AN INTRODUCTION TO
THE PHILOSOPHY
OF SCIENCE

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AN INTRODUCTION
TO THE
PHILOSOPHY OF SCIENCE

By A. CORNELIUS BENJAMIN
Assistant Professor of Philosophy
University of Chicago

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To

My Wife

PREFACE

Recent years have witnessed the publication of a large number of monographs, magazine articles, and books, whose subject matter has seemed to defy classification. Though they have been written, for the greater part, by scientists, they are not properly scientific. They begin with science, they talk about science, and they end with science, yet they do not conform at all to the tradition of scientific writings. Were it not for the fact that they differ in important ways from the usual books on logic they might be placed in this class. Yet they are not logical in the usual sense. Their repeated reference to philosophical issues tempts one to classify them with this group, yet the writings approach these problems in a new spirit and with a new method, which seem quite foreign to the traditional philosophy.

Confronted by such a state of affairs, one has two alternatives. On the one hand, he may examine this extensive literature carefully, decide upon the existent discipline with which it has the closest kinship, and force it into this compartment. The essential disadvantage of such a procedure is that it does violence to the traditional terminology. On the other hand, he may simply collect all of the literature together and define a new discipline *ad hoc*. This method has a corresponding disadvantage, for it defines the field by the crude extensional method rather than by classifying its problems; one can say of the new discipline only that it involves problems like those discussed in such-and-such monographs, articles, and books.

In spite of this important difficulty the latter method seems to offer the greatest prospect of success. It prescribes neither limits to the field nor structure within its domain. It forces the problems into no predetermined form, yet it does not leave the field amorphous for it allows the problems themselves to take form according to their own characters. Furthermore, it permits the field to achieve more accurate formulation when the problems themselves have become more or less standardized and organized.

It seems certain that no finally satisfactory definition of the field in question is possible today. Not only are most of the problems still unsolved—that is quite to be expected in an embryonic science—but for the greater part the problems have not even been clearly stated or precisely differentiated from one another. It may be that the field has not yet reached that stage of development which permits even a tentative formulation of its boundaries and structure. Apparently the only way to decide is to attempt such a formulation and examine the results. If the theoretical description of the type of problem corresponds closely with the actual field of literature, the formulation may be looked upon with favor. Furthermore, the choice as to the most adequate term for describing the field is still an open matter. General use seems to have favored the term "philosophy of science," though the phrase is not without some disadvantages. Here, again, all that one can do is to use the term consistently in some well-defined sense, and see whether any confusion arises. Such a definition of the field of the philosophy of science will be attempted in the following pages.

The main practical considerations which have guided the author in the arrangement and selection of material should be stated at this point. The book has been written with a definite view to textbook use. In recent years there has been introduced into the colleges and universities of the country an increasing number of courses in the philosophy of science. Since the source material for such courses is to be found in some hundreds of books, many of which contain only a few pages of relevant subject matter, and in magazine articles printed in periodicals not commonly found except in the larger libraries, teachers have been somewhat at a loss for supplementary readings. The present book has been written in the attempt to include as much of this source material as is possible, consistent with a unified presentation. Where the material has not been actually quoted it has been referred to in footnotes and in bibliographical lists at the ends of the chapters. The aim has been to acquaint the student with the literature of the field, as far as it is available in English, and to indicate where the more detailed discussions of the various problems may be found. Reference material is drawn both from philosophers and from scientists, with, perhaps, undue emphasis on the latter, since sources of this kind are not commonly known to the average student.

In view of the textbook character, certain principles have been kept in mind in the selection and arrangement of material. No attempt has been made to give a comprehensive survey of the field of the philosophy of science. In fact there is some doubt as to whether the phrase "comprehensive survey" would have any meaning as applied to a field whose limits have not yet been determined. Many readers will be disturbed by what appear to be important omissions. For example, in Part I there is a more or less complete neglect of considerations pertaining to postulate schemes and the formal structure of thinking, and less emphasis on such topics as classification, analogy, definition, measurement, Mill's experimental methods, and the general problem of induction, than would seem proper in a book on the philosophy of science. The justification for these omissions and weakened emphases lies in the fact that a definite attempt has been made to avoid duplicating problems usually discussed in courses in logic. The intention has been to write a supplement to a textbook on logic rather than a substitute for it. Similarly, in Part II the concepts chosen for analysis are meant to be illustrative rather than exhaustive of problems in this field. It so happens that the concepts of the mathematical and physical sciences have undergone the greatest developments, and have been subjected to repeated analysis in recent years. They are thus especially valuable as illustrative of certain techniques of abstraction and idealization. An exhaustive consideration of problems at this level would require similar analyses of the concept of life, with its associated problems of mechanism, vitalism, and teleology; of the concept of society, and all of the problems of value which it raises; of the concept of consciousness, with its important problem of behaviorism *versus* introspectionism; and so on, perhaps even through the special concepts of economics, political science, law, religion, and art. It hardly seems the function of a general textbook in the philosophy of science to deal with problems of such scope.

Furthermore, the material has been chosen so as to make it extremely unlikely that the book would overlap with any of the texts in "introduction to philosophy by way of the sciences." Courses centering about such books have a definite function in the curriculum and there has been no intention of replacing them by alternative approaches. Survey courses of this kind pay much more attention to the subject matter of science than does the present book. The attempt has been made in the following pages to acquaint the student not so much with the facts of science as with the foundations of science. What is required for the philosophy of science, as Russell points out, is knowledge not of the complex features of science but of its abstract features—a knowledge of its first chapters rather than of its last chapters. This fact has permitted the elimination of many of the highly complex illustrations, particularly those drawn from relativity and quantum mechanics, which have obscured much of the literature in the field for the non-technical reader. Except in connection with the analysis of the basic concepts, where reference to the more complicated problems is unavoidable, illustrative material has been drawn from the more elemental theories, comprehensible to the average reader. It seems likely that the important philosophical features of science are located in the problems which the scientist finds wherever he turns his head—in classification, measurement, observation, hypothesis-formation, and so on—rather than in the comparatively rare problems associated with advanced sciences.

Special acknowledgment is hereby made to the following for reading the specified portions of the manuscript and suggesting valuable improvements: To Dr. P. L. DeLargy for examining Chapter V; to Professor Frank C. Hoyt for reading Chapter XV; to Professor Carl Eckart for many suggestions relevant to Chapters XIV and XVI; and to Professor Raymond W. Barnard for information pertaining to the definition of "number" in Chapter XIII. None of these men, however, is to be considered as in any way subscribing to the ideas presented in the portion of the manuscript which he examined. I wish also to express thanks to Mr. Richard Schlegel who helped in the routine work connected with the manuscript and prepared the index.

A. C. B.

CHICAGO, ILLINOIS
July, 1937

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INTRODUCTION

THE FIELD OF THE PHILOSOPHY OF SCIENCE

CHAPTER I

PHILOSOPHY AND SCIENCE

The defining of a field of study is always a precarious enterprise. The possible objects of knowledge are not labeled, like the magic cakes in *Alice in Wonderland*, "Eat me." One must learn what is edible and what is not by the treacherous process of trial and error. This fact forbids one to assert in advance of any investigation what is and what is not to be included in the investigation. One cannot say before science has begun what the possible objects of science are, for one can know whether an object is scientific only by trying to know it scientifically.

For this reason any preliminary delimitation of the field of the philosophy of science, and, derivatively, of the fields of philosophy and of science, must be viewed as tentative only, and as subject to later revision. Definition has two rôles to play in the development of a discipline. It serves first as an advance guard, preparing the way by a rough demarcation of the path and by elimination of the most formidable obstacles. In this function it cannot be examined or criticized on logical grounds; it is essentially psychological, and has merely an informative value. It is tentative and conjectural, often expressed in vague language, and serves rather to direct the attention of the listener to the proper locus than to give a complete and accurate description of its object. But definition also plays the part of a rear guard, organizing resources and fortifying the acquired territory. In this function it can justly be criticized on logical grounds, since it aims to indicate not merely the boundaries of its object but also something of the internal structure. The definition of the term "philosophy of science" which will be undertaken here must be considered as psychological or preliminary rather than as logical or final.

The character of the term determines the nature of the approach which must be made to its definition. It is obviously a composite term, taking its meaning from the more elemental terms constituting it. No definition of the philosophy of science is possible without a prior analysis of both philosophy and science. But these terms, unfortunately, have had a long history and a variety of meanings. Furthermore, if there is any uniformity at all noticeable in the historical uses of these words, it is such as to establish the essential opposition or antagonism of the two notions, and hence would compel one to conclude that the philosophy *of* science is a contradiction in terms. The fact seems to be, however, that the difficulties experienced in the attempts to define "philosophy" and "science," and the apparent incompatibility of the two terms, are themselves the main factors which are responsible for the emergence of the philosophy of science. Because of the heated controversy over the precise demarcations of these two fields, there arises a discipline whose precise task is the determination of such boundaries.

This fact determines the approach to the definition of the philosophy of science which will be made in the present chapter and the one which follows. A brief consideration of the historical problem will serve to introduce the thesis that the terms are rival claimants to knowledge. The outstanding attempts of recent and contemporary writers to eliminate this antagonism by assigning to each of the disciplines a certain type of subject matter or a certain type of method will then be examined. Each of these attempts will be briefly criticized with a view to determining what essential truth it contains. The ground will then be prepared for the positive attempt, in Chapter II, to construct a definition of the philosophy of science.

HISTORICAL APPROACH

History seems to justify the conclusion that the term "philosophy" in early Greek thought was relevant to no particular subject matter. The philosopher was one who loved wisdom without being greatly concerned as to the specific kind of object to which he directed his devotion. There is reason to believe that by the time of Plato and Aristotle the term had narrowed somewhat in scope and no longer included mathematics. At least it seems significant that the two greatest philosophers of antiquity were not mathematicians. Though Plato considered knowledge of mathematics an important preliminary to the study of philosophy, he apparently did not suppose it a part of philosophy proper. Aristotle wrote authoritatively on a wide range of subjects including logic, rhetoric, metaphysics, physics, astronomy, meteorology, cosmology, botany, zoölogy, psychology, ethics, and politics, and there is reason to believe that all of these were in some sense a part of his philosophy. It is probable that the shift of the center of civilization from Greece to Rome is responsible for the introduction of the word "science" into the vocabulary of culture, and it was quite natural that the term should be adopted to characterize those intellectual disciplines which were to be differentiated either by method or by subject matter from philosophy as the mother study. Scholasticism witnessed the separation of theology from philosophy. The revival of learning and the turning of man's attention to the natural world may have been responsible for the increased use of the term "science" to characterize the disciplines which had broken away from philosophy. Even as late as the sixteenth century a candidate for the degree of doctor of philosophy was regarded as equally competent to teach mathematics, astronomy, physics, metaphysics, logic, rhetoric, ethics, and politics. Christian Wolff, almost two centuries later, actually taught mathematics, physics, logic, psychology, practical philosophy, and political science at the University of Halle. The full title of Newton's great work, *The Mathematical Principles of Natural Philosophy*, determined the use of the term "natural philosophy" as a characterization of physics—a use which persisted until recent years. The term "mental philosophy" is still much in vogue in English universities. Many disciplines are even now in the act of breaking away from the mother stem. Cosmological speculations are on the border line between metaphysics and physics; psychology is still in many universities closely allied with philosophy; sociology is only with difficulty distinguishable from social philosophy; and the so-called normative sciences, ethics, esthetics, science of religion—attempting as they are to apply measurement techniques to the determination of value attitudes—are in the unfortunate position of having been denied philosophical status before they have been granted scientific recognition.

The generalization of this historical fact is clear. Philosophy is progressively losing ground, and the ground which is lost is promptly claimed by science. Philosophy, which once meant the totality of knowledge, now means the residue of knowledge after mathematics, theology, physics, biology, psychology, ethics, esthetics, and still others which only the future can disclose, have broken away and set themselves up on independent bases. Philosophy, which once was the glorification of knowledge, approaches indefinitely near to a position of complete extinction. Science, on the other hand, which was once non-existent, is taking over the field of knowledge and promises soon to occupy the enviable position which philosophy has been obliged to abandon.

