, the basic meaning of the whole set of propositions disappears.

Many American philosophers attempt to escape the meaninglessness of such an infinite regress by using social norms (social approval, or the generally accepted standards). But, as the British analysts have pointed out, it is one thing to say, "The society in which I live approves of certain actions"; it is another to say, "I ought to pay attention to what society approves of." The second proposition cannot ever be justified by an appeal to society itself. For this would be a completely circular justification, and would amount to no more than saying, "Society approves of my following what society approves of."

As William James had already seen, one possibility would be to shift the meaning of usefulness or success from the objective outcome of the action to the subjective relationship. Now, if we take pragmatism in this broad sense—as allowing the meaning of good to consist in the satisfaction of personal wants and desires—then indeed we avoid the infinite regress of meaning, but we seem to be reduced by another route to the same situation in which British theory finds itself.

It would seem then that right and wrong consist ultimately in nonrational, inexplicable, and even groundless likes and dislikes, for there is no knowledge process by which ethical theory and moral facts and obligations can be ascertained. Does this entail the end of any meaningful discussion of moral problems? The analysts seem to have accepted this conclusion, to have allowed rational discussion only about the *logic* of moral propositions, not their content. If this were the final word, my assignment would be simple enough. To the question, does physical "knowledge" have any relevance in the moral realm? I should have to answer, In principle, none; in practice, if people should happen to agree, such knowledge has a secondary function of clarification—or something like that.

In common with many philosophers [4]—and, I think, with most nonphilosophers—I believe that there is a rational way to talk about the moral good. I do not intend to offer you yet another ethical theory to compound the confusion, but rather to suggest a way in which we can combine the proven conclusions of the various theories into an acceptable and successful way to explain the nature and the knowledge of the moral good. I should like to ask for one limitation on our considerations and discussions: namely, that we should *not* begin with particular moral problems on which there is widespread and violent disagreement. I propose instead that we restrict our discussions to a few fairly simple cases on which we can safely presume that all of us agree—the evil of lying, or that of deliberately causing pain without any need or reason; or, again, the positive obligations of telling the truth in science, or of alleviating the pain of others. In such examples we can successfully investigate our real knowledge that there are obligations and consider how this knowledge has been won. From this, we can then draw a number of general conclusions that will help us to answer the general question: What is the relevance of knowledge of physical facts and laws in the moral and social realms?

The first step in such an analysis is to determine the meaning of *moral good.* In the light of the philosophical discussions we have just reviewed, two points become clear. First, the good is neither a thing in itself, nor an absolute inhering quality of a thing. When, for example, a mountain climber is asked why he wants to climb mountains and he says, "Because the mountain is there"—this, obviously does not answer the question. The mountain *is* there, it is tall and rugged, and so on. But, no matter how we amplify these physical descriptions, in no way do they answer the question: Is the mountain a good?

Moreover, the same is true of any *action.* Yet, is not an action of helping another a good? This is a crucial instance. An action of helping another—this action has a physical reality. It consists, let us say, in bringing food to a person in desperate need of it. What is done can be observed, described with care, analyzed in terms of various forces and movements; in short, an entire factual description can be given of the action taken simply in itself. Is there anything in the course of this description which makes the action good? Or, perhaps, is the whole action good even if none of the parts are? It would seem that we cannot say so. To show that this is not the case, we can put it into different conditions. For example, let us consider the father of a family whose child is in immediate and desperate need. This man is at the point of giving the necessary food to his own child. At the moment when he is doing so he sees before him a stranger in equal need. If he simply leaves his child to starve and goes on to feed the stranger, is his action still good? I think at this point all of us would hesitate. We would be inclined to say, "It is no longer good to abandon one person to save another; at least, it is not clearly an obligation."

This second case can lead us to see why we call an action good. We do not consider an action good because there is either in some element or in the whole of the action considered in abstraction a physical quality which we can call goodness. To this extent we must agree with the criticisms directed against the naturalistic fallacy. It is simply not possible to consider *good* as if it were some kind of absolute quality. But we need not therefore have recourse to some nonphysical quality. The equally forceful criticisms by the positivists of G. E. Moore's intuitionism precisely go to show that good cannot be a nonnatural quality either.

The action we are considering is good only when viewed in relation to the total situation. Now, from this we should be led to see that good is not an absolute quality of any one thing. Can we then say that good is a *relational* attribute? Let us see whether this will help us in our two previous types of cases. With regard to *things*—the mountain is not good in itself if it is viewed simply and nonrelationally. But if, for example, it is viewed as a challenge to a mountain climber, then it is good *for someone*. The *action* of helping another is not good considered simply in itself. But it is good *for some one* who does have food to give to another.

But have we not thereby fallen into the pitfall of emotive ethics? "The good is what I like," and since what I like is a purely arbitrary choice, then, the moral good is a purely arbitrary consideration that can be disregarded. Or, if we take account of it, it is purely private. There cannot be any objective agreement; and, if agreement must be found, we shall have to look elsewhere—to legal coercion or social conformism.

This would be a sorry escape indeed. But is it necessary that we have recourse to some kind of arbitrary, whimsical choice, or unfounded and baseless desire? There is another way which is both more obvious and more realistic. Let us begin again with things. Quite likely we can grant that the desire to climb mountains is somewhat personal. In fact, to the extent that is a merely personal response to the mountain's presence, most men would be inclined to say that it is not moral at all. There is lacking in it any kind of necessity. On the other hand, if we turn from mountain climbing, which is entirely free and arbitrary, to something necessary, for example, food, the case is different. Food also is not good in itself as simple and absolute being; food is good *for someone*. But food is good for someone not merely because he has an arbitrary desire for food. It is good for someone who has a need for food. This need is not arbitrary, conventional, artificial, subject to free choice. Nor are we concerned with whether people like to eat or not. It is, of course, true that most people enjoy eating; but the goodness of food does not lie in its relationship to the pleasure that is usually experienced. Neither do biologists discover that organisms need food solely by examining all organisms and then drawing a kind of rough generalization. They are able to say: an organism needs food, because it has a certain structure and a particular kind of material. In other words, the need for food is natural to living things. It is therefore correct to say that we can have some knowledge of a *nature*. By the term *nature* I mean the internal constitution of a thing—like a man, a maple tree, a monkey—which is the source of its constant specific tendencies and of the activities proper to it. A nature is relatively permanent; it is manifested by activities and is logically connected with them. To speak of "human nature," for example, is not merely a useless way of restating the original facts or a verbal trick adding something to the facts. A man is a man even while he sleeps, that is, while he is *not actually* performing some of the activities peculiar to man. Nature, then, is a real, concrete, structured possibility of acting.

Granted that man's nature is not the same kind of nature that we would find if we were dealing with gold or silver or other chemicals, it remains true that something can be said about human nature.[5] Consequently, at least in principle, the term to which "good" is referred can be investigated with some hope of success, some hope of objectivity, and, consequently, some hope of agreement (though there have been very considerable disagreements not only about what human nature really is but also about the very possibility of knowing human nature objectively).

This is not a question of grasping by intuition, by some mysterious immediate process, the entire nature of man; we all recognize that such knowledge is not within our grasp. Nor is there question of any *a priori* approach, any sort of preconceived or analytically given notion of nature. We can find out what man is in the same way and only in the same way as we find out what an organism is.[6] We have to study organisms and analyze their behavior. Similarly, from a study of what men do we can find out what man is. The analysis of human nature is, however, not a statistically summarizing process. We do not simply observe man in all possible situations, then summarize, generalize, abstract, and arrive at a set of universal descriptions. We find involved in the various activities of man some basic characteristics common to a whole line of activities, no matter how much those activities may differ in other respects. For instance, we do not have any *a priori* notion of rationality. We can only find out what rational behavior is by examining what men do—including ourselves. Sometimes men perform actions which are reasonable; sometimes they act stupidly. Now, we do not strike an average between the most intelligent behavior—for example, that of a scientist, a statesman, a great artist—on the one hand, and the most stupid, ignorant, superstitious, irrational behavior on the other, and say that human nature has as its characteristic behavior something between the two extremes. We observe human actions, and we find in them, for example, an adaptation of means to ends. Whether it is really an adaptation of suitable means to proportioned goals, or whether it is an entirely blind combining of means which leads to no success at all, in both extremes we find the effort to adapt things and actions for purposes. This is the kind of way in which we can analytically study what men do to find out what man is. Similarly, we do not find out either that man is free or what freedom is by averaging together a bit of compulsive behavior and a bit of fully deliberate rational choice and then say this average is what freedom naturally is. Rather, by examining carefully various kinds of behavior we discover in some of them the exercise of freedom, and from this conclude what man *can do* if conditions are ideal. The work of some contemporary anthropologists confirms and in some respects clarifies the notion of human nature that we derive from analysis and reflection.[7] Not only man's merely organic needs but also his strictly human needs—the need for love, for friendship, for association, for development, opportunity, scope for freedom—all these things can be established to be real and true human needs.[8]

Next, is the simple natural relation of hunger and food itself a moral one? We would of course recognize that animals also need food and that food for a hungry animal is good. Would we be inclined to say that the animal was behaving morally? We would indeed say that it was behaving naturally. Though some philosophers may have maintained that animals behave morally in seeking food, I do not think the majority do so. There is still something lacking. If morality is a peculiarly human quality, then something about the relational situation which we qualify as morally good must be peculiarly human. It is not peculiarly human to need food, but it is peculiarly human to have a recognition of this fact and to be able to judge how it is best fulfilled. What raises the merely natural good to the level of a moral good is its submission to reason. That is morally good which bears a relationship to a natural need or tendency inasmuch as this relationship is judged by reason and its use is guided by reason. The moral good is a reasonable good. What do we mean by qualifying an action as "reasonable"? We mean that it is not haphazard, random, arbitrary, merely impulsive, but is justifiable. We mean, positively, (*a*) that it is taken in relation to the entire context of the objective situation, especially in relation to its consequences, and (*b*), on the subjective side, that all man's tendencies be considered in their essential order, not just one in isolation.[9]

Moreover, reasonableness in action can be objectively, impersonally judged; it is neither subjective, nor a conventional, customary norm. For example, biologists and doctors have criteria which, although they have been arrived at by an examination of particular cases, are not merely an average or a conventional standard. The amount, or quality, or kind of food that the human organism needs is not arrived at by examining the eating habits of all the races of men in all their cultures and then extracting an average which is named the reasonable amount; much less does the doctor judge by what he himself eats. If a doctor wants to give a patient a suitable diet, he considers such factors as weight, state of health, type of activity, availability of foods, and by means of these considerations arrives at a quite impersonal and objective judgment.

At least at many levels it is possible to see that one use of a particular physical thing or action is reasonable and another not. Thus, let us examine the action of telling the truth as compared with that of telling a lie. There is an external action of communicating through words or other external signs what is in a man's mind. As the other term of the relation, there is a man who has knowledge, a power of communicating, and a need and desire to communicate with his fellow men. Now, what is the reasonable way to use communication? Note, we do not say that a man must talk or otherwise manifest his mind; [10] but, if he engages in communication and at the same time chooses to do so in such a way that he does not manifest his mind but rather something contrary to it, then the action is unreasonable. If, then, lying is unreasonable, it is a moral evil. On the other hand, telling the truth is a reasonable use of an external action corresponding to the need and tendency of man and judged by reason to be suitable in the concrete circumstances, as well as guided by reason; therefore, it is morally good.

Now, with all this in mind, we can intelligibly approach the proper topic that I have been directed to examine: Is there any relevance of physical laws and facts for the moral and social realm? To the extent that such laws and descriptions clarify the nature of the object or action that man is thinking of performing, they are relevant to morality. Let us again take a simple example. Ordinarily men proceed on the assumption that those foods which are tasty are also healthful, and in many cases simple sensory response is a sufficiently good criterion. There is no question and no need for any more elaborate knowledge of the object. But it is unfortunately true that this criterion is not always applicable. Ptomaine poisoning, for example, is not discoverable merely by the taste of the food. Here a scientific knowledge of what ptomaine poisoning is, how it is caused, and how it can be prevented is of great use and indirectly enters into a moral consideration. If man is to use food reasonably, the food he eats ought to be such as not only to be palatable but also, and principally, healthful, nourishing. Food that in spite of its appearance and good taste is not healthful ought to be avoided by a reasonable man; therefore, it would be immoral to use, sell, or give it to others. To the extent, then, that scientific techniques are sometimes necessary to find out which food is healthful or unhealthful, even in the simple decision about the right use of food we may be obliged to make use of complex scientific knowledge. This example can be applied to a very urgent contemporary case, the effect of fallout on certain foods. The physical knowledge possessed only by carefully trained scientists is nevertheless a matter for everyone's moral consideration, and must influence everyone's own individual moral choices.

The same case can be made to illustrate the bearing of physical knowledge on social decisions and actions. Society is concerned with the common good, the common welfare, the welfare of all. Now, suppose that someone is doing something which has as a long-term result the extensive poisoning of the food supply of an entire nation. The intrinsic character of the real sensible object cannot be discovered by immediate sensory response, but instead needs very elaborate means. Yet society, through its responsible authorities, is obliged to take scientific knowledge into account in allowing, controlling, or stopping the action which is causing these effects. To illustrate with another very pertinent case. Only a scientist can give an adequate account of the likely effects of nuclear warfare, which are beyond the grasp of the ordinary untrained person. Nevertheless, what the physicist can establish to be the probable effects of extensive nuclear explosions can change our moral judgment about war and the testing of nuclear weapons.

But this case, obvious as it seems to be, can be misinterpreted. First, it is not precisely as physicist that the physicist proclaims the evil of nuclear warfare, if he does, but as a man who has in addition to his human judgment the advantage of much more knowledge of relevant facts. Second, the harm to human life, great as it is, is not an absolute evil. The reason is that physical life and physical health are not absolute goods. As we have seen earlier, for a *moral* good, there are needed not only a suitable object and a natural tendency but also the judgment of reason. Now, a preservation of life or health which involves a denial of truth, a betrayal of good, a surrender of freedom, is no longer to be chosen automatically. Instead, reason must weigh the two sets of consequences—their importance for a truly *human* life, the probability or certainty that they will occur, and so on. And, in the light of these considerations, it could be that the right moral judgment would be to risk the physical evil to preserve the rational good.

Next, we must ask: What about the relevance of physical *laws* to moral judgments? From one point of view, scientific laws are general propositions that state relations between *classes* of facts. As such, they have the same kind of relevance for moral judgment that particular facts have. There is some difference in that particular facts are part of the present situation, whereas laws are more likely to enter into moral judgment as giving us knowledge of future consequences. But, since consequences are merely future facts, they are not *moral goods* in themselves, any more than present facts are. Furthermore, since the facts, taken absolutely, cannot be moral, the laws relating them are not moral laws. As Professor Margenau said, scientific truth is not established by, nor to be judged by, the effects of the *use* of that knowledge upon human beings. A proposition, a law, or a theory is not true *because* it is useful, or desirable, but, by the same token, because it is true it is not necessarily good.