It is reasonable to expect that no philosopher today would be willing to accept such a conception of the difference between philosophy and science. But it is curious to note that many have, on the basis of this historical accident, attempted to formulate theoretical conceptions of the nature and scope of each of these disciplines in terms of which the opposition may be understood. The assumption is that the gradual encroachment of science upon philosophy must be due to some basic antagonism between them. More fundamentally, it must be due to some specific advantage which science has over philosophy, either as to its subject matter or as to its methods of approaching nature. Hence the disciplines are defined around this opposition, and the studies become sharply differentiated from one another.

The result of this attitude has been a conception of the relation between science and philosophy which has placed investigators in the respective fields into warring camps, which has made any coöperative enterprises impossible, and which has made the notion of a philosophy of science a contradiction in terms. According to this view, science has stood for exact knowledge, verified conclusions, and progress; philosophy, for obscure ideas, conjecture, and stagnation. Science uses mathematical methods, which forbid fuzzy thinking; philosophy uses meditation, imagination, and speculation, all of which lack precision and permit the intrusion of ambiguity, vagueness, and obscurity. Science subjects all of its ideas to experimental test and corroboration, promptly rejecting all inadequate hypotheses; philosophy proceeds by pure reasoning, often discounting the evidences of the senses when they conflict with the results of pure logic—a fact which eventuates in the merging of philosophy with poetry and mythology, and in the retention in philosophy of all sorts of imaginative and fanciful ideas whose only justification is their emotional agreeability. Science, in its history, has exhibited progress through coöperative effort; philosophy, on the other hand, has engaged in continuous war between opposing schools of ideas. Hence philosophy and science differ from one another in the *clarity*, *certainty*, and *progressiveness* of the resulting knowledge; and in terms of this distinction the historical encroachment of science upon philosophy is to be understood.

There is probably much in the history of these two disciplines to justify such a theory. Certainly most philosophical concepts are incapable of the same precision in logical formulation as are mathematical notions; probably too much of philosophy has in the past proceeded by the "speculative" method in which the philosopher retired into his study, closed the doors, and endeavored to spin out a theory of the universe; without doubt there has been less advancement in philosophical ideas than in scientific knowledge. But the recent point of view, while admitting the fact, has adopted another explanation. It insists that the differences between science and philosophy are fundamental but not irreconcilable—that the disciplines, instead of being opposed and antagonistic to one another, are really supplementary. Nature is one, but there may be different kinds of objects in nature, and there may be different ways of studying natural objects. Science and philosophy are distinct, either because there are scientific and philosophical objects and the study of the one cannot be the study of the other, or because there is only one general kind of object in nature but alternative ways of viewing it. The result of this approach to the problem in recent years has been the formulation of a number of points of view from which the apparent opposition between the two fields may be understood. For present purposes five of the contemporary theories may be discussed: (1) Science *describes*, philosophy *explains;* (2) science *describes facts empirically*, philosophy *analyzes symbols logically;* (3) science describes *facts*, philosophy describes *values;* (4) science describes *quantities*, philosophy describes *qualities;* (5) science *analyzes*, philosophy *intuits*.

DESCRIPTION *VS.* EXPLANATION

The view that science should limit its task to that of mere description, leaving the more ultimate problems of explanation and interpretation to philosophy, has been characteristic of the so-called positivistic school, of which Mach, Kirchhoff, and Karl Pearson are representatives. Even as far back as Comte one finds an acknowledged positivism. "Our business is—seeing how vain is any research into what are called *Causes*, whether first or final—to pursue an accurate discovery of . . . Laws, with a view to reducing them to the smallest possible number. By speculating upon causes, we could solve no difficulty about origin and purpose. Our real business is to analyze accurately the circumstances of phenomena, and to connect them by the natural relations of succession and resemblance." [1] In the words of Pearson, "the law of gravitation is a brief description of *how* every particle of matter in the universe is altering its motion with reference to every other particle. It does not tell us *why* particles thus move; it does not tell us *why* the earth describes a certain curve around the sun. It simply resumes, in a few brief words, the relationships observed between a vast range of phenomena. It economises thought by stating in conceptual shorthand that routine of our perceptions which forms for us the universe of gravitating matter." [2] The scientist is not forbidden, however, to talk about the *supersensuous;* he may, for example, talk about the atom. But "either the atom is real, that is, capable of being a direct sense-impression, or else it is ideal, that is, a purely mental conception by the aid of which we are enabled to formulate natural laws. It may pass . . . from the ideal stage to the real; but till it does so, it remains merely a conceptual basis for classifying sense-impressions, it is not an actuality. On the other hand, the metaphysician asserts an existence for the supersensuous which is unconditioned by the perceptive or reflective faculties in man. His supersensuous is at once incapable of being a sense-impression, and yet has a real existence apart from the imagination of man. It is needless to say that such an existence involves an unproven and undemonstrable dogma." [3]

On the other hand, philosophy "is concerned not with the accumulation of facts, but with the interpretation of previously ascertained facts, looked at broadly and as a whole. When the facts of physical Nature and of Mind and the special laws of their connection have been discovered and systematised by the most adequate methods of experiment, observation, and mathematical calculation at our disposal, the question still remains, how we are to conceive of the whole realm of such facts consistently with the gen-

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[1] A. Comte, *Positive Philosophy*, tr. by Martineau (New York, 1858), p. 28.

[2] *Grammar of Science* (3d ed.; New York: Macmillan, 1911), p. 99.

[3] *Ibid.*, p. 96.

eral conditions of logical and coherent thought."[1] This interpretation may involve reference to ultimates such as first causes, things-in-themselves, essences, and the like, on the grounds that the human mind cannot be completely satisfied with mere description, and *demands* that we go beyond to more basic interpretations.

It is clear that such an apportioning of the intellectual task to science and philosophy need not result in a view that the two disciplines are hostile to one another. For the extreme positivists, who claim the exclusiveness of the method of description, all philosophy becomes nonsense. But for the more moderate proponents of the position, science and philosophy constitute a coöperative enterprise in which science assumes responsibility for the less ultimate problems and philosophy for the more ultimate. The ground for this attitude is the recognition that the distinction between description and explanation is not an ultimate one. When one describes, he explains in terms of the more obvious features of the given, and when he explains he describes in terms of the less obvious features. With the advance in instrumental techniques explanatory entities become descriptive entities, e.g., molecules, as seen in the Brownian movement. The answer to the question *why* is not fundamentally different from the answer to the question *how;* when one explains he seems to be calling attention to the more basic but somewhat more elusive features of nature—he seems to be getting behind nature to enduring connections, more permanent substances, and the like. But it is the fate of all explanatory entities whose existence becomes well established that they become descriptive entities, which must in turn be explained by still more basic entities. This argues for the view that science and philosophy represent a continuum with reference to one another—a continuum of understanding, at one end of which the approach to nature is through descriptive techniques, and at the other end of which the approach is through explanatory methods. Hence

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[1] A. E. Taylor, *Elements of Metaphysics* (New York: Macmillan, 1904), p. 47.

science and philosophy are not opposed but supplementary.

FACTUAL DESCRIPTION *vs.* SYMBOLIC ANALYSIS

A second theory of the relation between science and philosophy has appeared in recent years among a group of scholars who identify themselves as the Vienna Circle, and are commonly called logical positivists, or logical empiricists. The outstanding representatives of the school are Carnap, Schlick, and Wittgenstein, and they consider themselves the direct descendants of the earlier positivists. For them the dichotomy between science and philosophy has become absolute. To science is assigned the task of both description and explanation; to philosophy is assigned the task of the logical clarification of ideas. "The result of philosophy is not a number of 'philosophical propositions,' but to make propositions clear." [1] It is not the job of the philosopher, therefore, to talk about the world of empirical facts, but rather to talk about the propositions which one asserts when he claims to be talking about such facts. Thus the subject matter of philosophy is really propositions, and the business of the philosopher is the logical analysis of propositions with a view to determining exactly what they mean. If the propositions which he examines prove to be empirical propositions, i.e., propositions of natural science, he must clarify them by reducing them to other propositions which are capable of direct verification. If the sentences prove to be logical propositions, i.e., assertions about symbols and their interrelations, he must clarify them through consideration of the rules of syntax. Often he finds that the application of these two types of technique reveals that a given sentence is nonsense, i.e., a pseudo-proposition. Most of the propositions of metaphysics as formulated in traditional philosophy prove to be pseudo-propositions; the propositions of epistemology and of ethics reduce, respectively,

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[1] L. Wittgenstein, *Tractatus Logico-Philosophicus* (New York: Harcourt, Brace, 1922), prop. 4.112.

in so far as they have content, to propositions of psychology and of sociology, and, in so far as they have no content, to pseudo-propositions. The total range of sensible propositions, then, reduces to two classes: material propositions, which assert about facts and therefore have empirical sense; and formal propositions, which assert about symbols and therefore have sense only in so far as symbols are themselves facts of a certain kind. For Wittgenstein even the propositions of logic are without sense; for Carnap they possess sense of their own peculiar kind.

What may be said in estimation of this important theory of the interrelation of science and philosophy? Its neatness gives it a strong appeal. Furthermore, any technique which offers a method for simplifying the philosophical task by eliminating a large number of pseudo-problems is certain to be looked upon with favor. Again, even apart from the general presuppositions of the position, the analysis of logical techniques and the general contributions to logical theory which have been made by this group have given it a place of importance in the contemporary scene. But that philosophy is adequately defined *merely* as the logical analysis of symbols is, perhaps, questionable. Certainly there are many highly general propositions about the world which are not strictly a part of science and yet which are not merely about symbols. Such propositions tend to be considered as symbolic since their high generality suggests the impossibility that they should be false, and they are consequently taken to be legislative over the world merely by virtue of an arbitrary use of symbols. In spite of the fact that a proposition such as "an event is not both *p* and *not-p*" obviously asserts something about events, it is presumed by this group to be merely an assertion about symbols, i.e., "the symbols '*p*' and '*not-p*' are so defined as to make false any proposition in which they are jointly predicated of an event." A definition of philosophy which would retain its obvious empirical reference seems to be preferable to one which reduces it to symbolic analysis. Yet, in spite of this feature, logical positivism has the same important advantage that the preceding theory has—it does not create an opposition between science and philosophy. In fact many of the representatives of the group are themselves scientists, and have been led to the problems of logical analysis by some of the difficulties which were encountered in the scientific pursuit. They recognize therefore the necessity for coöperation between the investigators in the two fields. Though the distinction between the two realms is sharp, i.e., a scientific proposition is not a philosophical proposition, nevertheless the scientist should consider his propositions philosophically (logically) and the philosopher is bound to apply his techniques to empirical propositions.

FACTS *VS.* VALUES

The third theory of the relation between science and philosophy is based on the dichotomy of facts and values. One finds in the intellectual enterprise that he may make judgments of two kinds: he may assert that something *is*, and he may assert that something *is valuable.* When he makes judgments of the former kind he is asserting existential propositions, and when he makes judgments of the latter kind he is asserting normative propositions. Factual judgments are existential not merely in the sense that they assert that something *is* or *is not;* they also characterize or describe an object or relate it to another object, so long as the characterization, description, or relation does not involve preferential notions or activities. Valuational judgments, on the other hand, assert about the goodness, or beauty, or truth, or usefulness of objects, and thus involve definite reference to standards or norms. To say that snow is white is to utter a factual judgment, but to say that it is beautiful is to make a normative judgment.

Such a distinction between kinds of judgments affords a convenient basis for a differentiation of science and philosophy. The realms of fact and value "are completely separate worlds, and the moral sciences must, in their own highest interest, oppose the natural scientist when he operates with ideas as the 'art forms of nature' and the like. That is not his business, he may pronounce judgments of existence, but not of value. Conversely, the scientists themselves are accustomed to agree to this, since long historical experience has taught them, that the introduction of judgments of value into science has more than once dangerously obscured the view of research as regards objective fact." [1]

Though Bavink believes that the scientist cannot avoid considering questions of value, Patrick insists that problems of this kind are really philosophical. "It is not the purpose of science to study meanings, values and appreciations. . . . But since our primary interests relate to meanings and values, science must be supplemented by philosophy. My new motor car, for instance, is a thing of beauty and it gives me joy just to contemplate its curves and its gloss and its correct proportions. It will have great *value* for me, as I imagine, enabling me to keep distant appointments, to economize time, to live more in the open air, to keep my family entertained, to maintain or increase my social prestige. . . . Hence it becomes necessary to go beyond science to philosophy. Life must be interpreted, not merely described. It must be seen as a whole, not broken into separate parts. Its meaning and value must be sought, its purpose inquired into. Perhaps it has no purpose, meaning or value; *but such a conclusion could be reached only after reflective inquiry, and such reflective inquiry would be philosophy.*" [2]

Recent science has done much to obliterate the distinction between fact and value, and thus to destroy it as a basis for the differentiation of science and philosophy. Value attitudes are being drawn into the realm of scientific subject matter and being described and measured by laboratory techniques. Preferential states are, after all, psychological facts, and to the extent to which one can classify and order such facts he can talk about attitudes in scientific terms.

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[1] B. Bavink, *The Natural Sciences* (New York: Century, 1932), p. 594.

[2] G. T. W. Patrick, *Introduction to Philosophy* (New York: Houghton Mifflin, 1924), 1st ed., pp. 16–17.

Every value judgment is, in the last analysis, also a judgment of fact, viz., the fact that someone has taken a value attitude toward an object. Furthermore, from the other side, every judgment of fact is probably, in some direct or indirect way, the expression of a value attitude. Facts are attended to through acts of selection which may express unconscious interests and unrecognized desires. Even the scientist must guard against the tendency to blind himself to the existence of data which would constitute a refutation of a pet theory. The very fact that the scientist prefers true judgments to false is the result of a preferential state. Hence the distinction between judgments of fact and judgments of value, which has a certain practical working effectiveness, becomes relatively unsatisfactory as a foundation for the distinction between science and philosophy.

QUANTITY *vs.* QUALITY

A fourth view of the interrelation of the two fields is that which apportions all discussions of the quantitative aspects of experience to science and all considerations of the qualitative aspects to philosophy. The following quotation from Eddington is a classic as a description of the task of the scientist. "Let us examine the kind of knowledge which is handled by exact science. If we search the examination papers in physics and natural philosophy for the more intelligible questions we may come across one beginning something like this: 'An elephant slides down a grassy hillside . . .' The experienced candidate knows that he need not pay much attention to this; it is only put in to give an impression of realism. He reads on: 'The mass of the elephant is two tons.' Now we are getting down to business; the elephant fades out of the picture and a mass of two tons takes its place. What exactly is this two tons, the real subject matter of the problem? It refers to some property or condition which we vaguely describe as 'ponderosity' occurring in a particular region of the external world. But we shall not get much further that way; the nature of the external world is inscrutable, and we shall only plunge into a quagmire of indescribables. Never mind what two tons *refers* to; what *is* it? How has it actually entered in so definite a way into our experience? Two tons *is* the reading of the pointer when the elephant was placed on a weighing-machine. Let us pass on. 'The slope of the hill is 60°.' Now the hillside fades out of the problem and an angle of 60° takes its place. What is 60°? There is no need to struggle with mystical conceptions of direction; 60° *is* the reading of a plumb-line against the divisions of a protractor. Similarly for the other data of the problem. The softly yielding turf on which the elephant slid is replaced by a coefficient of friction, which though perhaps not directly a pointer reading is of kindred nature. . . . And so we see that the poetry fades out of the problem, and by the time the serious application of exact science begins we are left with only pointer readings. . . . The whole subject-matter of exact science consists of pointer readings and similar indications." [1]

What, then, is left for the philosopher? The philosopher must tell us about elephants and hillsides and ponderosity, or, more generally, about colors, sounds, odors, tastes, and touch sensations, and the complexes called "objects" into which they are united. But he must describe these in qualitative terms, not in measured values. Though red may be measured by a wave length, red *is not* the wave length; there is something in the qualitative given which eludes analysis and quantification. Science abstracts from objects, and by this act loses an important feature of them. The artist, the religious man, the mystic all try to capture this elusive element, each in his own way; but all of these ways are essentially emotional and irrational. The philosopher must pursue this qualitative element by *rational* techniques. The philosopher must restore to the world its expansiveness and durational character, which the scientist has replaced by meter sticks and clocks; he must repopulate it with

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[1] *From* A. S. Eddington, *Nature of the Physical World*, pp. 251–252. By permission of The Macmillan Company, publishers.

heat and movement and pushes and pulls, which the scientist has eliminated in favor of molecules and differential velocities and pointer readings. Only through this supplementation can one understand the world in its totality.

It seems likely that a thorough-going analysis of the notions of quality and quantity would do much to obliterate the sharpness of this distinction. There certainly is a sense in which quantities are themselves qualities. In fact a quality becomes a quantity just to the extent to which it may be shown to have a certain place in a series of a specifiable kind. Qualities which are orderable are also quantities. When one thinks, for example, of the natural numbers he tends to picture them as arranged in the order of magnitude, 1, 2, 3, 4, . . . , rather than as a mere aggregate, 2, 4, 3, 1, ... Hence he tends to think of the differences between them as being quantitative rather than qualitative. Actually one can discover the same qualitative difference between 2 and 3 that he can between red and green, though the ready ordering of the numbers compels him to neglect this difference in favor of a quantitative one. Though the insertion of intervening members in the series, for example, the fractions, tends to turn attention away from the qualitative difference and toward the quantitative difference, the former distinction remains and is found at the new level between any two fractions. To order is not to eliminate quality. Serial arrangement serves only to replace qualities which are not readily orderable by qualities which are.

The distinction between science and philosophy can then be expressed, though it takes a slightly different form. Science is interested in the consideration of those qualitative aspects of experience which are readily orderable, and especially in the supposed correlation between these qualities and certain others of which they seem to be the measured values. Physics, for example, is occupied with wave lengths and with the functional relations between wave lengths and colors. Philosophy, on the other hand, busies itself with those qualities which are not so readily orderable, for example, the colors themselves, and especially those other qualitative manifestations of experience, such as life, intelligence, pleasure, and pain where all ordering seems more or less artificial. This, at any rate, proves to be a more satisfactory working distinction between the two fields.

ANALYSIS *vs.* INTUITION

Closely related to this distinction, but not to be confused with it, is the point of view maintained particularly by Bergson in recent years. Science proceeds by the method of analysis while philosophy uses the method of intuition. According to Bergson, "it is easy to see that the ordinary function of positive science is analysis. Positive science works, then, above all, with symbols. Even the most concrete of the natural sciences, those concerned with life, confine themselves to the visible form of living beings, their organs and anatomical elements. They make comparisons between these forms, they reduce the more complex to the more simple; in short, they study the workings of life in what is, so to speak, only its visual symbol. If there exists any means of possessing a reality absolutely instead of knowing it relatively, of placing oneself within it instead of looking at it from outside points of view, of having the intuition instead of making the analysis: in short, of seizing it without any expression, translation, or symbolic representation—metaphysics is that means. *Metaphysics, then, is the science which claims to dispense with symbols.*" [1] The opposition between science and metaphysics is further clarified by showing that the one is conceptual, fixed, descriptive, mediate knowledge, while the other is intuitive, mobile, acquaintive, immediate knowledge. Science sees objects from the outside, metaphysics from the inside. Thus scientific knowledge is artificial, illusory, and dangerous, while metaphysical knowledge is real, true, and reliable. Through science one grasps the world by means of an empty, static

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[1] *Introduction to Metaphysics* (New York: Putnam, 1912), pp. 8–9.

form; through metaphysics one knows it without representation, as a full and changing reality.

There is probably much in Bergson's theory of the ultimate nature of reality that led him to this view of knowledge. If the world is a changing, growing reality, and the expression of a vital urge to develop, there is reason to believe that a conceptual scheme—which is by its very nature static—cannot portray its essential nature. Hence a method of knowing, peculiar to the subject matter, must be devised. Furthermore there is probably a sound epistemological foundation for the distinction between acquaintance and description, between synthetic and analytic methods of knowing, between feeling a duration and conceptualizing time. But it seems unlikely that any such feature can be employed as a basis for distinguishing between philosophy and science. It is much more probable that the differentiation of philosophy from science must cut across this methodological distinction, since either one of the methods, if not supplemented by the other, can hardly result in anything that can be called knowledge. Knowledge in its simplest form is *both* acquaintance and description, synthesis and analysis, feeling and conceptualization. Hence neither the philosopher nor the scientist could be said to know in the strict sense of the word unless he employed both methods. It is rather significant that Bergson's thesis, in spite of its neatness and emotional appeal, has been supported by comparatively few scientists and philosophers in the contemporary scene.

Certain other minor attempts to differentiate the two fields of knowledge may be mentioned in conclusion. It is sometimes maintained that the distinguishing feature of science is its use of laboratory and instrumental techniques, as contrasted with philosophy which proceeds by observation, introspection, and other methods of passive reception. Closely allied with this view is the theory that science is empirical and inductive while philosophy is rational and deductive. Or, again, it may be insisted that since science arose out of the need for control over the world it should be defined as practical knowledge, as opposed to the wisdom and understanding which is the unique possession of the philosopher. All of these distinctions seem to break down when one attempts an actual classification of the existing intellectual disciplines on the basis of one or more of them. There are acknowledged sciences which do not employ laboratory techniques; there are inductive philosophies and deductive sciences; there is theoretical physics and practical philosophy. Thus the adoption of any one of the distinctions would be in essential violation of the terminology not only of common sense but of critical thought as well, and it would seem inadvisable to follow this procedure. The alternative is to formulate the problem not in terms of philosophy *vs.* science, but in terms of a philosophy *of* science. The consideration of this problem will be found in the following chapter.

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CHAPTER II

PHILOSOPHY OF SCIENCE

In the last chapter consideration was given to some of the attempts which have been made in recent years to "save" philosophy from the encroachment of science. In each case, the resolution of the conflict was presumed to lie in some apportioning of the intellectual task to the separate disciplines either on the grounds of method or on the grounds of subject matter. It was maintained, for example, that science talks only about certain types of natural entity and leaves all others to philosophy. Or science talks about natural entities through the medium of a certain type of method and leaves alternative approaches to philosophy. In all cases the presumed reconciliation lies in a precise delimitation of the respective fields.

EMERGENCE OF THE PHILOSOPHY OF SCIENCE

The emergence in recent years of a new discipline called "the philosophy of science" was occasioned, I believe, by three factors. One was the recognition of the obviously temporary character of this presumed reconciliation between science and philosophy. A second was the appearance, within science itself, of certain inconsistencies, which compelled it to become self-critical. A third was the claim on the part of science that it had the right to legislate over all features of the individual life. Each of these may be examined briefly.

The unfortunate character of the type of reconciliation suggested in the preceding chapter is that it can be, at best, only temporary. Inevitably questions arise either as to the *importance* of the respective disciplines, or as to the *legitimacy* of the type of problem which the one or the other discipline is called upon to answer. The view that science is description leads imperceptibly to the view that all knowledge is description, and that problems of explanation and interpretation are pseudo-problems. The claim that science employs quantitative methods to understand qualities passes gradually into the position which insists that when qualities have been explained they have been explained away, hence there is nothing left for philosophy. Or, on the other hand, if philosophy knows things immediately and intuitively, then science, which knows things only mediately and symbolically, must be pseudo and illusory.

It was readily seen that no permanent harmony could be established by means of such a reconciliation. But it was also apparent that the very attempt to decide questions of the importance of the scientific method as over against the philosophical method, questions of the legitimacy of the two types of problem, questions of the truth of the knowledge obtained through the respective approaches, gave rise to interesting speculations and, in many cases, to significant answers. The suspicion immediately arose that the investigation of the methods of science and philosophy, of the limits of the respective fields, and of the relative importance of the two types of knowledge might itself become a discipline with significant results to offer toward the solution of the ultimate problem of knowledge. Since the method and subject matter of science seemed more definite than the method and subject matter of philosophy, this new discipline was called "the philosophy of science." The task of this new enterprise was the critical study of science from the point of view of its presuppositions and unanalyzed notions, from the point of view of its techniques and methods, and from the point of view of the limits of its problems. By this means a more or less permanent reconciliation was brought about, for if philosophy is the study of science just as science is the study of nature, there can no more be a conflict between science and philosophy than there can between science and nature. There can be no question of the relative importance of the two disciplines for they are, so to speak, upon different levels, and the question of importance depends upon the level in which one is interested. Thus out of the attempt to integrate the two fields there arose a study whose problem was precisely the integration of the two fields, and this gradually became the discipline which is now called "the philosophy of science."