Scientific laws, however, are usually more than just empirical generalizations. Usually they are refined according to the requirements of theory, and so we must look for a moment at physical theory.[11]

According to the terminology I am using, scientific theory is at a higher level of generalization than law, connects laws rather than facts directly, and is deductively related to these facts. By this I mean that, if we grant the theory, the laws and facts follow as logical consequences. In relation to facts and laws already known, theory stands as explanation, as intelligible ground or reason. For an acceptable scientific theory this is not enough; theory must be "fruitful": it must suggest new experiments hitherto unthought of, whose outcomes, predicted by the theory, constitute its "verification." For this reason, many scientists, though not all, admit the legitimacy of theoretical elements in the structure of science.

As most contemporary philosophers of science agree, theory consists of rational constructs. These "products of the scientific imagination" are by no means arbitrary; their form is controlled by the facts and laws, and they give consistency and connection between more particular laws. Hence, theory can be true or false; or, if we are squeamish about this terminology, theory can be correct or incorrect. It is fashionable in some quarters today to suggest that such remote propositions are statements only about ourselves as observers rather than about the things. Granted that this is sometimes so, I believe that an interpretation that makes this necessary *in principle* is at variance with the whole spirit of science. It seems to me that the basic attitude of science is realistic. But, also, scientific truth is not simply that of a descriptive or historical proposition. *A fortiori,* the truth of a theory is not that of a naive isomorphism. The truth of a theory is defined by the laws from which it arises and the new results to which it leads.

For these reasons, theoretical concepts no longer express the direct intelligibilities of things and activities. But it is concrete things and activities with which moral judgments are concerned. So, the presence of harmful radiation in food directly affects my moral judgment concerning my use of that food, or my activities which might increase such activity. But, for example, the interior structure of an atom, the nature of "distances" between intra-atomic particles, the presence or absence of parity, the amount of angular momentum, seem to have no direct influence on human action and moral judgment, important as they are for scientific theory and even scientific experiment. Indeed, it is hard to see how propositions like this could be thought to impinge upon the sphere of moral behavior.

True, there have been writers who have claimed for scientific theory an impact far greater than what I have allowed for. It is sometimes said that human freedom would be in conflict with determinism, that the human mind is incapable of knowledge beyond the scope of sense experience, that the human being is so much a part of nature as to be one with it, that morality is so interwoven with superstition that it must be abandoned wholesale, and so on. To my way of thinking, such propositions are not scientific but philosophical, and so not necessarily connected with any scientific law or theory—certainly not of the physical sciences.

In fact, even the life sciences develop theoretical constructs which are not directly significant for moral judgment, even though they are intimately connected with what is distinctive about moral behavior, the nature of man. For example, the harm caused by an uncontrolled use of drugs is discoverable apart from any explanatory account of how drugs affect personality. Nor do we judge the evil any differently when we know that there are psychological factors which bring a man to the use of drugs. This is not to say that we do not judge the drug addict differently; we may well decide that drug addiction is a disease rather than a moral fault. But, fault or disease, we still consider it morally wrong to allow a pusher free access to our school playgrounds. And this is the point I am trying to make.

In summary, a science which is engaged in the discovery of facts and laws and the construction and verification of theories to explain them does not have the *logical* possibility of making moral judgments. But because moral good is a relational attribute which implies two terms: the thing or action in itself, and the natural need or tendency of man, any factual knowledge, including the scientific, of either of these terms in their mutual relation does have relevance in the moral and social realms.

5

Dualistic Pictures and Unitary Reality in Quantum Theory

ALFRED LANDÉ

1. *Critique of Dualism*

ERNST MACH once observed: "There is no cause nor effect in nature; nature simply *is*. Recurrences of like cases exist only in the abstraction which we perform for the purpose of mentally reproducing the facts." To which one may add: the mental reproduction of facts in the form of a methodical schema, or law, or theory must of course be checked for its empirical adequacy. And it ought to be evaluated also under the criterion of simplicity or economy of thought. Under these criteria *determinism* has been found wanting and has been replaced in modern quantum theory by statistical law. But, again, paraphrasing Mach: nature as such is not quantal or dualistic; nature simply *is*. Although there is no question as to the empirical adequacy of the half-corpuscular, half-undulatory quantum formalism, I doubt whether the present dualistic ideology approaches the ideal of simplicity, with or without the copious literature trying to make dualism more palatable to the ordinary mind and to the physicists themselves. It would certainly be preferable to have a unitary theory, either resting on a continuous concrete substratum supporting waves with occasional corpuscular appearances, or assuming discrete particles as the *real* constituents of matter which occasionally produce the *appearance* of waves.

Here I must ask your indulgence for using the words "real" and "apparent" in the same ordinary sense as when a stratified layer of clouds is said to *appear* like a continuous train of waves, yet upon closer inspection is found *in reality* to consist of many droplets in a statistical arrangement. It is true that in the microphysical domain the situation is more complicated, because, the closer one looks, the more blurred the picture becomes in some of its features. From a purely empirical viewpoint it thus may be hard to arrive at an immediate decision in favor of one or the other "mental picture." But how about the criterion of simplicity?

Instead of scrutinizing and improving the quantum ideology under this criterion, physicists, like other people in a quandary, have sought refuge in philosophical reflection rather than taking positive action. It is argued: why set up unnecessary problems when the answer is so simple? There is a fundamental *principle* of duality; and the equal rank of corpuscles and waves as mental pictures is assured by their give and take in the *principle* of complementarity. Besides, what is the difference between physical appearance and physical reality, anyway? The idea of a real world behind the phenomena is a metaphysical dream. And it is precisely the quantum theory that has given us the "epistemological lesson" that an objective reality, independent of the means applied by an observing subject, should not even be a topic of intelligent discussion. Let us be content, then, with two pictures or constructs whose union so adequately *describes* the facts.

This positivist viewpoint of Bohr and Heisenberg is of course diametrically opposed to the *realism* of Einstein, who with reference to the quantum dilemma declared: [1] "The concepts of physics refer to a real external world in which 'things' (material bodies, fields, etc.) claim to have real existence independent of perceiving subjects." To this I would add that, carrying the philosophical discussion about "reality," or lack of it, into natural science is, in my opinion, a misuse of philosophy for the profane end of brushing off an important internal problem of atomic physics. In the case of stratified clouds nobody will speak of two mental pictures, one of waves and one of droplets—irrespective of the philosophical reflection that our "external world" is a mental picture. One rather will weigh all the evidence and use his reason to decide which he will accept as "real," cloud waves or discrete droplets. In atomic theory it is just as imperative, *and possible, too* (see section 2), to establish a simple, realistic, and unitary theory in which the "quantum miracle" of a wavelike misbehavior of particles becomes a necessity. But let me tell the story in historical order.

Schrödinger, from 1925 up to this day, has consistently taken the stand that only matter *waves* are real, that particles are mere appearances, perhaps high wave crests in the wave field, in the sense of unitary field theory. And what looks like momentum conservation for particles in collision is actually a resonance phenomenon of interfering matter waves. As late as 1953 Schrödinger declared: [2] "There is, I think, no other way of accounting for the atomicity of matter than by admitting the eigenvalues of the wave equation to be discrete." Few quantum physicists look with favor on this view.

In 1926 Max Born suggested that, in spite of matter ray diffraction, only particles are the real constituents of matter, as confirmed by many decades of atomic theory as well as by the observation that the diffracted intensity pattern is built up by individual corpuscular impacts, one after another, in a statistical fashion. Born's *unitary particle interpretation,* which regards Schrödinger's wave amplitude as a "probability amplitude," has been accepted by most physicists, myself included.

Born's victory was watered down, however, by diplomatic intervention from the neutral country of Denmark through a *third view,* which wants to have it both ways. We are told that an electron, strictly speaking, is neither particle nor wave. Instead, both particles and waves are mental pictures of *equal rank,* neither having a preference over the other; rather, they complement each other. Born himself has lately become neutral and dualistic. In an otherwise most illuminating article of 1953 [3] he declares that water waves and electromagnetic waves are "real" by virtue of having certain invariant characteristics. But then he continues: "Why then should we withhold the epithet 'real' even when the waves represent in quantum theory only a distribution of probability?" My answer is similar to that of the good Doctor Johnson rejecting the subjectivistic idealism of Bishop Berkeley: you can kick, and be hurt by, a stone as well as by a particle and by a water and an electromagnetic wave; but you cannot kick, or be hurt by, a list of statistical fractions. Putting such a list of betting odds, compiled according to past experience—even when this list can be graphically represented by a wavelike curve—on the same plane of "reality" with those particles whose average fate is represented by the "curve" is indulging in a linguistic trick for the sole purpose of saving face for an obsolete dualism, obsolete ever since Born interpreted wavelike appearances as actually produced by particles.

And here I come to the root of the "quantum mess." The same people who accept Born's clear and realistic unitary particle interpretation *also* do homage to the dualistic and neutral doctrine of Bohr and Heisenberg, occasionally also relapsing into the ideology of unitary wave theory (e.g., in the so-called contraction of a wave packet of Heisenberg's, which in fact rests on a vacillation between two meanings of the word "state," [4] physical state of an object *versus* state of expectation of an observer). This double- or triple-think is defended by semiphilosophical argument, such as, quoting von Weizsäcker,[5] "Perhaps we can best speak of the collapse of the category of substance; perhaps we should rather speak of the necessity of adapting our logic, formed by thinking in objects, to the new situation."

Adapting our logic? And to which new situation? Everybody knows that the elastic data of a piece of iron, and viscosity data of the same piece of iron in the molten state, are mutually incompatible observables; one must never even *think* that both data could be possessed by the same piece of iron at the same time, one overt and the other hidden. This example is even more "quantal" than that of position q and momentum p, which *can* be possessed simultaneously. For example, α particles are emitted by a radioactive substance with a well-known momentum p; when they hit a screen at a certain place q, they arrive at q with momentum p. Only the *future* p in a subsequent experiment cannot be predicted (Margenau [6]) better than with margin

$$\delta p \sim h/\delta q$$

This uncertainty relation would have no physical meaning unless *exact* p values belonging to a given range δq are first observed, and then found to be scattered over a range δp

(K. R. Popper [7]). The scatter δp of exact p values for given δq of a particle gives us an important lesson in mechanics. But it is hard to see why it should give us an epistemological lesson, compel us to change our logic and not to think in objects any more, etc. Those profundities seem to have but one end: to save the pre-Born dogma of a fundamental duality according to which "an electron is neither particle nor wave," instead of simply conceding that electrons and other particles statistically behave in a wavelike fashion, and *we still do not know why*. Refer to section 2, however, for an explanation.

The question "why" is hardly answered by the allegation (von Weizsäcker [5]) that "an electron, under certain experimental conditions, behaves as though it were a wave filling the whole space." This "as though" ignores the key consideration of Duane [8] of 1923 that the *periodic* pattern on a screen is due to the mechanical activity of the *periodic* crystal "filling the whole space" rather than to a mythical wave character of the electron filling the whole space. Duane's systematic quantum-mechanical particle interpretation of the wavelike diffraction (which preceded Born's particle interpretation of the ψ function by three years) has been ignored also by Bohr in his long and inconclusive "Discussion with Einstein," [9] which otherwise could have been resolved in a few pages.

What can be the reason that physicists accept the statistical particle interpretation in their everyday work, yet pay lip service to a dualistic ideology which replaces objective physics by an evaluation of subjective expectations and mental pictures? The reason, in my opinion, is that neither Duane, nor Born, nor their successors have ever proceeded from interpretation to an explanation as to *why*, i.e., on the grounds of which elementary nonquantal features of particle mechanics, said particles (or bodies in general) should display wavelike interference phenomena and periodicity in space and time; e.g., *why* is $E = h\nu$ and $p = h/\lambda$? Reference to a "principle" of duality is no better than saying that people are poor because of their poverty. I think, however, that Duane-Born's particle interpretation of undulatory quantum phenomena can indeed be supplemented by an *explanation* based on a few elementary, almost self-evident, nonquantal ground postulates from which one arrives at the same amazing rules of quantum mechanics which are usually introduced *ad hoc* or as manifestations of "quantum principles." I can report on these developments only in the most perfunctory manner. For details refer to the physical literature.[10]

2. *Unitary Foundations of Quantum Mechanics*

As a *first step* one has to renounce the determinism of classical mechanics in favor of still unspecified statistical laws for the *transition* of a specified object (particle, atom, field, etc.) from state to state in response to macroscopic testing instruments such as a position meter, an energy meter, a momentum meter, and so forth. Let us call these testing instruments an A-meter, B-meter, etc. From an A-meter test our object can emerge only in one of the A states, A_1 or A_2 or A_3, etc., respectively. The B-meter test leaves the object in one of the B states, B_1 or B_2 or B_3, etc. Suppose that the object has emerged from an A-meter test in the state A_3. If it is now subjected to a B-meter test, its emergence in the particular state B_5 occurs with a definite statistical frequency, $P(A_3, B_5)$, also known as the transition probability from A_3 to B_5. The various transition probabilities from the A-states to the B-states can be ascertained experimentally and then be compiled in a table or "matrix" of the form

$$\begin{matrix} P(A_1, B_1) & P(A_1, B_2) & \cdots & \\ P(A_2, B_1) & P(A_2, B_2) & \cdots & = P(A, B) \\ \cdots & \cdots & \cdots & \end{matrix}$$

Similarly there are tables $P(B, C)$ and $P(A, C)$, and so forth, pertaining to the same object. The transition probabilities are two-way *symmetric,* $P(A_3, B_5) = P(B_5, A_3)$ in correspondence with the reversibility of classical mechanical processes. From this it follows that not only every row but also every column in a P matrix adds up to *unity.* One may therefore denote the P matrices as *unit magic squares.*

As a *second step* let us assume that the various P tables are connected by a (still unspecified) general correlation law of "transformation" so that, when the tables $P(A, B)$ and $P(B, C)$ are given, the table $P(A, C)$ is thereby determined, or at least restricted—as the length $L(A, C)$ in a triangle is restricted when the lengths $L(A, B)$ and $L(B, C)$ are given. The problem of figuring out possible forms of a general correlation law of transformation between P tables leaving their unit-magic-square quality *invariant* is a purely mathematical proposition. There is only one conceivable law that satisfies this condition of invariance, known as the law of *unitary transformation.* It is precisely what the physicist calls "interference for probability amplitudes." It is best expressed in terms of intermediate quantities ψ which are complex, but so that $\psi(A_k, B_j)$ is complex conjugate to $\psi(B_j, A_k)$, and the absolute square of both equals $P(A_k, B_j) = P(B_j, A_k)$. Complex quantities are often used in physical theory for representing wavelike quantities having amplitude and phase. Therefore the physicist is tempted to see, in the law of unitary transformation, which in matrix notation reads

$$\psi(A, C) = \psi(A, B) \times \psi(B, C),$$

a "wave law." However, since a complex quantity can also be represented by a *vector* in a plane, it is more adequate to conceive the complex probability amplitude ψ as a vector giving direction to the corresponding probability P, so that all the P's together form a kind of structural framework in a *plane*, as the lengths $L(A, B)$, etc., connecting points $A, B, C, \cdots$, in a plane form a geometrical framework, with the associated vectors obeying the law

$$V(A, C) = V(A, B) + V(B, C).$$

Therefore, the "interference of probabilities" is not a wave law impressed on particles by a strange whim of nature wishing them to conform with Bohr's principle of complementarity or duality. It rather is the only *conceivable* general correlation law between the probability tables (unit magic squares), supposing that a general correlation law exists at all, rather than chaos. Incidentally, a uniqueness proof is still lacking; therefore the word *conceivable* rather than *possible* has been used. The situation here is similar to that of statistical mechanics, which has been erected without a strict proof of ergodicity.