The second factor responsible for the emergence of the philosophy of science was the appearance in science itself of certain features which were responsible for its own instability. The philosophy of science is inevitable whenever a science becomes self-critical. But self-criticism arises only when there is an obvious breakdown. So long as a machine operates smoothly there is no occasion for the operator to suspect that it is not in order. The smooth operation seems a sufficient guarantee of efficiency and adequacy. But now suppose the machine suddenly to stop, either because of an internal breakdown or because of an external obstacle. Immediately the question arises as to the cause of the interruption, and more specifically, as to the general character of a machine which will suddenly stop in the face of a promise of continued functioning. A breakdown can never occur unless it is potentially in the engine even when it appears to be running smoothly. And an engine which has the potentiality of breaking down is clearly an inferior engine, as compared with one which has not this potentiality. Thus the problem of preventing malfunctioning is as much a problem in connection with machines as is the problem of guaranteeing continued functioning. Now if the subject matter of one's study is not an engine but science itself, the problem is essentially the same. Science belongs to science so long as it exhibits no inadequacies. But when science proves inadequate it belongs to philosophy. This does not mean that philosophy functions as the great intellectual repair man. Mere repairing is essentially the same problem as original construction, namely, a scientific problem. Philosophy repairs only in the sense that it attempts to show primary inadequacies, unwarranted original assumptions, questionable predictions, doubtful methods. Thus when the scientist looks at his product with a view to the presuppositions which were employed in its construction, with a view to the inherent weaknesses in its internal structure, with a view to the possible inadequacies which may later come to light—when a scientist does this he ceases to be a scientist and becomes a philosopher.

Two examples of such crises in science, both drawn from the last century, may be offered for consideration.

The first is taken from mathematics, and, more specifically, from the field of geometry. Probably no one of the sciences has had a more complacent history, a more continuous development, and fewer regressive movements. Euclidean geometry was a monument to scientific ingenuity, exhibiting internally a high degree of consistency and affording externally a highly accurate and extremely convenient method for measuring land, constructing buildings, and meeting innumerable problems of practical life. Then came two individuals, about the middle of the last century, Riemann and Lobachewski, who showed in a conclusive manner that there could be *alternative* systems of geometry, i.e., that the Euclidean system of geometry was not *the* geometry but merely one among a large number of such systems. Each such system was shown to be internally perfectly consistent; from a group of axioms and postulates accepted as true the total body of propositions could be deduced by strictly logical processes. But between systems there was an essential contradiction. For example, in the Euclidean system, the sum of the angles of a triangle is equal to 180° but in the Lobachewskian system the sum of the angles of a triangle is always less than 180°; and in the Riemannian system the sum is always greater than 180°. Immediately the question arises, which system is true? Clearly they cannot all be true for they are incompatible; yet one of them must be true.

To answer this question would take one into the very heart of recent physical science. What should be pointed out here is the change in character which geometry experienced as a result of this shock. It became self-critical. Geometricians began to ask such questions as the following, which, it should be noted, are not geometrical questions but philosophical, i.e., logical and epistemological. What is meant by the consistency of a system of propositions? Can a system of propositions be consistent and not true? What is truth? Does geometry describe the world? Is space objective, i.e., independent of the knowing activities, and, if so, *which space*—Euclidean space, Lobachewskian space, or Riemannian space? Thus the geometrician was compelled by the intrinsic inconsistencies of his system to turn his attention to philosophical considerations. When he did so he ceased to be a scientist and became a philosopher, and he created that study which is called "the philosophy of science," or, in this case, "the philosophy of geometry."

The second example is drawn from the field of physics. At the end of the last century physics was also in a state of great complacency. Though its youth and development had not been so uneventful as that of mathematics, it had reached a stage of relative quiescence. It was generally recognized that the great discoveries in physics had all been made and that progress would consist of the introduction of greater accuracy. The point of view is well expressed by the statement in the announcement of courses in the department of physics in one of the great universities of this country near the end of the last century, to the effect that research work in physics in the future would consist essentially in the transformation of physical laws so as to make them accurate to the fourth rather than merely to the third decimal place.

Then came the shock of the relativity theory. A certain experiment, devised to measure the variations in the velocity of light due to the changes in the velocity of its source, was performed. It was confidently predicted that the anticipated outcome would be verified. But the results were unequivocally negative. The crucial character of the experiment made its negative outcome of great significance. Ordinarily when an hypothesis fails of verification the scientist throws the theory aside and sets up a new one in its place. But here the situation was different. For the theory which was negated by the experiment was at the very foundation of the whole physical structure; if it were to be thrown aside, most of physical theory would have to be discarded. Physics was thrown into turmoil. The problem of the future of physics now seemed to lie in abandoning the entire structure and starting again from the ground.

Again it is not important to solve the problem here raised. What should be pointed out is that physics immediately became self-critical. And in becoming self-critical it became philosophical. For the problems which are raised by the theory of relativity are unquestionably philosophical in character. They have to do with the nature of time, space, and motion; with the assumptions implicit in the method of measurement; with the applicability of geometrical concepts to the world; with the very meaning of "truth," "theory," "law," and other logical concepts. For example, if the results of this experiment are to be accepted, there seems to be a peculiar interrelationship of space and time. This is popularly, but very inaccurately, expressed by saying that time turns out to be a fourth dimension of space. What is meant is that space and time are shown to constitute a four-dimensional system, of which three dimensions are spatial and one dimension is temporal. In other words, in order to locate a happening completely one must specify not only where it occurs but also when it occurs. But, surprisingly enough, one cannot tell when it occurs without knowing at the same time where it occurs, nor can one tell where it occurs without knowing when it occurs—hence the puzzling interrelation of space and time which has been one of the essential problems of the philosophy of science. Here again what should be pointed out is merely that the physicist, when he turns his attention to such problems as that mentioned above, ceases to be a pure physicist and becomes a philosopher of physics. This does not mean that he must cease to be a physicist, but it does mean that he approaches his subject matter with a different attitude, a different method, and a different aim.

In the third place the emergence of the philosophy of science occurred as a result of a fact which was essentially social in character. The last century was, by all odds, a period of outstanding scientific progress; the present century promises even to surpass it in the extent of knowledge, in the application of this advance to the improvement of comfort and convenience, and in the effect which this increased information is certain to exert upon individual and social values. The fact is that we are better informed than we were a hundred years ago, and the difference is essentially due to science. Science has been so much popularized in recent years that the average layman knows more today than the specialist of a century ago. Furthermore, the advance in knowledge has not been of a purely theoretical sort; instead it has resulted in revolutionary transformations in the manner of life of the ordinary individual. Man is today more comfortable than he has ever been, and he has more opportunities for cultural advancement, education, and travel than his ancestors ever dreamed of. Moreover, science is claiming the right to legislate over his entire life; the cry is to make religion scientific, to make behavior scientific, to make politics scientific. Our age has been properly characterized as the scientific age. Certainly it is scientific in the sense that its ideals are essentially the ideals of science, the spirit with which it approaches its problems is preëminently the spirit of science, and the solution to its problems seems to lie in further scientific knowledge.

All of this has turned man's attention to science. Science is no longer being taken for granted; it is no longer a negligible aspect of the social scheme, free to pursue its independent course. It has emerged as a dynamic factor, exerting influences upon all phases of the contemporary scene—religion, morality, art, education, and the cultural life in general. Inevitably man, fundamentally a rational animal, begins to ask the question: what is science? What is this force which has taken the leading rôle in the modern scene? Is it entitled to the claim which it asserts—the claim that every phase of life may justifiably be subjected to scientific analysis, the claim that only the scientifically verifiable is real, the claim that man's salvation is to be found in subservience to the laws of nature as revealed through the techniques of science? The systematic attempt to answer this question became the philosophy of science.

THE PHILOSOPHY OF SCIENCE

Through these three approaches there has gradually taken form a new discipline which has been called, variously, "the philosophy of science," "the science of science," "philosophical science," "the logic of science," "the metaphysics of science," and "metaphysical science." It is probably impossible today to obtain any general agreement among authorities as to what exactly constitutes a problem of the philosophy of science. All that one can say is that a new type of question is being asked, and that there is gradually growing up a body of literature which seems to be devoted to the attempt to answer this kind of question. Some of the literature tries definitely to answer the question; some of it offers certain vague hints; some of it can hardly be said to have contributed anything specific toward the solution. Four new periodicals[1] have appeared in the last decade, all of them devoted directly or indirectly to the attempt to answer this question. The First International Congress of the Philosophy of Science was held in Paris, September, 1935, and the plan calls for a continuance of the movement in the form of annual meetings. Courses in the philosophy of science are given in practically all of the leading colleges and universities of the world. Contributors to the subject include such eminent figures as Einstein, Eddington, Jeans, Heisenberg, Poincaré, Whitehead, Russell, Planck—to mention only a few of those best known.

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[1] *Philosophy of Science*, Williams and Wilkins, Baltimore; *Analysis*, Basil Blackwell, Oxford; *Erkenntnis*, Felix Meiner, Leipzig; *Theoria*, Wettergren and Kerber, Gothenberg.

At present one cannot set the limits of the field of the philosophy of science; one can only list the problems which are being discussed in contemporary literature—problems which are neither scientific in the strict sense of the word nor philosophical according to the traditional use of the term. The following list occurred in the first number of the *Philosophy of Science.* It is purely tentative and presented merely on the editor's responsibility.

I. Studies in the analysis of meaning, definition, symbolism.
II. Studies in presuppositions—axioms, postulates, maxims.
III. Studies in method.
IV. Studies of the nature and formulation of theoretical principles.
V. Studies in the structure of the sciences, their hierarchies.
VI. Studies in the function and significance of science within various contexts.[1]

The program of the First International Congress of the Philosophy of Science was divided into sessions designated by the following topics: unity of science, logic and mathematics, pseudo-problems, definition and experience, protocol propositions, language and logic, induction, probability, semantics, psychology and sociology, history of logic, logic and theory of groups.

If one cannot set the limits of the problems which belong properly to the philosophy of science, so much the less can one classify within themselves the problems which by a more or less arbitrary act are assigned to the field. However, a tentative classification may be made upon the grounds of certain characteristics which are exhibited by the sciences. The most obvious of these features are the following:

1. The most important characteristic of science is that one finds not *science* but *sciences.* Every scientific investigator is a physicist *or* a mathematician *or* a biologist, i.e., a specialist. In general he pursues his studies in his own particular field without any concern for what is being discovered in other fields except in so far as these have direct

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[1] Vol. I, No. 1, pp. 3–4.

bearing upon his own subject matter. The result is that the individual scientist feels quite justified in neglecting considerations of the following kind: the conclusions in any field other than his own; the nature of the relationships holding between his field and other fields, and between the other fields themselves; the character of the total world which has thus been departmentalized for purposes of study.

2. Another important feature is that the scientist, as such, is not a logician, i.e., he is not clearly aware of the method which he is employing and of the criteria in terms of which its validity should be judged. The scientist proceeds largely by hunches and guesses, by trial and error, by intuition, by unconscious inferences and techniques based primarily on habit. The point is not that these are bad methods, but that they are accepted uncritically. It is not unusual, for example, to find teachers of science advising students against taking a course in logic on the grounds that they become so much concerned with what is going on in their minds that they are no longer able to observe what is taking place before their eyes. Fortunately this extreme attitude is not taken by the greater number of scientists. But there is unanimity in the view that the techniques of science should not be examined until there is an obvious breakdown, indicating a fundamental weakness in the structure. On such an occasion either the scientist or the philosopher may undertake the reconstruction, but if the scientist does it he is not acting as a scientist but as a philosopher.

3. Still another feature is the scientist's acceptance of many concepts without a careful analysis of their meaning, and many beliefs without a careful justification of their truth. These are often called the presuppositions or the foundations of his system. It is not the task of the biologist, for example, to examine the concept of matter, nor of the psychologist to examine the concept of life, nor of the physicist to examine the concepts of number and order, nor of any of the scientists to examine the concepts of relation, quality, event. Nor is it the task of the individual

scientist to justify his belief in the uniformity of nature, or in the independence of the world which he is studying, or in the basic rationality of nature. These propositions are accepted uncritically by the scientist, and presumed to be true until evidence to the contrary is revealed.

These are facts about science, and can, I think, hardly be called into question. But it is quite another matter to ask whether the neglect of these problems by the scientist produces any unfortunate consequences. Are these problems, after all, important? Granting that the scientist *qua* scientist is justified in neglecting them, does it follow that they may be disregarded by all investigators, or are they of sufficient importance to warrant turning them over to the philosopher or some other specialist for study?

It is the presumption of the philosophy of science that problems of this type are important and should be, if not solved, at least clarified. Their importance resides in the fact that their neglect leads to difficulties. More particularly, the extreme specialization of any scientist almost inevitably results in a warped outlook not only on the world as a whole but even on his own field. The belief soon arises that the aspect of nature which he is studying is the *only* aspect, and that all other aspects are either reducible to it or such as can be studied by methods applicable to it. This results in all sorts of erroneous conceptions, on the one hand, of contradictions and antagonisms between the special fields, and, on the other, of unjustifiable continuities holding between them. For example, it may be held that science and religion are in irreconcilable opposition, and doomed to wage war on one another until one or the other is exterminated; or it may be maintained that religion is continuous with science and therefore reducible to it in the sense that religion is merely the feeling of awe and wonder in the presence of the marvelous organization of nature—a feeling which the scientist experiences in its most acute form, and which compels him, willy-nilly, to be religious. Specialization, furthermore, often blinds the investigator to the possibilities of coöperation in the scientific enterprise; often newly discovered techniques in one field have proved applicable to other fields, and frequently conclusions in one department suggest analogies in other departments.

Moreover, whatever may be said as to the dangers run by science when it becomes over-attentive to method, it can hardly be said that science will profit by a complete neglect of logical considerations. Though the scientist cannot be expected to tell us what truth in the abstract is, he can certainly improve his results by clarifying his notion of the techniques to be employed in testing the truth of an hypothesis, and the dangers to which such techniques are exposed. It is not unlikely that knowledge of the most common errors of observation will make the observer less susceptible to them; it is reasonable to suppose that an understanding of the principles of deduction will enable the scientist more readily to discover faulty applications of them in his own mental processes. Even the carpenter must occasionally pay attention to his tools.

Finally, clarification of the implicit concepts and assumptions of science can only result in a corresponding clarification of its explicit beliefs. Every intellectual enterprise rests upon certain undefined notions and certain undemonstrated propositions. These basic ideas, however, are the foundations of the science in a *logical* sense rather than its origin in a *temporal* sense. One makes no conscious assumptions to begin with but discovers in the course of his investigation that his conclusions can be justified only if he makes explicit certain beliefs which seem to have been implicitly present from the beginning. But the fact that the science rests upon these notions does not mean that they are clearly understood; on the contrary they can be clarified only by a slow and painful process which shows their derivation from data which are more clearly given. "The truth is that what is logically most primitive in nature is not what is now most familiar to us, and therefore it is better for didactic purposes to start with the logically derivative but practically familiar, and work back to the logically primitive but practically unfamiliar."[1] The clarification of the basic concepts and assumptions is the revelation of that part of science which is logically primitive.

PROBLEMS OF THE PHILOSOPHY OF SCIENCE

On the basis of these three facts about science one may formulate a tentative classification of the problems of the philosophy of science. The philosophy of science has as its task the consideration of three main types of problem: (1) The ascertainment of the limits of the special sciences, of their integrations one with another, and of their implications so far as these contribute to a theory either of the universe as a whole or of some pervasive aspect of it; (2) the critical examination of the method of science, of the nature of scientific symbols, and of the logical structure of symbolic systems; (3) the clarification of the basic concepts and postulates of the sciences, and the revelation of the empirical grounds (or absence of grounds) upon which they are presumed to rest.

The first aspect of the philosophy of science may be called *speculative*, *synthetic*, or *synoptic philosophy*. It differs from traditional cosmology only in its insistence upon the fact that philosophy in this sense is dependent upon the special sciences, and therefore not free to pursue the uncontrolled speculation which has been its characteristic method in the past. According to Broad, "its object is to take over the results of the various sciences, to add to them the results of the religious and ethical experiences of mankind, and then to reflect upon the whole."[2] He then goes on to point out that it necessarily presupposes the more critical examination of the sciences; that it can, at best, consist only of more or less happy guesses; and that it is almost certain to be influenced by one's hopes and fears, likes and dislikes. All of these condemnations are probably justified; but they

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[1] C. D. Broad, *Scientific Thought* (New York: Harcourt, Brace, 1923), p. 26.

[2] *Ibid.*, p. 20.

mean only that the study should be pursued with unusual caution, and that its results should not claim to be more than conjectures substantiated to a greater or lesser degree.

The second aspect of the philosophy of science may be called the *logic of science*, or the *philosophy of method.* It differs from traditional logic in its insistence that science offers, by and large, the most effective method for acquiring knowledge. Hence the task of philosophy toward science is the analysis and criticism of its method, with a view to estimating its scope and determining its limitations, with a view to exposing its errors and revealing its criteria of validity, with a view to the clarification of the assumptions about symbols, meaning, and language which are involved. This is the fundamental part of what Broad calls *critical* philosophy. "It deals with such concepts as *truth*, *implication*, *probability*, *class*, etc. In fact it may be defined as the science which deals with propositional forms, their parts, their qualities, and their relations." [1]

The third aspect of the philosophy of science may be called the *metaphysics of science*, or *metaphysical science.* It is closely connected with the logic of science, and is included by Broad under critical philosophy. "Common-sense constantly makes use of a number of concepts, in terms of which it interprets its experience. It talks of *things* of various kinds; it says that they have *places* and *dates*, that they change, and that changes in one *cause* changes in others, and so on. . . . Science takes over these concepts from common-sense with but slight modification, and uses them in its work. . . . The most fundamental task of Philosophy is to take the concepts that we daily use in common life and science, to analyze them, and thus to determine their precise meanings and their mutual relations." Though the scientist may and does pursue this analysis, whenever he "begins to discuss the concepts of his science in this thorough and disinterested way we begin to say that he is studying, not so much Chemistry or Physics, as the

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[1] *Ibid.*, p. 23.

Philosophy of Chemistry or Physics."[1] Unfortunately problems of this sphere merge imperceptibly into problems of the logic of science, for often one can tell what the basic concepts mean only by knowing how they are *used* as concepts of explanation. Frequently the basic concepts are definable through operations of generalization, serial extension, interpolation, idealization, etc., upon objects which are empirically given; hence the analysis of their meanings is essentially logical. But the empirically given must itself be subjected to critical analysis, and this is essentially a metaphysical activity.

GENERAL CHARACTER OF THE STUDY

Granting that there is a legitimate study which may be called the philosophy of science whose task is the examination of the types of problems which have been enumerated, the problem immediately arises as to who is to carry on such study. More specifically, is it to be the scientist or the philosopher, or, perhaps, an individual who is neither? It is clearly within the rights of both scientist and philosopher, at least according to the traditional senses of these terms, to deny each in his own way responsibility for these problems. The scientist may properly insist that there are sufficient problems of a strictly scientific sort to occupy his time—problems which are pressing and offer some prospect of solution, as contrasted with considerations of assumptions, methods, and interrelations which require philosophical knowledge and can be postponed indefinitely on the grounds of their unimportance and the unsatisfying character of any solution which can be given to them. On the other hand, the philosopher may justly insist that problems of this sort require specialized scientific knowledge which he does not and cannot possess without devoting his life to scientific pursuits. Through some such arguments as these there have arisen on the one hand scientists and on the other philosophers, who insist that the two disciplines should

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[1] *Ibid.*, pp. 15, 16, 17.

pursue their own independent ends, without hostility, to be sure, but also without coöperation and without recognition of border-line problems.

Fortunately there has been in the last few years an ever increasing group, consisting both of scientists with an interest in philosophy and of philosophers with an interest in science, who feel that the problems of the philosophy of science are both genuine and important. It must be admitted that there is much literature in recent philosophy of science which must be discounted either on the grounds of its bad philosophy or on the grounds of its bad science. It is well recognized that most scientists write bad philosophy, and most philosophers write bad science. "The recent advances in physical theory," writes Broad, "have been so important and spectacular that they have only too obviously 'gone to the heads' of some eminent physicists, and have encouraged them and the public to believe that their pronouncements on technical philosophical problems, for which they have no special training or aptitude, are deserving of serious attention. This is of course a profound mistake."[1] On the other hand many philosophers who consider these border-line problems prove to be writing not about science but about what philosophers think science is. This is equally misleading. Yet it must also be recognized that too great familiarity with one's own field may constitute a disadvantage in seeing it in the large; true perspective is not gained from within. Many scientists think they are writing about the philosophy of science when they are merely considering problems of scientific theory, and many philosophers when they are merely drawing sweeping conclusions on the basis of specific scientific hypotheses. The peculiar combination of insights which entitles one to make authoritative judgments in both fields is extremely rare. Significant advances in the field of the philosophy of science will be made neither by the scientist nor by the philosopher,

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[1] *Examination of McTaggert's Philosophy* (Cambridge: Cambridge University, 1933), p. liii.

but by the *philosophically minded scientist* and the *scientifically minded philosopher.*

Granting the legitimacy of such a discipline as the philosophy of science, an important difficulty arises. What is to be its method? Unfortunately it does not follow that a philosophy of science will always be a scientific philosophy. As Cohen insists, it often requires a specialist to know what the results of the special sciences are, hence any synthesis of these conclusions must be regarded as tentative and subject to continual revision.[1] The philosophy of science is not for the lovers of fixed systems. Furthermore, as Broad points out, the actual synthetic act is almost certain not to be scientific since it passes immediately to broad and sweeping generalizations and attempts to recognize the claims of the emotions. Cohen asserts that it is dominated more by "practical, dramatic, and aesthetic than by scientific motives." [2] The philosophy of science runs these same risks in its other aspects, though not to so great a degree. Hence the problem arises as to how to pursue these precarious studies with a hope of maximum success.

The answer, in lieu of anything better, must be that the same cautious method which has characterized science itself should be the method of the philosophy of science. For any study a method must be presupposed, since a method cannot be used and critically examined at the same time. If science has attained a measure of success by means of its own method, the presumption is that the method may itself be examined in the same way. This does not mean that experimentation, measurement, and other laboratory techniques must be employed in the philosophy of science. It will be shown in the course of the following studies that these are not the distinguishing features of the scientific method. Science proceeds, rather, by formation of hypotheses which are based upon clearly formulated data and verified in terms of consequences. This must be the method of the philosophy of science.

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[1] M. R. Cohen, *Reason and Nature* (New York: Harcourt, Brace, 1931), p. 147.

[2] *Loc. cit.*

It will be the task of the following pages to illustrate the main types of problem which constitute the field of the philosophy of science. For convenience the treatment will be divided into three parts corresponding to the three problems. In connection with each problem the attempt will be made to indicate its essential character, and to discuss some of the most typical of the answers which have been suggested in recent literature. Because of the importance of method, problems of the logic of science will be examined first. This will prepare the ground for the consideration of those logico-metaphysical problems having to do with the analysis of the basic concepts and presuppositions. Speculative problems, resting as they do upon both logical and metaphysical problems, will be examined last.

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PART ONE

PROBLEMS IN THE LOGIC OF SCIENCE

CHAPTER III

THE LOGICAL STRUCTURE OF SCIENCE

With preliminary considerations disposed of, attention may be directed to the first of the three main problems of the philosophy of science, viz., the nature of the logical structure of science. What is this structure? The very formulation of the question suggests that preliminary considerations have not, perhaps, after all, been completely set aside. There is clearly an assumption involved in asking this question—an assumption which is twofold since one may raise doubts both as to the necessity for asking such a question and as to the possibility of answering it. Hence two other questions are relevant: (1) Is it *important* to know what the logical structure of science is? (2) Is it *possible* to know what the logical structure of science is?