The *third step* which completes Duane-Born's statistical interpretation into an explanation concerns the wavelike periodicity rules of quantum dynamics, Planck's $E = h\nu$, de Broglie's $p = h/\lambda$, and their modern counterparts, Born's commutation rule and Schrödinger's p-operator rule, culminating in the Schrödinger wave function

$$\psi(q, p) = \exp(2i\pi qp/h).$$

The h-dominated quantum rules resulting in this wave function are usually introduced *ad hoc.* However, they can be *explained,* i.e., deduced from the following well-known elementary features of linear coordinates q and conjugate linear momenta p:

(*a*) Any physical quantity defined in terms of two (or more) coordinates q' and q'' shall depend on the *difference,* $q' - q''$, rather than on absolute q values. The same shall hold for p.

(*b*) The statistical density in q-space for given p is *constant,* and the statistical density in p-space for given q is *constant.*

Both (*a*) and (*b*) stipulate only that there is no preferred system in q-space or in p-space, that is, *invariance* of mechanics under Galileo and/or Lorentz transformations. Neither (*a*) nor (*b*) is a quantum postulate. Yet, in combination with the previously discussed interference law (unitary transformation) with *invariance* of the unit-magic-square quality of the P tables, (*a*) and (*b*) lead by mathematical necessity to the result

$$\psi(q, p) = \exp\,(2i\pi qp/\text{constant})$$

which is the backbone of quantum dynamics. The constant is called h; it must be a universal constant, the same for all objects, because interaction between two objects would be impossible if the one should give out quanta h and the other could receive only quanta h' different from h. The interference of probabilities and the wavelike probability amplitude function $\psi(q, p)$ can thus be explained on the grounds of simple and general nonquantal postulates of symmetry, invariance, and the like, on a unitary statistical particle basis.

The same formal quantum rules can immediately be applied also to a *field* when the field is first "mechanized," i.e., represented by an energy function in terms of generalized field coordinates and field momenta. In the case of an electromagnetic field the question is not: does light consist of waves or of photons? It is rather: why does a field vibration of frequency ν display energy changes $E = h\nu$ only? The answer is that fields, like particles, are subject to the general quantum rules that have been known since 1926, but can also be *explained* as seen above. A completely different question is that of the special energy functions of various "fundamental" particles and their fields. We here are concerned only with the general formal quantum rules, not with their application to special mechanical systems. The formal quantum rules, however, can be understood without appealing to a "principle" of duality. Duality remains as a superficial appearance; it is not a fundamental principle in its own right.

6

Metaphysics: Before or After Physics?

RAYMOND J. SEEGER

IT WOULD be interesting to ascertain the common denominators of agreement and of disagreement of these papers on *The Nature of Physical Knowledge*. Each one of us, I suppose, would agree and disagree with each speaker to some degree. My role as a discussant of the talks, not of the topic, will be essentially that of an individual critic. My only choice is whether to make everyone unhappy by criticizing each paper somewhat, or to make myself unhappy by reviewing one paper completely. As a compromise, I shall consider primarily the three papers that were sent me before the meeting.

Our basic problem in this discussion is the philosophy of physics, i.e., an attempt to answer certain perennial questions which always arise along any broad approach to the study of man and his environment. For example, from the viewpoint of physics: Is the law of gravitation true? Is a physical field real? Why study physics, anyway? The philosophy of science, in general, is an attempt to get common answers to these common questions from all scientific avenues, namely, physics, biology, psychology, sociology: What is true? What is real? What is of value? Three distinctive attitudes are typified by the statements: "I do not know" (dogmatic agnosticism); "I do not know, but" (indifferent skepticism); "I do not know, but I am finding out more and more" (aggressive faith). We must keep in mind throughout our discussion the peculiar viewpoint, expressed or inarticulate, which each speaker undoubtedly has with regard to these fundamental philosophical questions (including Professor Percy Bridgman's belief that physics has "almost no recognizable philosophical component"). Otherwise, there will be foggy confusion.

Our speakers seem to cover the entire gamut. Professor Bridgman, who claims, "The scientist has no place for faith," [1] fears that we may soon be involved in the law of diminishing returns for science, inasmuch as its problems are becoming more complex and seemingly fewer new methods are available for their solution. In this respect I note that George Gamow concludes his recent book, *Matter, Earth, and Sky,* with an equally pessimistic note. Viewing the frontiers of the universe for the very large and for the very small, he draws an analogy with the old discovery of the "new world" and hence the round earth, which by its very finiteness limited further findings. As the haze of quantum theory and of relativity spread across Lord Kelvin's almost cloudless view, a glorious dawn hailed a new day! We, too, can hopefully peer beyond our seemingly inaccessible horizon.

In looking over the three papers one is struck at once by an apparent lack of clarity due to the failure of precise definition of the terms of the topic. No common ground for discussion is evident. Professor Bridgman, it is true, confesses his own qualms in this regard, particularly with respect to the word *nature;* Professor George Klubertanz, indeed, seems to use it in the same sense as one does in speaking of human nature. The word *physical,* too, is not sufficiently definitive for satisfactory discussion. Professor Bridgman would have it include modern biological phases and possibly some psychological aspects. What about the other speakers? *Knowledge* (in quotation marks in the title) seems to be an especially vulnerable concept for each of the speakers, albeit they use the term *science* (its Latin equivalent) with little such apprehensiveness. Professor Bridgman would avoid any distinction between physical knowledge and any other so-called knowledge. He considers that knowledge, in general, implies truth, which he himself does not bother defining here—even operationally; frankly, he wishes to avoid any suggestiveness as to platonic ideas. Neither does Professor Henry Margenau find it necessary to define physical knowledge, while regarding certain "metaphysical" principles as prerequisite to any such physical knowledge—i.e., existing strictly before physics, not after it. Thus he not only assumes *a priori* guides, but he insists that they exist expectantly beyond the realm of human senses. More difficult to understand are the particular "metaphysical" principles which he regards in some sense as only half *a priori* and half *a posteriori* (a paradoxical contradiction of terms). One would wish some operational or practical criteria for recognizing these undefined aspects.

There is need for individual clarity, if not common agreement, not only with respect to the words in the topic itself, but even more with respect to other terms that are frequently used by the speakers. *Explanation* is just such a word. Professor Alfred Landé is evidently unhappy with Max Born's atomic picture, inasmuch as it does not purport to "explain" facts; i.e., it does not seek to relate them to "familiar and simple general principles." He makes an interesting, but not logically necessary, characterization of theories as being descriptive, interpretive, and explanative. Furthermore, Professor Margenau would view the deductive method as equivalent to explanation, somewhat in the sense of Professor Landé, but not entirely so. He likes to use the word *exact* in connection with such logical consistency—exactly what I would not wish to do in applications of mathematics to phenomena.

The word *metaphysics,* too, has been frequently used. Professor Landé regards a metaphysical stage as "a hidden power or principle that accounts for phenomena"; he cites the old "horror vacui" as an example. Unfortunately Professor Margenau has not found it necessary to give here a clear-cut, complete definition of his own usage of this term. In this connection I am reminded of a statement by the late Richard von Mises: [2] "Every author that writes on metaphysics more or less changes his boundaries."

Last, and most important, is the use of the term *reality.* Professor Bridgman would urge physicists to avoid this word *reality* as an anathema, although he admits grudgingly that measurements can be regarded as being physically real. He observes rightly that physicists never deal with complete reality. Professor Landé, on the other hand, argues that every physicist must be a realist—like Einstein. From a different viewpoint Professor Klubertanz, too, believes that science should be realistic.

This casual usage of undefined terms results in the evident lack of communicability among the participants. Accordingly I should certainly agree with Professor Bridgman that any panel must agree upon definitions before attempting to discuss their applications—particularly at a Socratic symposium on natural philosophy.

All these papers are concerned not just with the philosophy of physics, but with the broader question: What is physics? I shall not attempt to go into details as to their individualistic answers, but rather indicate important aspects that I personally would emphasize.

First, let us consider the experiential origin of physics. Much has been said here of the need for repetitive experiences, which Professor Bridgman implies are essential. A single explosion, of course, is not repetitive; but even history does not strictly repeat itself. Apparent repetition of an event is frequently due to our more or less simplified view of it. We often say, "It happened again." But what is "it"? How do we recognize "it"? How do we isolate an event from its associations? Observed repetition depends upon our operational ability (in principle, at least) to identify approximate classes which may recur from time to time. In this connection we assume tacitly the uniformity of nature and require understandingly a community of ideas, both of which serve as automatic checks upon any verification beyond our personal confirmation (Professor Bridgman's "intellectual integrity"). Isolated individuals, such as Leonardo da Vinci, to be sure, may unknowingly be correct, but ahead of their times. The rate of scientific progress, however, depends upon the communicability of ideas as well as upon their correctness. Professor Margenau would apparently minimize the opinions of others in comparison with his own self-knowledge. He would regard personal transsubjectivity as a primary, objective fact—presumably as cogent as Professor Bridgman's private "scientific" proof. But what about John Dalton's color blindness? Would not his consistently colorful pictures be different from those of other viewers? In these discussions, as physicists, we seem to forget that, in all our total experiences, we focus upon a few selective aspects, which necessarily distort our own views of the phenomena themselves. Man and his environment become a skeleton framework outlined quite differently by physicists and by biologists, as well as by other scientists.

The other major component of physics is theory. All the speakers—myself as well—agree as to the uncertainty of physical knowledge. In particular instances, however, I am inclined to be even more uncertain than some of them. Professor Bridgman, for example, stresses the fact that one should never state: "Not *A* is, because *B* is"; but, rather, "*A* may be because *B* is." I should claim, more cautiously, "*A* may be because *B* may be." Moreover, I am not so positive as mathematically inclined Professor Margenau that the deductive method is more significant than the inductive one and, therefore, will produce a greater exactitude of theory. (That comfortable word *exact* again!) Deduction, indeed, when applied to natural phenomena does not have any greater certainty in its premises than induction in its conclusions. A deduction is either a logical tautology restating what is implicitly involved in the premise, or else a scientific proof based upon inductive generalizations, which we call axioms. Deduction and induction are experientially opposite sides of the same coin. As far as I am concerned, the conservation of momentum derived from Newton's laws of motion is no less certain than the laws themselves—indeed, it may be preferable as a starting point for certain purposes. "Deductive uncertainty" involves empirical choice; "inductive uncertainty" is based upon theoretical choice. By no means do I find that "those principles, metaphysical or pragmatic, as you will, serve to minimize or limit the uncertainty."

In other cases, I have apparently a greater feeling of certainty. For example, Professor Landé views with alarm a particular kind of positivist philosopher who exhibits a "world picture of his own making without claim to represent real objective events in a real world." He regrets that such positivists may look at "two pictures with the reservation that neither picture is true." To me, a theory is pragmatically "true" only in so far as it agrees with observed facts, i.e., describes and predicts phenomena. Our present difficulty with the concept of an electron arises to some extent out of our desire to describe it in familiar language as a particle or as a wave, whereas we have no experiential basis for doing so. Professor Landé views the use of both pictures as an immature composite like a Greek mythological creature, say a centaur, half horse and half man. Is it not possible, however, for us to use different metaphors to describe the same situation? We may note, "He is a lion in the fight"; or we may say, "He won't give up the ship." In each case, a certain characteristic is communicated without the necessity of combining both pictures into a lion-eating ship or a ship-sailing lion—as when viewing different sides of a partially hidden building. I endorse wholeheartedly Professor Landé's desire to have a single picture—by all means in classical terms, if possible. The question is whether or not such a picture is humanly possible on the basis of what we now know. At present, I find nothing more logically satisfying than the principle of complementarity. This is not the occasion to examine the details of Professor Landé's ingenious and interesting presentation of quantum mechanics.[3] I would merely note a few personal misgivings.

In the first place, we cannot regard an electron as a particle in all experimental setups. Even when we speak of the mass m of an electron we generally extrapolate its dependence upon speed to the condition of zero velocity. Any such experiment will necessarily be related to the measurement of its energy E with the measurement of its conjugate time t—and hence an uncertainty ΔE related to an uncertainty Δm. (I do wish physicists would not speak so loosely of "masses which gradually melt away into photon energy." After all, mass is truly conserved in such a process.) We have not actually fixed an electron in space and in time; hence we have no experimental evidence that it behaves wholly like a macrocosmic particle. Now an electron behaves like this, now like that! But what is it—this elusive electron? I find difficulty also in recognizing operationally the generality of Professor Landé's basic probability function, $\psi(q, p)$, and its associated unitary matrices. His argument seems to be based only on the reasonableness of some specific instances. Finally, the equivalent mathematical formalism which he develops does not happen to give me a "familiar" picture. I would, of course, agree that the mathematical solution of a wave equation need not be interpreted as a physical wave; indeed, it does not have to be related to a physical or mathematical model at all. Professor Landé's basic complaint seems to be that we all are concerned more with what an electron does than why it does so. One is reminded of Aristotle and of St. Thomas Aquinas who insisted upon discerning an intelligible principle as to why a rock falls and smoke rises, why a body moves at all, rather than as to how bodies fall, as questioned by Galileo Galilei. There is always a practical limit as to what any of us will accept as "general, simple, and familiar." Certainly no single theory has ever been in complete agreement with all pertinent facts. Our theoretical choice [4] is dependent at any time upon many factors, past, present, and future.

In short, as the speakers look at the experimental and theoretical aspects of physics, they seem to be faced with the need of agreeing first of all upon what physics itself is.

I should like to consider at greater length Professor Margenau's interesting belief in the existence of metaphysical presuppositions such as simplicity, elegance, causality, et al.[5] We would certainly all agree that there are such heuristic factors affecting the choice of hypotheses: economy of thought, symmetry, invariance, etc. All of us, for example, would probably subscribe at least to the assumption that any theory should be simpler than the facts it purports to describe.

The question today is whether these are truly metaphysical principles or merely pragmatic guides, as Philipp Frank[6] suggests. Professor Margenau would regard them as "metaprinciples." Although a definition is not given explicitly by him here, the meaning seems sufficiently clear from the context. But are they inherently scientific in nature or essentially extrascientific (i.e., metascientific)—beyond observable nature? I note that Professor Margenau defines science as "everything accessible to experience." Why then should one not include these in the behavorial sciences, say the sociology of science? Extrascientific or "nonempirical" may imply merely a narrow definition of science. I cannot subscribe to his artificial distinction between socially acceptable elegance and socially produced education—the former being presumably a metaphysical principle; the latter, only cultural conditioning. Certainly intersubjectivity, i.e., the communality of science, is an inherently behavioral pattern of society, not a metaphysical presupposition.

What are his "rational criteria" for selecting such metaphysical principles, as contrasted with social pragmatism? Mach's economy of thought, to me, is basically a social development of science—not necessarily a lodestar, which he set out to reach. The cultural pattern, I believe, is an important factor; it is not "socially neutral." For example, so-called simplicity of mathematics depends upon the mathematical knowledge and technical skill of the user, and is therefore a function of time. One would wish to record all historical failures to designate simple things, as well as remembered successes for any such proposed principle. Professor Margenau's qualitative distinction between a metaphysical principle and a physical theory, based upon the quantitative difference of their half-lives, lacks logical cogency. The explanation may be merely in the more subjective component of the former and the more objective aspects of the latter. Simplicity, indeed, may be of value not so much for its esthetic relations as for the dynamic impetus it gives to the developing of a theory, say by the heuristic employment of models (including mathematical ones). James Clerk Maxwell and Albert Einstein were both able to generalize more easily because of earlier, comparatively simple theories. Elegance and beauty, cultural attributes, both vary with time and place. As phenomena and their associated theories become more complex, simplicity itself may fade into a nebulous concept—even into the grin of a vanishing Cheshire cat. Symmetry, too, is certainly less binding in art today than in the time of the Greeks. Nevertheless, does symmetry remain a metaphysical principle of modern science? Has the heuristic value of symmetry in dealing with the existence of elementary particles been a coincidence of large numbers—or an intrinsic phase of a universal design? How shall we describe its failure to insure parity? What about modern asymmetry tomorrow? Presumably complex phenomena might well show symmetric relations as a first approximation, whereas essentially asymmetric, simple phenomena would never exhibit any symmetry at all. Would Professor Margenau regard yesterday's Pythagorean glorification of integers, used even by Johannes Kepler, as a metaphysical presupposition for science? What is the role of such an idea today? Is it akin to the late Arthur Stanley

Eddington's numerology? Would Professor Margenau have regarded William Prout's hypothesis as a metaphysical principle yesterday? What about now?

What, moreover, must one do in the case of inevitable conflicts among such metaphysical principles, that are unresolved in social usage? The general gas law, it is true, may be relatively simple, but it certainly does not represent enough economy of thought inasmuch as it does not comprehend all the data contained in the less simple van der Waals expression. Professor Margenau himself is aware of this dilemma, but he does not propose to resolve it—by any hypermetaphysical principle. What is particularly lacking in this restrictive view is the role of the human imagination! Despite Professor Margenau's unforgettable image of the neutrino as a "metaphysical gleam in Pauli's eye," I myself see no aura of metaphysics about it. It was a shrewd guess, and as such would normally be considered as a part of the psychology of creation and invention. There is nothing sacred or even rational about such a practice. The primary question is its usefulness. Such guides, indeed, are not necessarily "pre-empirical commitments," except in that a scientist may personally believe in them. I would regard them as being born in experience and subject to growth changes, not prenatal conceptions—except in so far as our minds behave thus under certain conditions. They are just as much a part of the structure of physical theory as other presuppositions such as axioms —all justified pragmatically by their usefulness.

As a discussant, I cannot properly present my own view on this occasion. I would, however, emphasize a remark of Max Born,[7] namely, "Faith, imagination, and intuition are as essential to science as in any other human activity," although I would not necessarily agree with his designating such a belief a metaphysical principle. Factually, "we do not know, but we are finding out more and more." Consequently we all sketch a thoughtfully coherent world of science out of the seemingly chaotic world of appearances. Is it truly representative of an unknown world of nature behind the phenomena—on the assumption that nature itself exists? What about the relation between this supposedly unchanging world of nature and the changing world of science? I cannot altogether agree with Professor Margenau that "science will tell us what things are real." Neither do I find or expect to find a one-to-one correspondence between these two worlds—possibly because of the intermediacy of the brain, as stressed by Professor Bridgman. Through the increasing descriptability of science and the continuing predictability of its methodology, one gains more and more confidence in the likeness of the picture as a whole. To me, then, the dynamic world of science is essentially a symbol pointing to an indistinct invariant world of nature—the world of science created, to be sure, subjectively in man's mental image, but not out of nothing objective.

Professor Margenau has indirectly introduced some strange gods to this meeting. I regret that he compares good science with poor theology, although he admits the possibility of good theology—parenthetically. Perhaps, therefore, I may be permitted to complete my own view.[8] I cannot endorse his belief that, "on the plane of principles, metaphysical or otherwise, I can see no difference between gods and electrons." On the contrary, the very nature of a physical electron and that of a spiritual God make mandatory an essential difference in approach—even though similar elements are common. In our one world of man and his environment there are undoubtedly religious aspects. Out of the transient phenomena, therefore, man can and does construct also a world of theology which, I believe, points symbolically to the God of nature. This procedure may well involve extrascientific matters, even metascientific principles. Here, again, I find no one-to-one correspondence between the factitious world of theology, created out of the changing world of phenomena (including possibly revelations, i.e., insights) and the eternal world of God, but once again an increasing likeness as a whole is credible in view of the increasing understanding and continuing value of spiritual experiences.

"Knowledge is power," but prior to knowledge there is always creative faith that some knowledge is within the grasp of man. "Credo ut intelligam" has been the guiding light for all progress. "Now abide the faith, hope, love, these three"—but the first of these is faith. For faith is what physicists—indeed, all men—live by, the basis of any "program for action." Perhaps beneath our confusing verbiage there is latent considerable agreement as to practice, and open disagreement only as to belief in the theoretical meaning of the practical. Regardless of the terminology, therefore, one might view these so-called "metaphysical principles of science" as follows: (1) either assumed *before* physics as an existent prefabricated mold[5] (i.e., strictly extrascientific), regarded possibly as amorphous science;[2] (2) or identified heuristically in the behavioral sciences[2,6] (i.e., definitely extraphysical), believed possibly crystallizing *after* physics to become symbolic pointers[7] to a world beyond our sensory experiences.

7

The Role of A Priori *Elements in Physical Theory*

ADOLF GRÜNBAUM

1. *Introduction*

I WISH first to examine critically the thesis that, in a physical theory comprising a *network* of hypotheses, any one component hypothesis can be preserved in the face of seemingly contrary empirical findings as part of an *explanans* of these very findings by making suitable compensatory modifications in the remaining body of the total theory. This conception of the ingression of the *a priori* into physical knowledge is, of course, *not* the Kantian one, since it repudiates the latter's dichotomy between *a priori* and *a posteriori* certifiability as a basis for classifying *individual* principles of scientific theory. Instead, it claims that there is a fundamental theoretical *ambiguity* of the observational evidence which allows us, at a price, to *refuse* to abandon any one component hypothesis that we have thereby chosen for *a priori* espousal. This thesis was advanced about fifty years ago by the physicist, historian of physical thought, and philosopher of science Pierre Duhem (1861–1916).[1]

Duhem's argument was articulated and endorsed by

Einstein a decade ago in regard to the epistemological status of physical geometry. I therefore wish to inquire into its validity in the context of geometry by giving a critique of each of the following: (1) Einstein's geometrical defense of the Duhemian ambiguity; and (2) Jacques Maritain's allegation of the existence of a philosophical, as distinct from scientific, *escape* from that ambiguity.

This inquiry bears not only on the role of the *a priori* but also on the twin problem of the adequacy of physical theory as a description of the external world. For, once we shall have noted precisely the important distinction between the quasi *a priori* choice of a physical geometry in the sense of Duhem and the conventional adoption of such a geometry in the sense of Poincaré, we shall see the following: so far as a geometry can be affirmed *a priori* in the sense of Duhem, its characterization of the geometric features of physical reality suffers from uncertainty in a *less mitigable* sense than it does merely because it asserts more than is entailed by its supporting evidence.

Duhem maintains that, just as there can be no crucial verifying experiments, so also there can be no falsifying (refuting) experiments for any particular hypothesis ingredient in a physical theory in isolation from the remaining component hypotheses of that theory.[2] His grounds are that the logic of every disconfirmation no less than of every confirmation is such as to involve the confrontation with experience of an entire network of inextricably interwoven hypotheses rather than the separate testing of any one component hypothesis. The following symbolization will serve to clarify the reasoning underlying Duhem's contention. Let H represent the particular hypothesis to be tested, and A the set of auxiliary or collateral assumptions (laws, boundary conditions) integral to the deductive linkage descending from H down to the entailed observational findings F which might be presumed to support H as such. Then, if the arrow, $\rightarrow$, is the symbol for logical entailment, and the dot, $\bullet$, represents conjunction, we have the following premise for an inductive, *confirmatory* inference of H:

$$[(H \bullet A) \rightarrow F] \bullet F$$

But this premise not only precludes certainty as to the truth of H but also makes clear that even such merely inductive support as the evidence F does provide for a theoretical inference is conferred *not* on H by itself but only on the *conjunction* of H and the collateral assumptions A of the theory.

An analogous inconclusiveness obtains, according to Duhem, in regard to the *refutation* of an isolated hypothesis H upon the discovery of data O that may seem to be highly unfavorable to H because they are logically incompatible with the logical consequence F of $H \bullet A$. If the tilde, $\sim$, is used as the symbol for negation, the *premise* for the refutative inference that H is false is the following:

$$[(H \bullet A) \rightarrow F] \bullet \sim F$$

It is evident that this premise entails *not* the falsity of H *by itself* but only the weaker conclusion that H and A cannot *both* be true. And now Duhem makes the following far-reaching claim: by allowing that H be true while A is false, the observational findings O—whatever they may be—always permit the theorist to preserve H as a part of an *explanans* of O through a suitable modification of A, such that the conjunction of H and the now altered version of the auxiliary assumptions does *explain* the set O, which is logically *incompatible* with F. In this significant sense, then, Duhem claims, an *a priori* element does enter physical theory, because the logical constraints imposed by the observational data are sufficiently flexible and *ambiguous* theoretically to sanction a kind of *a priori* choice in regard to the alternative of either unalterably retaining H or abandoning it.

We shall refer to this logical situation as the "Duhemian ambiguity" or "Duhemian alternative."

I have argued elsewhere in detail [3] that Duhem's assertion of the *inevitable inconclusiveness* of the *falsifiability* of an isolated explanatory hypothesis H of empirical science is a *non sequitur* in the following sense: No *general* features of the logic of falsifiability can assure, *for every hypothesis H and independently of the domain to which it pertains,* that H can always be preserved as an essential part of the *explanans* of any empirical findings O whatever, provided that we rule out such *trivial* auxiliary hypotheses as $\sim H \vee O$ (where v is the symbol for the inclusive "or"), which no scientist would deem explanatory. For—to summarize my detailed argument—Duhem *cannot guarantee* on any general logical grounds the deducibility of O from an *explanans* constituted by the conjunction of H and some *nontrivial,* revised version R of the original auxiliary assumptions A: the existence of the required nontrivial set R of collateral assumptions must be demonstrated for each particular case.

Here I now wish to demonstrate geometrically that the categorical form of Duhem's thesis is *false.* A critical examination of Einstein's geometrical articulation of Duhem's thesis will now show that (1) the testing of physical geometry furnishes a counterexample to Duhem's categorical claim of the inevitable inconclusiveness of the falsifiability of part of an *explanans,* and (2) the valid core of Duhem's thesis is the following much weaker assertion: the logic of every disconfirmation, no less than of every confirmation, of an isolated empirical hypothesis

H is such as to *involve at some stage or other* an entire network of interwoven hypotheses in which *H* is ingredient rather than the separate testing of the component *H* at every stage.

2. *Critique of Einstein's Duhemian Thesis*

Physical geometry is usually conceived as the system of metric relations exhibited by transported solid bodies *independently of their particular chemical composition.* On this conception, the criterion of congruence can be furnished by a transported solid body for the purpose of determining the geometry by measurement only if the computational application of suitable "corrections" (or, ideally, appropriate shielding) has assured rigidity in the sense of essentially eliminating inhomogeneous thermal, elastic, electromagnetic, and other perturbational influences. For these influences are "deforming" in the sense of producing changes of *varying degree* in different kinds of materials. Since the existence of perturbational influences thus issues in a dependence of the coincidence behavior of transported solid rods on the rods' *chemical composition,* and since physical geometry is concerned with the behavior common to all solids apart from their substance-specific idiosyncrasies, the discounting of idiosyncratic distortions is an essential aspect of the logic of physical geometry. The demand for the computational *elimination* of such distortions as a prerequisite to the experimental determination of the geometry has a thermodynamic counterpart: the requirement of a means for measuring temperature which does not yield the discordant results produced by expansion thermometers at other than fixed points when different thermometric substances are employed. This thermometric need is fulfilled successfully by

Kelvin's thermodynamic scale of temperature. But attention to the implementation of the corresponding prerequisite of physical geometry has led Einstein to impugn the empirical status of that geometry. He considers the case in which congruence has been defined by the diverse kinds of transported solid measuring rods *as corrected for their respective idiosyncratic distortions* with a view to then making an empirical determination of the prevailing geometry. And Einstein's thesis is that the very logic of computing these corrections precludes that the geometry itself be accessible to experimental ascertainment *in isolation from* other physical regularities. Specifically, he states his case in the form of a dialogue in which he attributes his own Duhemian view to Poincaré and opposes Reichenbach's conception. But I submit that Poincaré's text will *not* bear Einstein's interpretation. For, in speaking of the variations which solids exhibit under distorting influences, Poincaré says, "We neglect these variations in laying the foundations of geometry, because, besides their being very slight, they are irregular and consequently seem to us accidental." [4] I am therefore taking the liberty of replacing *Poincaré* in Einstein's dialogue by *Duhem and Einstein. With this modification,* the dialogue reads as follows: [5]

Duhem and Einstein: The empirically given bodies are not rigid, and consequently can not be used for the embodiment of geometric intervals. Therefore, the theorems of geometry are not verifiable.

Reichenbach: I admit that there are no bodies which can be *immediately* adduced for the "real definition" [i.e., physical definition] of the interval. Nevertheless, this real definition can be achieved by taking the thermal volume-dependence, elasticity, electro- and magneto-striction, etc., into consideration. That this is really and without contradiction possible, classical physics has surely demonstrated.

Duhem and Einstein: In gaining the real definition improved by yourself you have made use of physical laws, the formulation of which presupposes (in this case) Euclidean geometry. The verification, of which you have spoken, refers, therefore, not merely to geometry but to the entire system of physical laws which constitute its foundation. An examination of geometry by itself is consequently not thinkable. . . . Why should it consequently not be entirely up to me to choose geometry according to my own convenience (i.e., Euclidean) and to fit the remaining (in the usual sense "physical") laws to this choice in such manner that there can arise no contradiction of the whole with experience?