IMPORTANCE OF THE LOGIC OF SCIENCE

The attempt to answer this question necessitates the clarification of a significant distinction. Theories may be *logically*, or they may be *temporally*, prior to investigation. Every scientist employs a certain method. Such a method is accepted uncritically and seldom called into question; so long as it produces the desired results it is supposed to be adequate. Now there is presumably for every technique of investigation a theory which would justify it; this theory consists of a set of postulates or hypotheses from which the method could be deduced. The method could then be established or shown to be adequate by actually deducing it from the postulates. Now the important question at this point is whether such a set, which is logically prior to the use of the method, is also temporally prior to it. In other words, must the scientist, before he can employ the scientific technique, construct a theory in terms of which that technique can be justified? Must his assumptions about method be temporally prior to the use of that method? I think the answer to this question is an unequivocal negative. Certainly most scientists do not make such critiques prior to engaging in their more specifically scientific pursuits. If there are certain assumptions which antedate the procedure of science, these are usually held implicitly rather than explicitly and are correspondingly vague. Probably every scientist believes with greater or less conviction that there is a world which is in some sense external to him, and that in his knowing activities he somehow gets into contact with the world. But these are not held as conscious assumptions, and it would probably be difficult for the average scientist to show in just what sense they justify his procedure. On the other hand it seems safe to say that there *is* probably some set of postulates which would justify his procedure, though the scientist may not know what that set is. It may or may not correspond with the vague assumptions which he held prior to his use of the method, but certainly it need not be formulated anterior to the use of the technique. However, it does seem true that in the long run the scientist would be better off if he were aware of this postulate scheme; hence it is important in this sense for him to know what the logical structure of science is. He is almost certain to find sooner or later that his knowledge is the effect of his method in ways which he had not suspected, and he will probably come upon situations in which his method will prove inadequate. In both types of situation he would be better able to overcome the difficulties if he had an adequate theory of the structure of science.

This difference between temporal and logical priority creates a difficulty in the treatment of problems of the logic of science. In any question which refers to the logical foundations of science one is confronted with the alternative of considering these so-called foundations as they are held implicitly by the scientist prior to his investigation, or as they should be formulated from the point of view of a strictly logical justification of science itself. The two questions can be formulated as follows: (a) What does the scientist actually assume, explicitly or implicitly, with reference to an external world, the uniformity of nature, the structure of knowing? (b) What theory of the external world, the uniformity of nature, and the structure of knowing will actually justify logically the procedure of the scientist? The answers to both questions are difficult, though for different reasons. On the one hand it is not easy to ascertain what beliefs the scientist holds implicitly, both because they are vague and because they differ from scientist to scientist. But on the other hand it is not easy to construct a system of postulates which will justify all science, for science is an extremely complicated thing and an adequate postulate scheme would necessarily involve complete solutions to some of the most profound problems of philosophy.

POSSIBILITY OF THE LOGIC OF SCIENCE

Granting the importance of understanding the logical structure of science, is such an understanding itself possible? It seems clear that the only way to answer this question is to plunge into the study of science and see whether results which are at all satisfactory can be attained. It was suggested at the end of the preceding chapter that there can be no critique of knowing which precedes all knowing. Hence one cannot demand that an empirical study of science be preceded by an examination of the empirical method itself. Since science is empirical, however, the presumption is that this method is by and large satisfactory. It seems reasonable to believe, therefore, that the method is the best one to employ in studying science itself. If one wishes the assumption to be made explicit he may state it as follows: an empirical study of science is possible. This means essentially that one may study science just as the scientist studies nature. Supposing that the scientist is attempting to photograph nature, one may also presume the legitimacy of attempting to photograph the scientist in this act. But one must not make the mistake of supposing that it is only the scientist who is significant in the resulting picture; properly speaking, it is the *act* of the scientist which is the fundamental feature, and this act must be considered as tying both the *scientist* and his own *picture* of nature with *nature* herself. Thus it is really the knowledge of the scientist which one is studying, but this is considered only as having arisen through a certain act and as having been directed upon a certain object. If one does not insist upon this at the outset he runs the risk of committing the fallacy of vicious abstraction.

Furthermore one should recognize that the act which is, so to speak, the core of the scientific method is itself an event in the life of the scientist, and as such to be inserted into the background of his history. The pragmatist has called attention to this fact in recent years. Thinking, he has insisted, is a natural event happening in the life of a certain type of highly developed organism. It must therefore be understood genetically in terms of the situation out of which it arose and for which it became an instrument of adjustment. Thinking in general arises when the organism finds itself in a situation for which it has no ready and successful response; thought is explicable, therefore, as a somewhat complicated trial and error response, the ultimate aim of which is the restoration to the organism of its previous state of composure. This makes all reasoning, and derivatively all science, instrumental in character, and hence inexplicable apart from the situation for which it is the tool. The scientist solves problems because they are irritating to him, just as the fox seeks food in order to allay his feeling of hunger, and one finds it impossible to explain either the intelligence of the scientist or the slyness of the fox apart from these facts.

While this is an important revelation, credit for which is due to the pragmatist, its full implications make a consideration of thought apart from problems impossible. But the very essence of the empirical method involves a recognition of the legitimacy of isolating systems for purposes of study.[1] In every experimental situation certain forces, usually of minor intensity, and having their loci in remote points, are assumed to be negligible. For example, in describing the rolling of a ball down an inclined plane, the attractions of the fixed stars may be neglected. This does not involve a denial of the existence of such forces, but simply a convenient neglect of them for purposes of simplification. It is similar with reference to the background of thought. Though thinking arises out of problem situations and exists only as an instrument of adjustment, one may conveniently neglect this fact in his analysis of the scientific method. He may consider thinking as a system which is temporarily isolated. This simplifies the problem, and necessitates only that in the final description the possible disturbing influences be reintroduced through some method of successive approximation.

One should also recognize that in speaking of the logic of science as an empirical study he can hardly expect its data to have the same clarity as those, say, of physics. The physicist is confronted with readings of pointers on scales, deflections of needles on galvanometers, clocks, and meter sticks, and other *sense-data.* These constitute "hard" data, about whose existence there can be little doubt, and as to whose character there can be little disagreement. But the data for the logician of science are elusive and vague. They consist of the "ideas" and "mental processes" of the scientist as these are described by the psychologist either in introspective or in behavioristic terms. They consist of symbols—pictures and diagrams drawn by the scientist, words and mathematical formulas uttered and written by him—but these symbols are considered not as mere physical existents such as configurations of chalk, or ink, or series of noises; they must be examined in their "meaning" aspect, and this is their characteristically elusive feature. Further

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[1] This point has been well emphasized by H. Levy, *The Universe of Science,* (New York: Century, 1933), especially Chapter II. More detailed examination of the function of isolation in science will be made in Chapter VI.

data consist of the behavior of the scientist as he manipulates telescopes and microscopes, as he applies burners to flasks containing liquids, as he dissects organisms—but here again the significant aspect is not so much what the scientist is doing in a physical way as what he is "thinking about." And this can be determined only by asking the scientist, which involves all of the difficulties associated with the formulation of the question in language, the possible misunderstanding on the part of the scientist, his own doubtful analysis of his mental processes, and the obstacles in the way of communicating the results of this analysis to another.

However, the elusive character of the data for the study does not compel one to abandon the empirical method. It suggests, rather, that one should ascribe to his results no more certainty than they are entitled to claim. It suggests that he should recognize the dependence of his conclusions on the results of psychology, biology, linguistics, and mathematics. It suggests, in other words, that the logic of science is highly complex, and subject to revision as the more elemental sciences of which it is composed progress and develop. When psychology and biology are able to tell the logician of science more about the symbolic process, when linguistics is able to tell him more about the nature of symbols and meaning, when mathematics and logic are able to tell him more about the structure of deductive schemes, then he will speak with greater authority in the field of the logic of science. Until then he must recognize that his study is, so to speak, twice removed from reality, and that its pursuit, while empirical in character, must be subject to limitations and inadequacies by virtue of the obscurity of its data.

When one turns to the actual analysis of scientific cognition, though he finds it to be complex, he can readily discern certain characteristic elements and certain dominant features of structure. The elements are four in number. In the first place, there is the scientist—a self or person who instigates and controls the activities of investigation and formulates their results in a system of symbols. In the second place, there is the world of nature or fact, upon which these knowing activities are directed; this may be called the subject matter of science. In the third place, there are the actual activities themselves—observing, measuring, experimenting, reasoning, verifying; these taken together and with others constitute the method of the scientist. Finally, there is the result of the activities, the knowledge itself, which may be looked upon as a system of symbols or ideas either held privately by the scientist or communicated freely to others who are coöperating in the enterprise.

It is a great convenience to be able to speak of these four elements of scientific cognition by means of a terminology which does not create any prejudices as to their essential natures. For this reason they will henceforth be spoken of as *the knower*, *the known*, *the knowing*, and *the knowledge.* By employing these derivative forms of the verb "to know" one can avoid the risk of assuming that the subject matter of science is a world of matter, or of space-time events, or of sense-data, or of phenomena; or that the method of science is observation, or intuition, or induction, or deduction; or that knowledge is a complex of "psychical ideas," or images, or words, or mathematical symbols. One can talk freely about the elements and the way in which they function in the total situation without attributing to them features which are the result of certain theories of cognition.

Superficial analysis reveals the general way in which these four elements are structurally united in the cognitive situation. It is primarily a causal complex in which knowledge functions as the effect, and the knower, the known, and the knowing together constitute a complex cause. Roughly speaking, the *knower* operates upon the *known* in a manner called *knowing* to produce *knowledge.* The complex cause is sufficient to produce the effect, and each of the elements of the cause is necessary. This conception does not assume any specific theory of causal connection but merely that when a knower is confronted with something to be known and sets the knowing activities into operation the outcome is usually knowledge. It states further that in the absence of any one of these elements knowledge will not normally occur.

Within this general matrix a great variety of interpretations of cognition is possible. One of these is the common sense theory which will be examined in a later chapter. Others, more critical in character, may be found in the history of philosophy, particularly since Locke. The theory which will be proposed does not claim to be completely adequate. Its justification lies mainly in the fact that it was formulated with the scientific activity in view, and may be said therefore to have been empirically derived from the very situation which it was devised to explain. It is an abstract theory, and endeavors to ascribe to cognition only the very minimum of properties which it must have if it is to meet the empirical demands of knowledge. Clearly, some very general relations must hold between the four basic elements of the cognitive situation if the theoretical structure is to satisfy the demands of empirical thought. What are these minimum requirements? [1]

THE KNOWN

One of the facts which has been brought to light in recent years is the impossibility of ascertaining the subject matter of science prior to science itself. This applies not only to the individual sciences but to the totality of the sciences. Physicists who begin by talking about matter end by considering electrons, fields of force, and probability waves; mathematicians who limit their subject to number, order, and quantity find themselves talking ultimately about the structure of deductive systems; biologists who insist that their discipline is a study merely of the reactions of living

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[1] Since the knower functions in the total situation in an impersonal way, he may be neglected in what follows. This does not mean that the scientist is an unimportant part of science. It means, rather, that the method of science is one, and that variations in the character of the outcome are due to individual differences in the skill with which the method is employed. The neglect of the scientist in the analysis of the logical structure of science does not imply a failure to recognize the place of genius.

organisms to stimuli of various kinds find themselves obliged to talk about the chemical structure of organic compounds. From a more general point of view, all attempts to limit science to the natural as opposed to the supernatural, or the real as opposed to the unreal, or the measurable as opposed to the unmeasurable, end with the recognition that the drawing of such lines is precisely the scientific activity itself. The task of science toward the supernatural, the unreal, and the unmeasurable is the application of scientific techniques in such a way as to transform them into the natural, the real, and the measurable. The miracles of yesterday are the normal phenomena of today; hallucinatory objects of a former age are now explicable in terms of biological and psychological laws; value attitudes, once thought to be purely intensive in character, are now being portrayed as graphs on coördinate systems.