Einstein is making two major points here:

1. In obtaining a physical geometry by giving a physical interpretation of the postulates of a formal geometric axiom system, the specification of the physical meaning of such theoretical terms as "congruent," "length," or "distance" is *not* at all simply a matter of giving an *operational* definition in the strict sense. Instead, what has been variously called a "correspondence rule" (Margenau and Carnap), a "coordinative definition" (Reichenbach), an "epistemic correlation" (Northrop), or a "dictionary" (N. R. Campbell) is provided here *through the mediation of hypotheses and laws* which are *collateral* to the geometric theory whose physical meaning is being specified. Einstein's point that the physical meaning of congruence is given by the transported rod as *corrected theoretically* for idiosyncratic distortions is an illuminating one and has an abundance of analogues throughout physical theory. Thus, in the theory of the Michelson-Morley experiment, for example, statements about round-trip times are linked conceptually to the readings of physical clocks via the sophisticated *optical theory* of interferometry. And the inadequacy of conceiving of all correspondence rules in physical theory

as straightforward operational definitions is acknowledged by Professor Margenau's avowal in his contribution to this symposium that operational definitions are merely *one species* of correspondence rules. I would add that operational definitions are a rather simplified and limiting species at that.

2. Einstein's second claim, which is the cardinal one for our purposes, is that the role of collateral theory in the physical definition of congruence is such as to issue in the following *circularity,* from which there is no escape, he maintains, short of acknowledging the existence of an *a priori* element in the sense of the Duhemian ambiguity: the rigid body is not even defined without first *decreeing* the validity of Euclidean geometry (or of some other particular geometry). For *before* the *corrected* rod can be used to make an *empirical* determination of the *de facto* geometry, the required corrections must be computed via laws, such as those of elasticity, which involve *Euclideanly* calculated areas and volumes. But clearly the warrant for thus introducing Euclidean geometry *at this stage* cannot be empirical.

If Einstein's Duhemian thesis were to prove correct, it would have to be acknowledged that there is a sense in which physical geometry *itself* does not provide a geometric characterization of physical reality. For by this characterization we understand the articulation of the system of relations obtaining between bodies and transported solid rods quite apart from their substance-specific distortions. And to the extent to which physical geometry is *a priori* in the sense of the Duhemian ambiguity, there is an ingression of *a priori* elements into physical theory to fill distinctively geometric gaps in our knowledge of the physical world.

I now wish to set forth my doubts regarding the soundness of Einstein's contention.

There can be no question that the laws used to make the corrections for deformations involve areas and volumes in a fundamental way (e.g., in the definitions of the elastic stresses and strains) and that this involvement presupposes a geometry, as is evident from the area and volume formulas of differential geometry, which contain the square root of the determinant of the components g_{ik} of the metric tensor. Now suppose that we begin with a set of Euclideanly formulated physical laws P_0 in correcting for the distortions induced by perturbations and then use the thus Euclideanly corrected congruence standard for *empirically* exploring the geometry of space by determining the metric tensor. *The initial stipulational affirmation of the Euclidean geometry G_0 in the physical laws P_0 used to compute the corrections in no way assures that the geometry obtained by the corrected rods will be Euclidean!* If it is *non*-Euclidean, then the question is: What will be involved in Einstein's fitting of the physical laws to preserve Euclideanism and avoid a contradiction of the theoretical system with experience? Will the adjustments in P_0 necessitated by the retention of Euclideanism entail merely a change in the dependence of the length assigned to the transported rod on such *nonpositional* parameters as temperature, pressure, magnetic field, etc.? Or could the putative empirical findings compel that the length of the transported rod be likewise made a nonconstant function of its *position* and *orientation* as independent variables in order to square the coincidence findings with the requirement of Euclideanism? The temporal variability of distorting influences and the possibility of obtaining *non*-Euclidean results by measurements carried out in a spatial region uniformly characterized by standard conditions of temperature, pressure, electric and magnetic field strength, etc., show it to be *quite doubtful* that the preservation of Euclideanism could always be accomplished short of introducing *the dependence of the rod's length on the independent variables of position and orientation.* Thus, in order to retain Euclideanism, it may be necessary to *remetrize* entirely apart from any consideration of idiosyncratic distortions and even after correcting for these in some way or other. But this kind of remetrization, though entirely admissible in *other* contexts, does *not* provide the requisite support for Einstein's Duhemian thesis. For it is the avowed onus of that thesis to show that the geometry *by itself* cannot be held to be empirical even when, with Reichenbach, we have sought to assure its empirical character by choosing and then adhering to the customary (standard) definition of congruence, *which excludes resorting to such remetrization.*

It is precisely such remetrization which Poincaré invoked as a basis for his claim that, if the customary definition of congruence on the basis of the coincidence behavior common to all kinds of solid rods does not assure a Euclidean description of the facts, then such a description can be guaranteed remetrizationally, i.e., by merely choosing an appropriately different noncustomary congruence definition which makes the length of a solid rod a specified *non*constant function of the *independent* variables of position and orientation.

By resting the possibility of giving *either* a Euclidean or a *non*-Euclidean description of the same spatiophysical facts on alternative metrizability in this sense, Poincaré is thus not at all invoking the alleged inductive ambiguity which *the Duhemian* claims to prevail even *when congruence is defined in the customary fashion by a standard rod for whose idiosyncratic deformations allowance has been made computationally.* For Poincaré tells us that, *quite* *apart from any considerations of distorting influences and even after correcting for these in some way or other,* we are at liberty to define congruence—and thereby to fix the associated geometry appropriate to the given facts—either by calling a solid rod equal to itself everywhere or by making its length vary in a specified way with its position or orientation. *Thus understood,* Poincaré's conventionalist conception of geometry is aprioristic in an innocuous sense.[6] By contrast, we shall now see that, in the context of his assumptions, the *Duhemian* cannot guarantee an *a priori* choice of a particular geometry from a set S of alternative geometries, since there is an important class of conditions under which the membership of his set S would be just one unique geometry!

That the geometry *by itself* may well be empirical, contrary to Duhem and Einstein, once we have renounced the kinds of alternative congruence definitions employed by Poincaré, is seen from the following possibilities of its successful empirical determination. After assumedly obtaining a non-Euclidean geometry G_1 from measurements with a rod corrected on the basis of Euclideanly formulated physical laws P_0, we can revise P_0 so as to conform to the non-Euclidean geometry G_1 just obtained by measurement. This retroactive revision of P_0 would be effected by recalculating such quantities as areas and volumes on the basis of G_1 and changing the functional dependencies relating them to temperature and other physical parameters. We thus obtain a new set of physical laws P_1. Now we use this set P_1 of laws to correct the rods for perturbational influences and then determine the geometry with the thus-corrected rods. If the result is a geometry G_2 different from G_1, then, if there is convergence to a geometry of constant curvature upon repeating this process several more times, we must continue to repeat it an additional finite number of times until the geometry G_n ingredient in the laws P_n providing the basis for perturbation corrections is indeed the same as the geometry obtained by measurements with rods that have been corrected via the set P_n. *If* there is such convergence at all, it will be to the *same* geometry G_n even if the physical laws used in making the *initial* corrections are not the set P_0, which presupposes Euclidean geometry, but a different set P based on some non-Euclidean geometry or other. That there can exist only one such geometry of *constant curvature* G_n would seem to be guaranteed by the identity of G_n with the unique underlying geometry G_t characterized by the following properties: (1) G_t would be exhibited by the coincidence behavior of a transported rod if the *whole* of the space were actually free of deforming influences; (2) G_t would be obtained by measurements with rods corrected for distortions on the basis of physical laws P_t presupposing G_t; and (3) G_t would be found to prevail in a given relatively small, perturbation-free region of the space quite independently of the assumed geometry ingredient in the correctional physical laws. Hence, *if* our method of successive approximation does converge to a geometry G_n of *constant curvature*, then G_n would be this unique underlying geometry G_t. And, in that event, we can claim to have found empirically that G_t is indeed the geometry prevailing in the entire space which we have explored.

But what if there is no convergence? It might happen that, whereas convergence would obtain by starting out with corrections based on the set P_0 of physical laws, it would *not* obtain by beginning instead with corrections presupposing some particular *non*-Euclidean set P, or *vice versa:* just as in the case of Newton's method of successive approximation, there are conditions, as Mr. A. Suna has pointed out to me, under which there would be no convergence. We might then nonetheless succeed as follows in finding the geometry G_t empirically, *if* our space is one of constant curvature.

The geometry G_r resulting from measurements by means of a corrected rod is a single-valued function of the geometry G_a assumed in the correctional physical laws, and a Laplacian demon having sufficient knowledge of the facts of the world would know this function $G_r = f(G_a)$. Accordingly, we can formulate the problem of determining the geometry empirically as the problem of finding the point of intersection between the curve representing this function and the straight line $G_r = G_a$. That there exists one and only one such point of intersection follows from the existence of the geometry G_t defined above, provided that our space is one of constant curvature. Thus, what is now needed is to make determinations of the G_r corresponding to a number of geometrically different sets of correctional physical laws P_a, to draw the most reasonable curve $G_r = f(G_a)$ through this finite number of points (G_a, G_r), and then to find the point of intersection of this curve and the straight line $G_r = G_a$.

Whether this point of intersection turns out to be the one representing Euclidean geometry or not is beyond the reach of our conventions, *barring* a remetrization. And thus the least that we can conclude is that, since empirical findings can greatly narrow down the range of uncertainty as to the prevailing geometry, there is no assurance of the *latitude* for the choice of a geometry which Einstein takes for granted. Einstein's Duhemian position would appear to be inescapable *only* if our proposed method of determining the geometry by itself empirically *cannot* be generalized in some way to cover the general relativity case of a space of *variable* curvature (in which the geometry cannot be specified by a single scalar like the Gaussian curvature) and if the latter kind of theory turns out to be true.

3. *Duhem's Thesis and J. Maritain's Philosophy of Geometry*

If my proposed method of escaping from the web of the Duhemian ambiguity were shown to be unsuccessful, and if there should happen to be no other scientifically viable means of escape, then, it seems to me, we would unflinchingly have to resign ourselves to this relatively unmitigable type of uncertainty. No, says the philosopher Jacques Maritain, who enticingly beckons us to take heart. The *scientific* elusiveness of the correct geometric description of external reality must *not* lead us to suppose, he tells us, that philosophy, when divorced from mathematical physics, cannot rescue us from the labyrinth of the Duhemian perplexity and unveil for us the structure of what he calls *"ens geometricum reale"* (real geometrical being).[7] As against Maritain's conception of the capabilities of philosophy as an avenue of cognition, I wish to uphold the following excellent declaration by Professor Bridgman, which he gave in his paper for the present symposium: "The physicist emphatically would *not* say that his knowledge presumptively will not lead to a full understanding of reality for the reason that there are other kinds of knowledge than the knowledge in which he deals." [8] To justify my endorsement of Professor Bridgman's statement in this context, I shall give a brief critique of Maritain's philosophy of geometry as presented in his book *The Degrees of Knowledge*.[9]

I have selected Maritain's views for rebuttal because they typify the conception of those who believe that the philosopher as such has at his disposal means for fathoming the structure of external reality which are not available to the scientist. In outline, Maritain endeavors to justify this idea in regard to geometry along the following lines. Says he: "There is no clearer word than the word reality, which means *that which is*. . . . What is meant when it is asked whether real space is euclidean or non-euclidean . . . ?" [10] To prepare for his answer to this question, he explains the following: "The word *real* has not the same meaning for the philosopher, the mathematician and the physicist.[11] . . . For the physicist a space is 'real' when the geometry to which it corresponds permits of the construction of a physico-mathematical universe which coherently and completely symbolizes physical phenomena, and where all our graduated readings find themselves 'explained.' And it is obvious that from this point of view no space of any kind holds any sort of privileged position.[12] But . . . the question is to know what is real space in the philosophical meaning of the word, *i.e.*, as a 'real' entity . . . designating an object of thought capable of an extra-mental existence. . . ." [13] One is immediately puzzled as to how Maritain conceives that his distinction between physically real space and the philosophically real space which is avowedly extra-mental is not an empty distinction without a difference. And, instead of being resolved, this puzzlement only deepens when he tells us that by the *extra-mental geometric* features of existing bodies he understands "those properties which the mind recognizes in the elimination of all the physical." [14] But let us suspend judgment concerning this difficulty and see whether it is not cleared up by his treatment of the following question posed [15] by him: How are we to know whether it is Euclidean geometry or one of the non-Euclidean geometries that represents the structure of the philosophically real, i.e., extra-mental or external space? In regard to this question, he makes the following assertions:

1. The capabilities of *physical measurements* to yield the answer to the question are nil,[16] because a geometry is presupposed in the theory of our measuring instruments which forms the basis of corrections for "accessory variations due to various physical circumstances." [17] Of course, we recognize this contention to be a strong form of the Duhemian one, although Maritain does not refer to Duhem.

2. The several non-Euclidean geometries depend for their consistency on their formal translatability into Euclidean geometry. This translation is effected by providing a Euclidean model of the particular non-Euclidean geometry in the sense of embedding an appropriately curved non-Euclidean surface in the three-dimensional Euclidean space. And the privileged position which Euclidean geometry enjoys as the underwriter of the consistency of the non-Euclidean geometries thus issues in a correlative dependence of the intuitability of the non-Euclidean geometries on the primary intuitability of the Euclideanism of the embedding three-dimensional hyperspace.[18]

Using the twin arguments from consistency and intuitability, Maritain then reaches the following final conclusion: "The non-Euclidean spaces can then without the least intrinsic contradiction be the object of consideration by the mind, but there would be a contradiction in supposing their existence outside the mind, and thereby suppressing, for their benefit, the existence of the foundation on which the notion of them is based.

"Either way we are thus led to admit, despite the use which astronomy makes of them, that these non-euclidean spaces are rational [i.e., *purely mental*] beings; and that the *geometric* properties of existing bodies, those properties which the mind recognizes in the elimination of all the physical, are those which characterize euclidean space. For philosophy it is euclidean space which appears as an *ens geometricum reale.*" [19]

I submit that Maritain's thesis is unsound in its entirety and can be completely refuted as follows.

First, as Hilbert and Bernays have explained,[20] the consistency of the Euclidean axiom system is *not* vouchsafed by its intuitive plausibility as an adequate description of the space of our immediate physical environment. Instead, we establish the consistency of Euclidean geometry by providing a model of the formal Euclidean postulates in the domain of real numbers in the manner of analytic geometry.[21] Now, Maritain overlooks that precisely the same procedure of providing a real number model can be used to establish the internal consistency of the various non-Euclidean geometries *without the mediation* of a prior translation into Euclidean geometry (except possibly in an irrelevant heuristic sense). And he is misled by the fact that, *historically*, the consistency of the several non-Euclidean geometries was established by means of a translation into Euclidean geometry, as for example in Klein's relative consistency proof of hyperbolic geometry via a model furnished by the interior of a circle in the Euclidean plane. For surely the *temporal* priority of Euclidean geometry inherent in the historical circumstances of our *discovery* of the internal consistency of the various non-Euclidean geometries hardly serves to establish the *logical primacy* of Euclidean geometry as the sole *guarantee* of their consistency. And Maritain's error on this count is only compounded by his intuitability argument for the uniqueness of Euclideanism as the only possible structure of extramental reality. The latter argument is vitiated by the inveterate error of being victimized by the misleading connotation of *embedding in a Euclidean hyperspace,* which is possessed by the terms "curved space" and "curvature of a surface." This connotation springs from unawareness that the Gaussian curvature of a 2-space and the Riemannian curvatures for the various orientations at points of a 3-space are *intrinsically* definable and discernible properties of these spaces, requiring no embedding. Moreover, Maritain overlooks here that even when the consistency proof of hyperbolic geometry, for example, is given on the basis of Euclidean geometry—which we saw is quite unnecessary—this can be accomplished *without* embedding, as in the case of the aforementioned *two*-dimensional Klein model, just as readily as by Beltrami's procedure of embedding a surface of constant negative Gaussian curvature (containing singular lines) in Euclidean 3-space.

Lastly, it can surely not be maintained that "the *geometric* properties of existing bodies" are "those properties which the mind recognizes in the elimination of all the physical." For, in that case, geometry would be the study of purely imagined thought-objects, which will, of course, turn out to have Euclidean properties, if Maritain's imagination thus endows them. And the geometry of such an imagined space could then not qualify as the geometry of Maritain's real or extra-mental space. The geometric theory of external reality does indeed abstract from a large class of physical properties in the sense of being the metrical study of the coincidence behavior of transported solids independently of the solids' substance-specific physical properties. But this kind of abstracting does not deprive metrical coincidence behavior of its physicality. And if the methods of the physicist cannot fathom the laws of that behavior, then certainly no other kind of intellectual endeavor will succeed in doing so.

4. *Remarks on the Contributions by Margenau and Bridgman*

Professor Margenau's intersubjectivity or communality of evidence as a controlling factor of scientific method is a safeguard whose reasonableness derives from the *mutuality of accreditation* obtaining between theory and evidence in virtue of the *interpenetration* of the criteria of credibility which certify evidence as *bona fide*, on the one hand, and theory as evidentially warranted on the other. This interpenetration enters into the scientific assessment of the credibility of reports of *non*repeatable *kinds of experiences*. The authenticity of the claims of such isolated kinds of experiences is made no less dependent on the latter's conformity to previous theory than evidence, in turn, is used to decide on the acceptability of a theory. Thus, loosely speaking, not only is evidence used to confirm a theory, but theory is invoked to certify reports of observations as veridical. And, in this sense, the quasi *a priori* lurks in the twilight of the fuzzy boundary between the evidence and its interpretation, between the observation terms and the theoretical terms of the language of physics. To cite but one example from the recent history of physics, one need only recall that, even before Shankland and his associates denied the adequacy of D. C. Miller's controls in his runs of the Michelson-Morley experiment,[22] many if not most physicists refused to give credence to Miller's claims. And their grounds for believing that Miller's findings could not have been obtained under the conditions claimed by him were none other than their quasi *a priori* theoretical supposition that the site of the Case Institute of Technology is *not* a terrestrial singularity.

In this same vein, it seems to me, one can reasonably reply to Professor Bridgman's challenge that we justify our confidence in our being awake rather than dreaming at certain times. If asked on what grounds I believe to be awake at the present time while talking, for example, I would answer *not,* as Hume did, that veridical perception is more *vivid* than dream experience but rather that the conformity of the structure of my present experiences to a large body of independently confirmed theory justifies my belief that they are waking experiences. And I would add that such conformity does *not* obtain in the case of dream experiences.

8

Discussion *

Question by Dr. Hanson of the University of Minnesota: Professor Margenau, I'd like to pose one question to Professor Landé. Since he knows that I ascribe to the Copenhagen interpretation perhaps more enthusiastically than is good for me, perhaps I may put it somewhat rhetorically. The point is this: Landé is perfectly right in saying that there is a sense in which, for example, if an accident took place on the road I could kick in its direction, but I could not kick at the probability amplitude of the density of accidents on Sunday. That is perfectly true. But, on the other hand, there is something, for example, in the beta-ray experiment, the diffraction patterns there, which seems to be perfectly kickable at, in exactly the same way that we were able to use the fact of periodic distributions in the Laue experiments on x-rays to settle that there was something wavelike there. I think we are perfectly correct in taking seriously the fact that in the Davisson-Germer experiments and in the G. P. Thomson work there was something that had to be taken seriously; to identify this kind of distribu-

* Transcribed from a tape recording.

tion with the sort of distribution that we have of the density of accidents on a Sunday seems to me a bit quick and I felt unhappy about it. It is also worth while noting, I think, that the 1923 paper by Duane that he mentions certainly points out that this was not very carefully read by most physicists. If what the 1923 paper asserts is true, the consequences throughout the whole history of our understanding of physics are going to be serious indeed. It is just like the 1925–1926 papers by J. J. Thomson, in which, for example, he tries to explain the Compton effect in terms of a pure wave theory (radiation pressure, etc., which is wholly qualitative); it is perfectly clear that no one has ever read them—not seriously, and if he was correct there would be, you might say, an ante-Landé and an ante-Born approach to the whole problem, and I was just wondering how he would react to either of these possibilities.

Landé: My comment is that, to adhere to the dualistic theory in which neither waves nor particles are real, nor quite unreal, is a complicated point of view, and as I said, the enormous literature produced day after day in order to make this duality more palatable may be a sign that basically we are not satisfied with it. We want to have a unitary theory; and (if I may use the word) even after thirty years of persuasion we still want a unitary theory. In fact, Born thirty years ago proposed the beginnings of a unitary theory in which even the diffraction experiments with maxima and minima, which look so very wavelike, still are interpreted in the same unitary way, as confirmed by the statistical build-up of the diffraction patterns. According to the duality, or let us say according to the original opinion, all these were "quantum miracles." The electrons seem to misbehave. They ought to behave according to the rules of classical mechanics but they simply, very strangely, obey wave laws. This obeying of wave laws has been codified and raised to an article of faith for the last thirty years. I think that as long as we believe that electrons misbehave we do not understand. But I think we could make this misbehavior, these miracles, understood by going a little deeper into the formal background of the interference law and the periodicity law. Both are, in my opinion, quite natural and simple, and have to be so according to what I said before.

The Chairman invited further questions and comments from the floor.

Saul A. Basri: It seems to me that as human beings the only things we can be sure of are our sensations and thoughts, and that because of this we assume that our sensations are due to things outside us which we call the macroscopic world. In other words, the existence of the macroscopic world is an assumption to explain our sensations, perhaps to put order into our sensations; and that the microscopic world is another assumption of the same kind. In other words, we assume the existence of electrons in order to explain the properties of the macroscopic world which we assume exist to explain our sensations. It is this which I wonder whether you, Professor Margenau, or Professor Bridgman or Professor Landé would like to comment on.

Chairman: Professor Bridgman?

Bridgman: I never use the word "reality"; I was just talking about the way I thought other people used it.

Basri: But would you agree with that view, or would you disagree?

Bridgman: Would I agree that other people say that things are real?

Basri: No, with this particular view of looking at physical reality through the macroscopic and microscopic world.

Bridgman: Well, I say I don't use the word. You can get along without it.

Basri: Would you say this is what you understand by the microscopic world?

Bridgman: Yes, I think the microscopic world has to have its explanation. Your language about the microscopic world is ultimately reducible to the language you used to describe the macroscopic world.

Margenau: Since I was included in this interrogation, may I say briefly what my view of reality is. I agree with Professor Bridgman that the term is obnoxious, that it has a great variety of meanings and causes a great deal of confusion in the minds of people, including physicists. One might therefore be well advised to shun its use. However, it seems to me that, if people 300 years ago, physicists 300 years ago, had decided not to use the word "force," which was as vague as is the word reality today, physics might not have developed as rapidly, as consistently, as it has.

There are two ways in which the difficulties arising from the diffusive usage can be remedied. One is to prohibit the use of a diffuse term. The other way is to make it more precise. This is indeed what happened to the terms "force," "energy," "momentum," which now beneficently infest the realm of physics. I don't believe that we should now have any objection to their use, because they have been refined, because they have been made definite. What I propose is to make the meaning of the term "reality" equally definite.

Let me now turn to the question of the interrogator. Yes, indeed, the role of theoretical physics may be regarded as an artifact to make what I would like to call sensory reality meaningful; to make sense impressions, observations, and so forth, coherent; to bestow upon them a degree of organization, of cohesion, of lucidity which they in themselves do not possess. We do this, I think, not so much by a direct appeal to the ontological existence of an external world, microscopic or macroscopic. To be sure, there are many who pursue that course. But this is not necessary in science. We can, in a sense, following Kant, investigate the processes by which we do reach trans-subjective certainty.

The process, as I see it, is something like this. One matches the immediate experiences against a realm of rational common sense. One establishes rules of correspondence between the immediately perceived flash of light and the construct light which is well known to the theoretical physicist, the construct which involves the idea of electromagnetic fields, and so forth. One sets up a correspondence between this immediate experience and these constructs. Now the constructs are so chosen in the first place that they make for what I call metaphysical satisfaction. They must satisfy economy of thought; they must be logically fertile; they must lead to consequences which can be observed. This is one set of requirements to which the so-called constructs may be subjected, *must* be subjected. This set of requirements, in contradiction to an understanding which had arisen in Dr. Seeger's mind, is not absolutely fixed. I think I said in my own talk, and I have written, that they are pragmatic devices, although I chose to call them metaphysical. Metaphysics can be pragmatic, can be tentative, and all that.

We impose upon our constructs these rules; then we see whether they agree through their deductions with empirical observations. So there are two classes of requirements: those called methodological or metaphysical, and those called empirical. When both are met we declare: what was originally merely a construct has now become what I have termed a verifact. At this point, since I was asked, "What is physical reality?" I would answer, *the verifacts of the physical sciences.* This is to me the most satisfactory way of stabilizing, of refining, the concept of physical reality.

Bridgman: May I ask one question? Is this reality which you describe this way unique?

Margenau: If you mean unique in the sense of categorical, clearly distinct from everything else, my answer is no. There is nothing in human experience that can be said to be unique, distinct, and valid in a self-declaratory sense. I would apply this even to the laws of logic. Certainly there are no pigeonholes in human experience. In other words, the concept of reality, or physical reality, which I have evolved in so inadequate and brief a fashion, is not one that allows you to confer the judgment, "This is real," "This is not real," upon all entities that interest the physical scientist. There are shadings. There are instances in which we are not willing to say, "This is real, and this is not."

Many of these instances are of no interest whatever. I suppose most of us would be willing to concede that electrons are real. When you come to the mass of an electron, would you say it is real? Well, you might say, yes it is, but in a sense it is not. Are qualities real? Are attributes real? Are they real in the same sense as what Aristotle called substances, and what we now call physical systems (electrons, atoms, stars)? Such questions are to me uninteresting, and I should not wish to answer them except by statutory definition. In a sense, then, physical reality is not unique.

However, it seems to me that if an approach like the one outlined were employed, the term "reality," the concept of reality, might gradually, progressively, attain a measure of precision if not uniqueness which is going to help the scientist, especially in his relations to the philosopher. It is my conviction that what the physicist ought to do is to try to refine his terms, endeavoring to make the philosopher see what he is doing, to adjust his own language to philosophic language, rather than slam the door upon the philosopher, rather than setting up an iron curtain and establishing one more specialty in the large domain of human concerns. And I think that by trying to make things more precise, by trying to show the philosopher what the physicist means by his terms, and, reciprocally, by inducing in the physicist an attitude which inclines him to listen with attention and understanding to what the philosopher is saying, we are going to bridge one of the deep crevasses that divides and bifurcates our culture.

John Forwalter: It seems to me that if we had an able semanticist present he might resolve our phenomenological problem somewhat. Professor Bridgman, of course, does so in terms of symbol and reality. However, much of the discussion seems to be a denial on the physicists' part that there is a reality. Perhaps this was accidental and can be cleared away easily. Really, the framework for clearing it away is present in many of the papers, such as that a distinction between microscopic and macroscopic leads us on to the notion that reality is in many levels or, if you prefer, seen through the eyes of many disciplines, and with the proper instruments we work within one of these levels. But the distinction a semanticist might make would be to warn us when we are talking about interpretive matters and when we are talking about, let us say, the data that we get from our instruments. The scientist would take the data, he would find some relationships among them—this is an old-fashioned way of looking at science, I recognize—he would make some constructs, he would relate them to a general network of knowledge, and then he would apply various tests, and I think a logician might have found that our tests for scientific truths were not all-encompassing; and then the scientist would end it here. Perhaps this is far enough to go with it. Even our philosophers present did not seem to go beyond to a necessary relationship between the facts as physics finds them, or the interpretations it puts upon them, and an older way of looking at reality. If comment is necessary it should be—I should like to have it—along the lines of making some of these distinctions or of emphasizing the over-all approach of science in terms of various levels of knowledge.

Margenau: I believe there were no specific questions here; I thought the remarks very illuminating and interesting.

Question from the floor (name not available): I should like to comment on Professor Collingwood's paper. I think he was trying to say that in addition to the primarily quantitative structure of physics there is a kind of qualitative knowledge, and so the question is this—in addition to the general principles that you have discussed as physicists that grew beyond the proper body of physics itself, do they not also assume a knowledge of things and common-sense use of these words which is also outside of the general volume of physics? If they do not make this assumption they would not be able to claim that they are using the same measuring rods on two different occasions when they are moving from place to place.

Margenau: I think there are two questions being asked. One is, must the scientist distinguish between quantitative and qualitative aspects of science? Must he acknowledge the necessity of qualitative judgments? The second question is, are there not certain peculiar qualitative things assumed even in physical science—things like the entities to which we assign quantitative properties? Am I stating your questions correctly, sir?

Questioner: Well, I think that if the physicist did not make this kind of assumption—would he know how to find and recognize ordinary common-sense things?

Margenau: There has to be a certain degree of, shall I say, substantiality, a certain degree of permanence in the systems to which the scientist ascribes measurable quantities. Now, with respect to the latter question, I would answer as follows: Yes, indeed, there is an Aristotelian hangover even in modern quantum physics. Because we still acknowledge the existence of electrons; the existence of neutrinos; the existence of what we generically call systems, although they are not directly sensible. The modern term *system* is the counterpart of Aristotelian substance; and the modern term *quantity* is the counterpart of the Aristotelian accidents. I wonder here if Mr. Collingwood would agree with me.

Collingwood: Quantity means the same in both contexts, I think.