The result of all these considerations is that science becomes reluctant to characterize more specifically than is necessary the type of entity about which it talks. Science may justifiably study anything whatsoever, anything which may be given in any sense. The realm of the known for science is the realm of phenomena, whatever appears or exhibits itself in a situation to which the general methods of knowing are applicable. This will include the natural and the supernatural, the real and the unreal, the measurable and the unmeasurable, the material and the immaterial, the public and the private, the past, present, and future, the abstract and the concrete, the large and the small, the extended and the brief. But science will not *begin* with this unlimited complex; it will rather select those of its aspects which are most clearly given and to which the methods of science are most readily applicable. It will direct its efforts, therefore, to the understanding of the natural, the real, the measurable, the material, the public, the present, the relatively concrete, the intermediary between the large and the small, and between the extended and the brief. But since this represents simply a convenient selection, the remaining elements will be placed on the shelf to be subjected to later examination. They are not taken out of the given but set aside in view of the abundance of phenomena which are more clearly given and more directly susceptible to the methods of science.

Recent literature in the philosophy of science seems to have adopted the term *event* as the most adequate name for the basic entity of science. The advantage of this term is its high abstraction. Almost anything that one may mention is an event—durations, spaces, relations, and numbers, things as well as processes, happenings in the physical realm as well as in "minds." The realm of the known may therefore be described as a complex of events. The events bear to one another relationships of various kinds—spatial, temporal, causal, similarity and difference, equality and inequality of magnitude. These relationships determine associations and dissociations of events, containing and contained events, dependent and independent events, elemental events, collections, organic wholes, and *Gestalts.* Thus in speaking of the realm of the known as a complex of events one does not deny that it may also be an event, i.e., events may unite together to form a system having certain unitary properties.

THE KNOWLEDGE

In the most general sense, knowledge may be defined as the awareness of the realm of events. That which is to be known by science becomes known when it enters into the proper relationship to the scientist. This relationship is the activity of becoming aware. Thus knowledge may be defined as that which terminates the activity of awareness when this is directed by the scientist upon the realm of events.

But to stop the analysis at this point would be to create an impression of pseudo-simplicity. Knowledge is actually a highly complex affair, exhibiting different aspects, manifesting itself under varying degrees of adequacy, and maintaining conflicting claims with reference to its descriptive values. One must, therefore, consider this element of cognition in greater detail.

The important feature of knowledge seems to be that when one knows the realm of events he is aware of it in different ways, and knowledge itself is a composite of these different kinds of awareness. A convenient approach to this problem is through a distinction suggested by Russell between "knowledge by acquaintance" and "knowledge by description." "We shall say that we have *acquaintance* with anything of which we are directly aware, without the intermediary of any process of inference or any knowledge of truths. Thus in the presence of my table I am acquainted with the sense-data that make up the appearance of my table—its color, shape, hardness, smoothness, etc.; all these are things of which I am immediately conscious when I am seeing and touching my table. . . . My knowledge of the table as a physical object, on the contrary, is not direct knowledge. Such as it is, it is obtained through acquaintance with the sense-data that make up the appearance of the table. . . . My knowledge of the table is of the kind which we shall call 'knowledge by description.' The table is 'the physical object which causes such-and-such sense-data.' . . . All our knowledge of the table is really knowledge of *truths*, and the actual thing which is the table is not, strictly speaking, known to us at all." [1] Elsewhere he speaks of descriptive knowledge as knowledge expressible in the form "a so-and-so" and "the so-and-so," and he suggests that when we know descriptively through propositions, "it is only what we may call the *concept* that enters into the proposition." [2] "Common words, even proper names, are usually really descriptions." [3]

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[1] Bertrand Russell, *Problems of Philosophy* (New York: Henry Holt, 1912), pp. 73–75.

[2] Bertrand Russell, *Introduction to Mathematical Philosophy* (London: Allen and Unwin, 1920), p. 168.

[3] Bertrand Russell, *Mysticism and Logic* (London: Longmans, Green, 1921), p. 216. For another formulation of the distinction see A. N. Whitehead, *Symbolism, Its Meaning and Effect* (New York: Macmillan, 1927), pp. 7 *et seq.*

While Russell's distinction is of basic importance in the understanding of cognition, it requires reinterpretation and supplementation to be made adequate to the scientific situation. In the first place something must be said with reference to the *directness* which characterizes knowledge by acquaintance. The presumption is that in acquaintance one knows events directly, without the intermediary of anything which is symbolic in its nature, and by this means one avoids the difficulty of the common sense position which, as we shall see later, considers mind to be limited to its own contents. What is the nature of this directness, and what is the nature of the event which is thus known? Since the detailed solution requires a consideration of the complicated problem of the theory of knowledge, what is suggested here may be taken for a mere outline, to be partially filled in by the discussion of Chapter V. By the directness of knowing, one means only the basic fact that in all cognition there must be a situation which one can describe by the sentence: "I am now aware of such-and-such." The presumption in a situation of this kind is that the "I" is directly in contact with the "such-and-such" in the sense that the latter is known without any intermediary. The fact of direct acquaintance does not prohibit the use of instrumental techniques and measuring devices. It demands only that recording machines be recognized for what they are, and not as substitutes for the observer. A registering instrument must be *read* before it can affect knowledge, hence it is the instrument itself which is directly known, though it may convey inferential knowledge as to something else whose character it is registering. One must be directly acquainted with pointer readings if he is to be indirectly aware of that which they measure. Even if one supposes that his sense organs are recording devices, giving him information as to an external world, still he must suppose that he is immediately aware of the sense-data though he may be only inferentially aware of their causes.

In the second place, it seems important to recognize that acquaintance must be not only direct but *non-transforming* as well. It is an unavoidable assumption of science that there is at least one kind of awareness which does not transform events, i.e., there must be an awareness which does not enter functionally into its object, hence may be neglected without prejudice to the character of that which is known. Awareness of this kind is a *bare* awareness which is "external" to its object and therefore without effect upon it. The reason for this assumption seems clear. A transformation is any act performed upon something given to produce something else. Hence to say that awareness transforms implies that it is performed upon something given. But a thing can be given only in awareness, with the result that there must be at least one kind of awareness which does not transform.

What should be emphasized here is only the fact that in awareness there is always something with which one is acquainted. In the great majority of situations of actual knowledge this direct awareness is associated with an inferential act, often unconscious, by which another object is called up before awareness. These two objects are frequently confused, and it is often impossible to tell which one is directly perceived. For example, it is difficult to say whether one actually sees the softness of velvet or infers it through the medium of past associations, and it is not easy to decide whether a table-top is seen in its "true" rectangular shape, or in its "apparent" shape as a parallelogram. In both of these cases one object is given in direct awareness, and the other is the result of inference. It is sometimes said that the directly given object becomes the symbol of the inferred object. Our limitation of the word "symbol" to its cognitive use would make such a formulation confusing. But the fact of inference seems undeniable, and all that need be insisted upon is that an inferential act demands something which is given as the starting-point for the act. This given may be said to enter into knowledge through an act of direct and non-transforming awareness.

In the third place, Russell's knowledge by description should be recognized for what it is, viz., symbolic knowledge. Symbolic knowledge is *indirect*. When one knows an event descriptively he is not presented with the event; instead he is directly aware of a symbol which has the peculiar power of referring to the event by virtue of a meaning character. Knowledge of this type is truly representative, since the symbol acts as a substitute for the event. Hence one knows an event symbolically when he knows it through words, pictures, images, models, diagrams, indices, and gestures. As will be shown in Chapter IV, symbols are of various kinds depending on the way in which they refer to events. Consequently there are different ways in which one may be aware of events symbolically. But in all cases one is directly aware of a symbol (which is, of course, an event) having the capacity of meaning or referring to another event of which the individual then becomes indirectly aware. Though Russell calls this type of knowledge "descriptive," it will here be called "symbolic" since a later chapter will show that there are two ways in which one may symbolize events—by description and by explanation.

In the fourth place, knowledge by acquaintance and knowledge by description, or symbolic knowledge, cannot constitute an exhaustive analysis of the aspects of cognition; there must be a third type by means of which the two are united. If the event is presented through acquaintance and characterized through symbolic knowledge, there must be a kind of awareness by which one may test the application of the symbol to the event. This must be a composite awareness which combines acquaintance and symbolic knowledge, and adds the awareness of the relation which the symbol bears to the event. One may be aware of a bit of metal, and he may know what iron is, but neither of these types of awareness will inform him whether the bit of metal is iron. In the one case he is aware only of the event, in the other he is aware only of the symbol; knowledge requires that he be aware of the applicability of the symbol to the event. This type of awareness is required in the movement of scientific discovery where events are identified by means of symbols, and in the movement of scientific verification where symbols are verified by means of events. In fact awareness of this kind may always occur in conjunction with acquaintance; there probably is no such thing as a bare awareness of an event without some awareness of a symbol which is applicable to it. Intuitions without concepts are blind, concepts without intuitions are empty. What is also required is an activity of *perception* in which the applicability of the concepts to the intuitions may be ascertained.

With these three aspects of knowledge clearly indicated, one can see what is meant by saying that knowledge is the awareness of events. It is a composite awareness which includes acquaintance with a realm of events, awareness of a system of symbols referring to a realm of events, and awareness that the realm of events referred to by the system of symbols is precisely that given in direct awareness. This, of course, represents knowledge in its ideal form. Actual knowledge falls short of this to greater or lesser degrees. No scientist is acquainted with the entire realm of events; he is obliged to make selections from it on the basis of the readiness with which the events are given for examination, and, as a consequence, his knowledge is only partial. No scientist has devised a system of symbols which is adequate either from the point of view of its internal organization or with regard to its scope. No scientist is ever satisfied with the directness in the reference of his symbolic scheme to the realm of events; it is usually an idealization or schematization which must be made more and more adequate by successive approximations. Hence actual science fails in any or all of these aspects of knowledge, and to this extent is relatively inadequate. But the presumption is that by the proper application of the techniques of knowing it may be made progressively better. This fact necessitates a consideration of the knowing activity.

THE KNOWING

By the knowing is meant the total complex of activities which result in knowledge. More specifically, it is the aggregate of activities devised to acquaint the scientist with events, to enable him to derive (or discover) a system of symbols representative of this realm, and to verify the symbolic scheme by comparison with events. The traditional formulation of the scientific method in terms of observation, induction, and deduction, represents an approximate description of the knowing technique in terms of these three features. Each of these aspects will be subjected to analysis in the following chapters. For the present each of them may be considered briefly.

Since the point of origin of all science is the world of events, the initial technique in the knowing activity must be concerned with *the establishment of situations in which the direct awareness of events is possible.* It is a commonplace that science begins with facts. But the discovery of these elemental facts is often a complicated matter. Facts are not all displayed to view and open to direct inspection. Most obviously, facts can be observed only in the proper spatial and temporal situations. Many events in nature, such as eclipses, occur only at temporal intervals, and one must therefore wait for them to take place. Other events occur only in certain localities, and one must go to these areas to experience them. Still other events, though given in the here and now, may be too minute or too brief to enter readily into awareness; in such cases one must employ instrumental aids such as microscopes and slow-motion pictures. Often one can overcome these difficulties by reproducing events in the laboratories under experimental conditions; he can put his fingers into nature, so to speak, and thus produce observational situations which either would not normally occur at all in nature, or might occur only under undesirable conditions such as in a remote space or time, in very intense or very weak manifestations, or in the proximity of various interfering factors. Many events are known primarily in terms of recording devices, such as deflections of needles, photographic images, and pointer readings of various kinds. The point with reference to all of these illustrations is that something is presented for direct awareness, and that certain complicated techniques are necessary in order to bring about situations in which such activity is possible. Bacon suggested that the scientist proceeds by collecting facts. His conception of the scientific method, as Jevons points out, was a "kind of scientific bookkeeping. Facts were to be indiscriminately gathered from every source, and posted in a ledger, from which would emerge in time a balance of truth."[1] The obvious truth in his method is somewhat obscured by the fact that the behavior of the scientist is one of activity rather than of passivity. Nature is often reluctant to speak. Her utterances are frequently produced only by prodding, and even then one is satisfied with her statements only when he has introduced elaborate recording and measuring devices. The fact that one's recording instruments may lose the essence of that which he is trying to observe need not be emphasized here, though it becomes a problem of some significance in the deeper consideration of science. For present purposes one need only reëmphasize the fact that at some point in the scientific setup the investigator is obliged to say "I am now experiencing such-and-such." Without such an initial *given* science becomes meaningless.