Margenau: I would suppose that in every stage of science a distinction between quantitative and qualitative attributes of things is necessary. We never get around it. However, as I read the history of science I seem to observe a progressive elimination of qualities in favor of quantities. Qualities are supposed to be elusive things, incapable of being approached, at any rate captured, by an application of scientific methods.

Let me give you an example of what I have in mind. Some fifty years ago, color was one of these esoteric qualities that could not be measured, and the reason was this: people stated the attributes of color, defined color, in terms of two observables, or "quantities," if you please. One was hue—wavelength; and the other was intensity. Now it is true that an artist could paint two canvases in blue, both having the same intensity and hue; and yet the two blues would look different to the eye of the observer.

Here then arose the claim that color is a quality, something that escapes the net of the scientist; something that is esoteric, that is really not tractable by the methods of science. Now what has happened is this: someone discovered that there is a further quantity, a third observable, involved in this business, an observable called saturation. Now if you paint two canvases in blue in such a way that they agree with respect to hue, intensity, and saturation, they also *look* alike. And so you have here the conversion of what was at one time called a quality, not tractable by the methods of science, into a scientific quantity capable of numerical quantification. And it seems to me that this process is going on forever. We cannot say that everything in the world that we regard as a quality will some day be converted into a quantity. This is a question of faith—not one of the maxims, metaphysical or otherwise, of science. I myself believe that this process of conversion of qualities into quantities is going on as long as the human mind inquires.

Grünbaum: I just wanted to remark—I thought I detected in Professor Collingwood's paper and also in the discussion something to the effect that quantifiability in the usual sense of metrizability is a necessary condition for scientific tractability. Now surely this is not so. After all, there are topological problems, for example, in general relativity theory, which are as scientific as other problems, and they are certainly not quantitative problems in the usual sense. So it seems to me somewhat dangerous to concede first of all that mathematics is the science of quantity; I think the domain of mathematics had better be left undefined because it is an elastically conceived domain which changes. And I think it is also pregnant with misleading potentialities to talk about scientific tractability in terms of quantifiability in the sense of metrical scales of measurement. Conceptual circumscribability, axiomatizability, articulatability are at least some of the crucial requirements for scientific tractability; not necessarily, it seems to me, quantifiability in the sense of scales of measurement.

Margenau: I quite agree. One must not take the term quantification in too literal and arithmetical a sense. However, I think it might be maintained that even topology is based on measurability and on numbers. I chose the term quantifiability, quantification in a larger sense; but it is certainly important to point out as Dr. Grünbaum has done that science at all stages does deal with qualities in the sense of noncontinuous, nonquantifiable, nonarithmetical entities.

Question from floor (name not available): Professor Margenau, would you care to comment on Professor Seeger's remark that your deductive method is simply tautological? You cannot get anything new. So, what do you think about the axiomatic method?

Margenau: May I say that I agree with almost every contention made by Dr. Seeger, but I do not agree with his reading of my arguments. As a matter of fact, I hoped it would be explicit that I do not rely solely upon the axiomatic method. The use of the axiomatic method amounts to this: one starts with postulates—these postulates are part of the axiomatic system. From these axioms one derives theorems, as many as possible; one wishes to exploit fully the logical contents of the axioms. So one spins out these theorems. These theorems are then related to what one might call empirical nature or perceptory nature, by certain correspondence rules, epistemic correlations which link, for example, the point in the mathematician's sense with the dot of chalk on the blackboard, the force in the sense of Newton's law with the force which Professor Landé experiences when he kicks an object. These are not the same entities logically; they are correlated by these rules of correspondence.

Now what the axiomatic method achieves is this: it makes quite explicit everything that is contained within the postulates at the start of the scientific system. It does not make them true. Reliance on the results of this analytic process of spinning out from postulates what they contain logically will never confirm, will never establish empirical truth. It will establish internal consistency. Now of course errors in science are of various kinds. One can easily go wrong in mistaking an observation, making an error in a reading. One can also go wrong in misreading the implications of an axiomatic system. Now the latter kind of error is avoided by an effort on the part of scientists to set up an axiomatic scheme for every science. When that axiomatic scheme is at hand, one can see whether the results it yields are in agreement with observations. Then comes the matter of empirical confirmation, validation; and when you have both these things, namely the axiomatic scheme providing facilities for deductive procedure together with the empirical, the inductive pursuit which starts at the other end of the range of our experience, then you have what, not I, but everybody in science, calls an exact science. We distinguish exact sciences from correlational, or inexact, or descriptive sciences. This is a technical term which I use. That does not mean that that kind of science is necessarily more exact because it uses axioms. It is more exact logically, yes. But it may be wrong. Exactness in this sense does not imply correctness. I wonder if I have answered your question, sir.

Questioner: You say that the deductive method could then be used to get further results; but it gives nothing new, for no new knowledge is accumulated.

Margenau: This depends upon what you mean by "nothing new." If you mean nothing nonanalytic, nothing synthetic, can come out of it, then of course I should agree. But, you know, new things do emerge analytically sometimes. Look at the theory of numbers. The finding of all the prime numbers is a purely analytic pursuit. But there's a lot of novelty in finding the million and seventh prime number. We must not confuse here novelty with analyticity. If you mean analyticity, then what you say is in my opinion correct. But if you mean novelty in the manner of surprise, of unexpectedness, in the manner of not having been able to predict it simply, then of course the analytic process, the deductive process, can lead to novelty. You may have a nasty differential equation which may take years to solve; and some day a mathematician succeeds in finding its solution. What he has done is to establish an analytic consequence of the differential equation according to its boundary conditions, or initial conditions, and so forth. And yet there is a great deal of novelty in that solution.

Grünbaum: Contrary to Jeans, God is not a pure mathematician—that is the one thing He is utterly bored by. Jeans said that God is a pure mathematician. What I am trying to say is, since God would presumably see all the implications of any postulate system, He would not be spending His time proving theorems.

[*Question from floor (not intelligible in the recording).*]

Margenau: Professor Bridgman has been accused by the questioner of having said somewhere that all knowledge, mathematical and otherwise, is ultimately empirical, that geometry is a branch of physics. Professor Bridgman has been asked to reply.

Bridgman: I never said that. I certainly recognize two kinds of geometry. There is a geometry of postulates, and a geometry of physical measurement.

Questioner: I still do not understand that Professor Margenau could believe that something could be derived from a set of axioms that is not already known.

Margenau: I am being accused of inconsistency in claiming that by using analytic procedures, that is, by deductive procedures, one can deduce something, one can obtain something, that is not already known. Now this is so easy an allegation to come back at that I fear I must have misunderstood the point of the question. Let me give you, very briefly, the example I chose before—a differential equation has solutions, and these solutions certainly are not known in the beginning. The process of solving the differential equation is often not a cut-and-dried affair, it often relies on human ingenuity; there may be no general rules for doing this. The process of solving this equation certainly has all the aspects of originality, and the results have all the earmarks of novelty. And certainly the solution of the differential equation is not known in the beginning. It is implied logically, yes. But it is not known. This is the simple remark I would make in answer to your question, but forgive me if I misunderstood it.

Notes

2. *Is "Physical Knowledge" Limited by Its Quantitative Approach to Reality?*

FRANK J. COLLINGWOOD

1. For other interpretations, cf. B. L. Van Der Waerden, *Science Awakening*, p. 125; and O. Neugebauer, *The Exact Sciences in Antiquity*, pp. 143–144.

2. In the famous illustration by means of the divided line Plato gives in a very concise form his view of the role that mathematics can play in advancing human knowledge of the truth. The line is divided once into two segments, one representing visible things, the other representing intelligible things. The first segment is then divided into two parts, one representing mere images such as shadows and reflections on water, the other representing trees, animals, and artifacts which are the models of the images. The segment representing intelligible things is divided into two parts, the first of which represents the realm which the soul investigates by treating as images the things represented in the second part of the segment representing visible things. This realm is the one in which the mathematician exercises his ingenuity. He makes use of visible forms and talks about them, but in truth he is thinking of the mathematicals of which the visible forms are but a likeness. "And do you know also that although they make use of the visible forms and reason about them, they are thinking not of these, but of the ideals which they resemble; not of the figures which they draw, but of the absolute square and the absolute diameter, and so on . . . the forms which they draw or make, and which have shadows and reflections in water of their own, are converted by them into images, but they are really seeking to behold the things

themselves which can only be seen with the eye of the mind." *Republic,* VI, 510 C ff.

3. *Republic,* VII. 525 B–C and 526 A–C.

4. *Republic,* VII, 522 C–E and 526 D. *Republic,* VII, 531 C. But he complains that these musicians fail to "ascend to generalized problems and the consideration of which numbers are inherently concordant and which not, and why in each case." Thus he criticizes them for failing to penetrate to the basic principles of number.

5. *Republic,* VII, 522 C.

6. "It is by means of problems, then," said I, "as in the study of geometry, that we will pursue astronomy too, and we will let be the things in the heavens, if we are to have a part in the true science of astronomy and so convert to right use from uselessness that natural indwelling intelligence of the soul." *Republic,* VII, 530 C. Cf. the immediately preceding part of the text for the censure of the astronomers of his time. Cf. the last part of the text in note 8 below, where the abstract mathematical formulas are seen as containing the concrete motions of the heavenly bodies.

7. *Socrates:* For instance, were we to eliminate from all arts those of numbering, measuring, and weighing, what would be left of any of them would, broadly speaking, amount to very little. *Philebus,* 55 E. Cf. also *Epinomis,* 977 C–E.

8. "Thus," said I, "these sparks that paint the sky, since they are decorations on a visible surface, we must regard, to be sure, *as the fairest and most exact of material things;* but we must recognize that they fall far short of the truth, the movements, namely, of real speed and real slowness in true number and in all true figures both in relation to one another and as vehicles of the things they carry and contain." *Republic,* VII, 529 D–E.

9. To the man who pursues his studies in the proper way all geometrical constructions, all systems of numbers, all duly constituted melodic progressions, the single ordered scheme of all celestial revolutions, should disclose themselves, and disclose themselves they will, if as I say, a man pursues his studies aright with his mind's eye fixed on their single end. As such a man reflects, he will receive the revelation of a single bond of natural interconnection between all these problems. *Epinomis,* 991 E–992 A.

10. *Timaeus,* 31 B–32 C.

11. *Timaeus,* 53 C–D.

12. *Timaeus,* 52 D–53 B.

13. Cf. *Phaedo,* 96 A ff., where Socrates tells how in his youth he desired to know that part of philosophy called the investigation of nature, but in pursuing its questions his eyes grew blind to things that he seemed to know quite well, for example, that the growth of man is the

result of eating and drinking. Now he does not believe that he understands the reason why one or anything else is destroyed, or generated, or is at all.

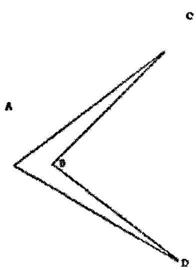
14. *Republic,* 511 D.

15. *Physics,* ii, 2, 194a7–12 (Oxford translation), and APo. I, 13, 78b37–79a2.

16. The following example of how geometry is used to prove propositions in optics is from Heath's *Mathematics in Aristotle,* Oxford, 1949, p. 59. "The dependence of optics on geometry is seen in all books on optics from Euclid's *Optics* onwards. Philoponus illustrates by the propositions 'Things seen from afar appear smaller, things seen near at hand appear larger' (Euclid's *Optics,* Prop. 5).

"Suppose an object *CD* seen by an eye at *A,* so that *AC, AD* are the extreme 'visual rays,' as the Greeks called them. The object is therefore seen in the angle *CAD.*

"Now suppose the eye moved nearer to the object, say to *B; BC* and *BD* are then the extreme 'visual rays.'



"As *B is* within the triangle *ACD, CB* and *DB* are straight lines drawn from the ends of the base of a triangle to a point within the triangle. Hence (Euclid, I, 21), the straight lines *CB, BD* are together less than the straight lines *CA, AD,* but include a greater angle. Therefore the angle *CBD* is greater than the angle *CAD.* But (Euclid, *Optics,* Def. 4) 'Things seen under a greater angle appear greater and under a lesser angle less.' Therefore from *B* the object *CD* appears greater than it does from *A.*"

17. *Metaph.,* xi, 4, 1061a29–1061b25.

18. Cf. P. Duhem, *The Aim and Structure of Physical Theory,* translated by Philip P. Wiener, pp. 132 ff., for an excellent account of the approximative nature of all measurement. Cf. also the work mentioned in note 20 below, p. 275.

19. Aristotle seems to have been aware of the approximative nature of practical mathematics. "The minute accuracy of mathematics is not to be demanded in all cases, but only in the case of things which have no matter. Hence, its method is not that of natural science." [*Metaph.,* ii, 2 (995a 15-16).] I interpret this text as saying that absolute precision is characteristic of pure mathematics and is not to be demanded of any practical mathematical science. Contemporary physics is an example of such a practical mathematical science.

This being so, it would logically follow that Aristotle would not make the mistake of conceiving speculative mathematics, the geometry codified by Euclid a half century after Aristotle's floruit, for example, as a science of actual quantities. "Obviously physical bodies contain surfaces and volumes, lines and points, and these are the subject-matter of mathematics. . . . now the mathematician, though he too treats of these things [surfaces, volumes, etc.], nevertheless does not treat of them as the limits of a physical body; nor does he consider the attributes indicated as the attributes of such bodies. That is why he separates [abstracts] them; for in thought they are separable from motion, and it makes no difference, nor does any falsity result, if they are separated. . . . While Geometry investigates physical lines, but not qua physical, Optics investigates mathematical lines, but qua physical, not qua mathematical." [*Physics*, ii, 2 (193b25–194a10).] This text clearly indicates that a discussion of Euclidean versus non-Euclidean space would seem absurd to Aristotle, for the characteristics of the idealized space of plane geometry are a result of abstracting from the actual dimensions of existing things. If one separates the notions of line, surface, and volume from mutable and moving things, he should not expect to find their characteristics as mutable and moving in his abstractions; nor should he hope to measure actual things by his abstractions.

20. Philipp Frank, in his *Philosophy of Science*, contemns the act of *"seeing" with the intellect* as being essentially a pathway to self-delusion in a metaphysics quite divorced from physics. He fails to account, however, for the *seeing with the intellect* that has elaborated all mathematical systems and all theoretical systems of physics. In fact, there is no possibility of having any object of scientific knowledge until it is seen by the intellect as having some possibility of yielding certain knowledge of a universal kind. I take it that everyone agrees that what is seen by the sense of sight, being material, is essentially changeable and therefore not a fit object of scientific (true) knowledge. I think the Platonic-Aristotelian disputation made perfectly evident the impossibility of having a true knowledge of the object of the sense of sight, precisely and only as an object of this sense. The stability that any object of scientific investigation has is given to it when it is conceptualized; considered in its state of material existence such an object is essentially mutable.

3. *Does Physical "Knowledge" Require* A Priori *or Undemonstrable Presuppositions?*

HENRY MARGENAU

1. Mainly through the researches of R. Carnap, *Logical Foundations of Probability*, University of Chicago Press, Chicago, 1950.

2. P. W. Bridgman, *The Logic of Modern Physics*, The Macmillan Company, New York, 1927.

3. Henry Margenau, *The Nature of Physical Reality*, McGraw-Hill Book Company, New York, 1950.

4. Paul Deussen, *Metaphysics,* Macmillan and Company, London, 1894.

5. There are men who feel that previous illicit use has poisoned the word *metaphysical* forever, and they prefer the term *methodological.* This is all right with me, provided that I am not required to abandon every word (e.g., force, action, energy) that has once suffered from grotesque interpretation.

6. Philipp Frank, *The Validation of Scientific Theories,* Beacon Press, Boston, 1954.

7. Alfred Landé, *American Journal of Physics, 108,* 891, 1957; *Foundations of Quantum Theory, a Study in Continuity and Symmetry,* Yale University Press, New Haven, 1955.

8. See A. Pap's incisive study, *The A Priori in Physical Theory,* King's Crown Press, New York, 1946; also *Semantics of Necessary Truth,* Yale University Press, New Haven, 1958.

9. I do not wish to affirm this for all concepts of divine being; theology, in its clearer phases, does use methods of reasoning similar to those of science and thereby removes some of the strictures implied in this critique.

4. *Does "Knowledge" of Physical Laws and Facts Have Relevance in the Moral and Social Realm?*

GEORGE P. KLUBERTANZ, S.J.

1. His two major works are *Principia Ethica* (Cambridge University Press, Cambridge, 1903) and *Ethics* (Oxford University Press, New York, 1912).

2. See, for example, the presentation of these arguments in two of the better-known contemporary British writers, R. M. Hare, *The Language of Morals* (Clarendon Press, Oxford, 1952); P. Nowell-Smith, *Ethics* (Penguin Books, Baltimore, 1954). But this does not mean that the analysts intend to support egoism or hedonism; see Nowell-Smith, *op. cit.,* pp. 133–144, or the summary given by T. E. Hill, *Contemporary Ethical Theories* (Macmillan, New York, 1950), pp. 25–26.

The analysts are not alone in condemning the naturalistic fallacy. See, for example, Patrick Romanell, *Toward a Critical Naturalism* (Macmillan, New York, 1958), p. 63, and F. S. C. Northrop, "Cultural Values," in *Anthropology Today,* edited by A. L. Kroeber (University of Chicago Press, Chicago, 1953).

3. See Nowell-Smith, *op. cit.,* pp. 75–91; Hare, *op. cit.,* pp. 79–93; J. O. Urmson, *Philosophical Analysis* (Clarendon Press, Oxford, 1956), p. 52.

I have not dealt with the Kantian *a priori* for several reasons. For one thing, the arguments against intuitionism also count against *a priori* forms. For another, the Kantian ethic is formal and "empty"; as Max

Scheler pointed out, it is the material content that is important. Thirdly, the Kantian ethic is one of duty; it does not recognize the moral ideal, which is more inclusive than duty.

4. See, among others, Romanell, *op. cit.;* Northrop, *op. cit.;* Alexander Sesonske, *Value and Obligation* (University of California Press, Berkeley, 1957); Maurice Mandelbaum, *The Phenomenology of Moral Experience* (Free Press, Glencoe, Ill., 1955); Morris Ginsberg, *On the Diversity of Morals* (Macmillan, New York, 1957); Abraham Edel, *Ethical Judgment: The Use of Science in Ethics* (Free Press, Glencoe, Ill., 1955) (his new book, with May Edel, *Anthropology and Ethics* [Charles C Thomas, Springfield, Ill., 1959], should be a further development of his ideas).

5. Existentialist philosophers insist over and over that man has no essence or nature. If this were meant literally, in the usual meaning of the words, there would be neither good nor evil; everything would be indifferent and arbitrary. But the existentialists also admit that there is an objective "human situation"; and, even in the most radical formulation, there is a distinction between "authentic" and "inauthentic" acts. By the word *essence,* therefore, they mean "an enclosed and necessitated essence"; yet, if man is to be defined as consciousness and freedom, he does have a nature, though in a different sense from the nonhuman "objects" of the world.

6. If, with Romanell, *op. cit.,* p. 55, we were to define "scientific method" as "the continuous commitment to base conclusions on evidence," an ethics proceeding in the way here described could justly be called a "scientific ethics." For a similar definition of "scientific," see also Harold K. Schilling, "Teaching Reciprocal Relations between Natural Science and Religion," in *Teacher Education and Religion* (American Association of Colleges for Teacher Education, Oneonta, N. Y., 1959), for whom the term "may refer to the general method of intelligence which, in attempting to solve problems or answer questions, proceeds logically, basing conclusions on evidence and avoiding bias and prejudice" (p. 261), and compare the usage of James B. Conant, *Science and Common Sense* (Yale University Press, New Haven, 1951), pp. 42–62.

Yet this use of "scientific" is not to be altogether recommended, since the term *science* is commonly used in several senses; on this ambiguity and the possible misunderstandings that may arise from it, see Robert J. Henle, S.J., "A Philosopher's Interpretation of Anthropology's Contribution to the Understanding of Man," *Anthropological Quarterly, 32,* 29–31, 1959.

It should be noted that I am not here entering into the distinction between the "knowledge of man as subject" and the "reduction of man to the status of an object" (alleged to be the necessary consequence of a scientific approach to man), so much insisted on by Gabriel Marcel and other existentialists.

7. On the contribution of anthropologists to a better knowledge of man, and to a clearer delineation of the common needs and tendencies of man, see, for example, Robert Redfield, "Anthropology's Contribution to the Understanding of Man," *Anthropological Quarterly, 32,* 3–21, 1959; Clyde Kluckhohn, "Universal Categories of Culture," and David Bidney, "The Concept of Value in Modern Anthropology," in *Anthropology Today,* edited by A. L. Kroeber (University of Chicago Press, Chicago, 1953). See, also, George St. Hilaire, S.J., "Cultural Relativism and Primitive Ethics," *The Modern Schoolman, 36,* 179–195, 1959.

Since writing this paper, I have had the opportunity to read Professor Bridgman's latest book, *The Way Things Are.* Professor Bridgman says, "There is something unique back of such a code [that is, a public code] which to a certain extent determines it, independent of the particular culture, namely, the traits which all human beings in all cultures have in common and which condition the things which any human being will find desirable. . . . From this point of view the attempts of anthropologists and humanists to find a universal basis for human morals have a justification in nowise tainted by metaphysics." (Harvard University Press, Cambridge, 1959, p. 267.)

Remarkably similar conclusions are being reached by a number of psychiatrists, who point out the destructive effects of the denial or perversion of the basic tendencies of man.

8. Readers familiar with Aristotelian or Thomistic philosophy will recognize that this approach is only a partial analysis of the goal-directedness ("finality") of human nature. I have avoided the more common terminology, partly because it is often misunderstood, partly because in the limitations of the space allotted to me I would not be able to present adequately the entire argument from "finality." On this point, an interested reader can profitably consult two excellent articles by John Wild, "Tendency: The Ontological Ground of Ethics," *Journal of Philosophy, 49,* 468–472, 1952, and "Nature Law and Modern Ethical Theory," *Ethics, 63,* 1–13, 1952.

9. On the notion of "the reasonable" and the various ways in which it is determined, see Thomas E. Davitt, S.J., "St. Thomas Aquinas and the Natural Law," in *Origins of the Natural Law Tradition,* edited by Arthur L. Harding (Southern Methodist University Press, Dallas, 1954), pp. 26–46.

10. "What is morally good" includes more than "what is of obligation." Once the basic meaning of "moral good" has been established, positive obligation can be found by means of a further specification. The argument would be developed, as I see it, along the following lines. Nature is dynamically orientated, and this orientation is discovered through man's natural tendencies. Moreover, one's own nature is a given in which reason itself is contained. Practical reason proceeds from goals whose suitability ("fittingness") for man is within limits determined for man. Therefore,

man's judgments are found to be *directed.* Now, in the course of practical reasoning, some actions (means) are found to be compatible with the natural goals without being a *sine qua non* of them; and in this case we find moral good desirable and ideal but not necessary. Other actions, however, are found to be so related to the goals that the latter cannot be seriously striven for without the former: in this case, there is obligation. Finally, when man realizes that there is a moral (or, practical) absolute to which he is directed, and that he is responsible to a personal higher being, then moral obligation is experienced in its fullest sense.

11. According to the terminology I am using, "facts" are the "things existing in space and time, and their relations" (cf. Morris R. Cohen and Ernest Nagel, *An Introduction to Logic and Scientific Method* [Harcourt, Brace, New York, 1934], pp. 217–219), or the events which occur (have occurred, will occur), as well as the propositions directly describing them. "Laws" are "generalizations which assert invariable sequence, or conjunction, or functional relationships between relatively directly observable or measurable magnitudes" (cf. Cohen and Nagel, *loc. cit.,* and Herbert Feigl, "Some Remarks on the Meaning of Scientific Explanation," in *Readings in Philosophical Analysis,* edited by Herbert Feigl and Wilfrid Sellars [Appleton-Century-Crofts, New York, 1949], pp. 511–512). "Theories" are higher-level generalizations (Feigl, *loc. cit.*) which consist of assumptions to interpret laws and facts; they constitute the hypotheticodeductive moment in science (F. S. C. Northrop, *The Logic of the Sciences and the Humanities* [Macmillan, New York, 1947], pp. 59–75); they are "constructs" (Henry Margenau, *The Nature of Physical Reality* [McGraw-Hill, New York, 1950], pp. 54–74), which take the place for the scientist for real intrinsic natures that he cannot directly grasp but only attains by a mixed experiential and rational process, the "empiriological method" (Jacques Maritain, *The Degrees of Knowledge,* translated under the supervision of Gerald B. Phelan [Scribner's, New York, 1959], pp. 21–67, 202–213). Theory, therefore, is the distinctive and characteristic element in modern science.

5. *Dualistic Pictures and Unitary Reality in Quantum Theory*

ALFRED LANDÉ

1. Albert Einstein, *Dialectica, 2,* 320, 1948.

2. Erwin Schrödinger, *Scientific Papers Presented to Max Born,* Oliver and Boyd, Edinburgh, 1953.

3. Max Born, *Philosophical Quarterly, 3,* 139, 1953.

4. Alfred Landé, *American Journal of Physics,* February 1959.

5. C. F. von Weizsäcker, *The World View of Physics,* Chicago University Press, Chicago, 1957, p. 33.

6. Henry Margenau, *Philosophy of Science Journal,* January 1958.

7. K. R. Popper, *The Logic of Scientific Discovery,* revised English translation, Basic Books, Inc., New York, 1959.

8. W. Duane, *Proceedings of the National Academy, 9,* 158, 1923.

9. Niels Bohr in *Einstein, Philosopher-Scientist,* edited by P. A. Schilpp, Library of Living Philosophers, Evanston, Illinois, 1949.

10. Alfred Landé, *Foundations of Quantum Theory, a Study in Continuity and Symmetry,* Yale University Press, 1955; *From Dualism to Unity in Quantum Mechanics,* Cambridge University Press, 1960.

6. *Metaphysics: Before or After Physics?*

RAYMOND J. SEEGER

1. P. W. Bridgman, *The Way Things Are,* Harvard University Press, Cambridge, 1959.

2. Richard von Mises, *Positivism,* Harvard University Press, Cambridge, 1951.

3. Alfred Landé, *Foundations of Quantum Mechanics,* Yale University Press, New Haven, 1955.

4. Philipp Frank, editor, *The Validation of Scientific Theories,* Beacon Press, Boston, 1954.

5. Henry Margenau, *The Nature of Physical Reality,* McGraw-Hill Book Company, New York, 1950.

6. Philipp Frank, *Philosophy of Science,* Prentice-Hall, Englewood Cliffs, New Jersey, 1957.

7. Max Born, *Natural Philosophy of Cause and Chance,* Oxford University Press, Fair Lawn, New Jersey, 1949.

8. R. J. Seeger, "Scientist and Theologian," *Journal of the Washington Academy of Sciences, 48,* 145, 1958; "Scientist and Poet," *American Scientist, 47,* 350, 1959.

7. *The Role of* A Priori *Elements in Physical Theory*

ADOLF GRÜNBAUM

1. For a concise recent statement of his views relevant here, cf. G. K. Herburt, "The Analytic and the Synthetic," *Philosophy of Science, 26,* 104, 1959.

2. P. Duhem, *The Aim and Structure of Physical Theory,* Princeton University Press, 1954, part II, chapter VI. This text, especially pp. 183–190, will not bear the reading given by K. R. Popper (*The Logic of Scientific Discovery,* London, 1959, p. 78) to the effect that Duhem denies only the possibility of crucial *verifying* experiments while allowing the possibility of decisively falsifying tests.

3. Adolf Grünbaum, "The Duhemian Argument," *Philosophy of Science, 27,* 75–87, 1960.

4. Henri Poincaré, *The Foundations of Science,* p. 76.

5. Albert Einstein, "Reply to Criticisms," in *Albert Einstein: Philosopher-Scientist* (edited by P. A. Schilpp), Library of Living Philosophers, Evanston, Illinois, 1949, pp. 676–678.

6. For further details on the ramified logical foundations of Poincaré's geometric conventionalism and on the fundamental ways in which it differs from the quasi-apriorism of Duhem, see Adolf Grünbaum, "The Duhemian Argument," *Philosophy of Science, 27,* especially pp. 83–87, 1960; "Conventionalism in Geometry," in *The Axiomatic Method in Geometry and Physics,* Amsterdam, 1959, pp. 204–222; "Geometry, Chronometry and Empiricism," in *Minnesota Studies in the Philosophy of Science,* vol. III (forthcoming); and "Carnap's Views on the Foundations of Geometry," in *The Philosophy of Rudolf Carnap* (edited by P. A. Schilpp), Tudor Publishing Company, New York (forthcoming).

7. Jacques Maritain, *The Degrees of Knowledge,* London, 1937, p. 207. A new translation of this work was published in New York in 1959 by Charles Scribner's Sons; all page references here are to the 1937 translation.

8. P. W. Bridgman, above, p. 21.

9. Maritain, *op. cit.,* pp. 201–212

10. *Ibid.,* p. 201.

11. *Idem.*

12. *Ibid.,* p. 202.

13. *Ibid.,* p. 203.

14. *Ibid.,* p. 207.

15. Cf. *ibid.,* p. 204.

16. Cf. *ibid.,* p. 204

17. *Ibid.,* p. 205.

18. Cf. *ibid.,* pp. 202, 205–206.

19. *Ibid.,* pp. 206–207.

20. Hilbert and Bernays, *Grundlagen der Mathematik,* Berlin, 1934, vol. I, §1, section A, pp. 2–3.

21. Cf. L. P. Eisenhart, *Coordinate Geometry,* New York, 1960, appendix to chapter 1, pp. 279–292.

22. Shankland, McCuskey, Leone, and Kuerti, *Reviews of Modern Physics, 27,* 167, 1955.

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