But knowing is not merely the becoming aware of events; it is also the *symbolizing* of them. The scientist knows nature not only directly but mediately in terms of models, pictures, and diagrams, in terms of mathematical equations, and in terms of words and sentences. Hence the second significant aspect of the scientific method is the large group of highly intricate activities constituting the invention or discovery of a symbolic system which is presumed to be the *interpretation* of the realm of events. Unfortunately, very little is

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[1] W. S. Jevons, *The Principles of Science* (London: Macmillan, 1907), p. 576.

known as to the nature of the techniques which are employed at this stage of the knowing process. Many insist that there is to be found here an ultimate irrationality—the mystery of scientific discovery—which is explicable only in terms of that still more basic mystery, scientific genius; the fact seems to be that some scientists make significant discoveries and others do not—and that is all that may be said on the question. Others insist that there are techniques for discovery. Still others maintain that the problem has not been properly formulated; the scientist does not *discover* a symbolic scheme and then suppose that the entities about which he is talking have real existence; on the contrary he *invents* it and then projects it upon the world as a pure construction and merely a convenient mode of explanation. One is forced to recognize, therefore, that one's view as to the nature of the knowing activities depends upon his theory of the nature of the scientific task in general. But it also depends upon his conception of the nature of symbols, the way in which they mean and take on meanings, the way in which they unite to form symbolic schemes, whether they claim to *explain* or merely to *describe*, and so on. All of these considerations need simply to be mentioned at this early stage. The point is that the scientist succeeds by means of certain techniques, conscious or unconscious, in becoming aware of a body of symbols through which he claims to know the world of events. The important feature of this symbolic system is its relative flexibility as compared with the realm of events. Events have a given character which is independent of the observer; but symbols may be modified in such a way as to become progressively more adequate in their representative task. A knowledge of the knowing techniques is helpful in this task of constructing a satisfactory symbolic scheme. But such knowledge is not sufficient to bring about an immediate realization of the ideal of a perfect system of symbols. For the proper techniques are not yet known, and even those which are known are not always correctly employed. Hence it is the fate of every system of symbols to be tentative only, and to be more or less completely abandoned as the realm of the given becomes expanded and as the techniques of knowing become more accurately formulated.

The fact that the construction or discovery of symbolic schemes does not seem to be subject to control by any recognizable techniques necessitates a third aspect of the scientific method which is usually called *verification.* If symbols are loosely tied to events so far as their origin is concerned, they must be fastened to events more firmly through the technique of testing and checking. It is the function of the elaboration of symbolic schemes to disclose new points at which knowledge may be tied down to the real; thus more or less freely expanded symbolic systems are instruments through which discoveries become possible in the realm of events. But this escape of thought from the realm of the given is also a source of error; symbolic schemes may be falsely elaborated, with the result that they become less rather than more adequate. Hence a return to the realm of events is obligatory. There is presumed in every verificatory act some conception of the possibility of a correlation between the body of symbols and the realm of events. Whether this is considered to be an actual correspondence, or mirroring, or whether it is supposed to be merely a pragmatic workability, depends, again, upon one's general theory of the nature of the scientific task. Whatever the theory, the testing is a fact. One must admit, then, a stage in knowing in which the scientist is at once aware of events, of symbols, and of the applicability of the symbols to the events.

It is, perhaps, unnecessary to point out that in concrete reflection the stages of knowing are only vaguely distinguishable from one another and may vary greatly in the respective parts which they play in constituting the total knowing act. For the Baconian scientist the initial exploration of the realm of events is the most important, if not the exclusive method of knowing. Bacon, at least in one of his phases, insisted upon the importance of making as complete a collection of data as possible before passing to the act of interpretation; the imagination should be held in leash until the scope of the facts is broad enough to permit one actually to read the explanation out of them. According to such an analysis the stage of verification is superfluous. For the speculative scientist, on the other hand, the act of discovery is the most important phase. Data should be collected, to be sure. But one should not waste valuable time poring over the facts in the faint hope that the discovery of just one more datum will set off the mind in the proper direction. Data should be limited to a minimum, and the speculative activities should be set into operation at the earliest possible moment. The stage of verification then becomes of increasing importance, for only in this movement can one check the flight of the mind, which, in the absence of specific data, may have been extensive.

The importance of *symbols* in the total structure of science demands that a more detailed analysis of this notion be made. This will be the topic of the following chapter.

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CHAPTER IV

THE NATURE OF SYMBOLS

It should be clear from the discussion in the preceding chapter that the scientific activity is centered about the attempt to devise a system of symbols which claims to be representative in some sense of a realm of events with which the scientist has direct acquaintance. The important point to note is that mere awareness of events is not knowledge, though it is the foundation upon which knowledge builds. Events enter properly into knowledge only when they are interpreted, described, or explained, and these supplementary activities require the use of symbols. Whitehead suggests that there is a great difference between direct knowledge and symbolism. "Direct experience is infallible. What you have experienced, you have experienced. But symbolism is very fallible, in the sense that it may induce actions, feelings, emotions, and beliefs about things which are mere notions without that exemplification in the world which the symbolism leads us to presuppose."[1] It seems questionable, however, whether presentational immediacy ought to be spoken of as infallible, since it is really not knowledge, and hence not the kind of thing which can be either fallible or infallible. The function of acquaintance is simply the production of that givenness which is presupposed in all knowledge. There are operations by which this may be brought about, but they are not properly cognitive techniques; they are essentially exploratory and manipulatory. On the other hand, the techniques which result in the formation of a symbolic scheme are significantly cognitive in character; they do or do not terminate in knowledge according as they are or are not correctly employed. Hence the consideration of the nature of symbols, how and what

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[1] A. N. Whitehead, *Symbolism, Its Meaning and Effect*, p. 6.

they mean, how they are differentiated into kinds, plunges one into the very heart of the cognitive situation. In the present chapter the attempt will be made to examine symbols and to describe their features so far as this is possible in the limited space available.

SIGN SITUATIONS

The most convenient approach to the notion of symbol is through a consideration of the more general idea of *sign.* "If we stand in the neighborhood of a cross road and observe a pedestrian confronted by a notice *To Grantchester* displayed on a post, we commonly distinguish three important factors in the situation. There is, we are sure, (1) a Sign which (2) refers to a Place and (3) is being interpreted by a person. All situations in which Signs are considered are similar to this." [1] The accuracy of this as a statement of sign situations in general is somewhat vitiated by the fact that animals behave in a way which leads one to believe that they react to signs. The dog who runs to the dining room at the sound of the dinner bell is exhibiting a type of behavior which can, in all fairness, be called interpretative response. Hence the limitation which is introduced by Ogden and Richards under (3) does not seem to be necessary for the description of general sign situations. For the rest, however, the characterization seems accurate. There must always be something which can be called a sign. This may be anything whatsoever, subject only to the condition that it has the capacity to refer or point when properly interpreted. Thus a flag at half-mast may signify death, a cloud may signify rain, and congested lungs may signify pneumonia. Furthermore, the sign must have the capacity to refer to something outside itself. This, again, may be anything whatsoever, subject only to the condition that it is the terminus of an act of signification. Thus the word "Entrance" may signify the actual doorway beneath the sign, an olive branch may

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[1] C. K. Ogden and I. A. Richards, *Meaning of Meaning* (New York: Harcourt, Brace, 1930), p. 21.

signify the general notion of peace, and the arrow on a weather-vane may signify north.

SYMBOL SITUATIONS

Within this general type of sign situation there are certain specific types, of peculiar importance to man, which may be called symbol situations. Ogden and Richards suggest that these situations should be characterized as those in which men use the signs as instruments of communication. Symbols may then be specified as those signs which are words, images, gestures, drawings, mimetic sounds. Such symbols are used by men to produce certain responses in other men. The interpretative act, as a result, becomes limited to the higher organisms, and is commonly understood in terms of such psychological notions as association, apperception, suggestion, and intension.

But symbol situations may be primarily *emotive* or primarily *cognitive*. This distinction is based upon the two important functions of symbols. Symbols are instruments for the conveyance of feelings and attitudes, and they are instruments for the conveyance of information. The former may be called the emotive function of symbols, and the latter may be described as the cognitive function. Probably all symbols have both emotive and cognitive properties. But some symbols are primarily emotive and others are primarily cognitive. Many words call up pleasant or unpleasant sensations more directly than they call up ideas. Political speeches and patriotic orations in oral discourse, and poetry in the written form abound in emotive appeals. Abstract science, on the other hand, especially mathematics, is constituted primarily by cognitive symbols; the function of such symbols is to *say* something and not to create feelings.

Since emotive symbols play a minor rôle in science they may be neglected for the purposes of this study. Henceforth by "symbol situation" will be meant "cognitive symbol situation," and symbols will be presumed to be significant in a purely cognitive way. One should be able to discover in any symbol, then, (1) the event itself, which is the point to which the referential property is attached; (2) the referential property, which somehow connects the event which is the symbol with another event which is that referred to by the symbol; and (3) the psychological interpretation of this referential property when it enters into the awareness of an individual. In order to make these three features clear they may be discussed in greater detail in connection with some particular symbol, say, the word "circle."

(1) Every symbol is an event or happening. It may consist of a written word made up of particles of ink arranged in a certain pattern; it may be a noise uttered by a person in an act of communication; it may be a drawing, or photograph, or a material model; it may be a group of mathematical symbols. Thus in the example one might replace the written word "circle" by the spoken word, or by a picture of a circle, or by the equation $x^2 + y^2 = r^2$; in all such cases one would have what might be called physical symbols of a circle. On the other hand, the symbol might consist simply of the gesture of pointing to a circle, or of the mental image which a given individual has of a specific circle; in the former case the symbol would be called biological, and in the latter case psychological. But in all cases the symbol would be an event occurring roughly at a time and place, and capable of analysis and description by the ordinary techniques of science. Ideally there would be a strict correlation between the properties of the symbol as a mere event and its meaning. Actually this is not the case since in language the same word may have different meanings, and different words may have the same meaning. There is, however, some correlation between the two; at least one cannot determine what the symbol means until he has ascertained its character as a physical, biological, or psychological entity. Often this recognition is spontaneous, but it is presumed in each individual case.

(2) Every symbol refers to something other than itself.[1]

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[1] This is not strictly true. For example, "word" refers to itself as well as to something other than itself.

This feature of a symbol is complex and involves (*a*) the referential property or relationship, and (*b*) the entity which is referred to. But these two features of the symbol do not belong to it in the same sense. The referential property must be given with the symbol; without it the symbol has no meaning and is therefore not a symbol. But the *referent* (as contrasted with the *reference*) need not be given in the situation at the moment except under the form of another symbol. To consider the example, the word "circle" must have a referential character in order to be a symbol, i.e., it must have the power to point to what would be identified as a circle were it given; but no actual circle may be given, though one may find himself aware of an image of a circle, or of certain words such as "equality of radii," "conic section," "two-dimensional plane figure," etc., and he may believe that such a thing as these is the referent of the symbol. It is the essential virtue of symbols that one can use them in the absence of their referents, and, hence, as substitutes for those events to which they refer. Just *how* symbols refer to events is hard to say. It is convenient to picture the referential property of a symbol by means of an arrow; this suggests in a vague sort of way certain of the features of the reference. It suggests, for example, if the arrow is properly drawn, that the reference somehow takes its origin in the symbol itself as a mere event, and goes out of and away from this source in the direction in which its referent is to be found. It also calls attention to two of the most important features of the reference, viz., its asymmetric character, and its capacity to limit the range of possible events referred to. For if the word "circle" refers to an actual circle, the actual circle cannot refer to the word in the same way. And through the word one is able to know, in advance of seeing the actual circle in question, the *kind* of event to which reference is made. Furthermore, the arrow is a convenient mode of representing the referential property because an arrow may point without pointing to anything, i.e., it may have a general directional indication but when one examines the locality thus called to his attention he may find nothing. Symbols enable one to talk about absent objects, but, unfortunately, by virtue of this same capacity, permit him to talk about objects which are not even known to exist at all. Hence it is an important feature of symbols that one may use them without any prejudice to the existence or non-existence of their referents.

It may be well at this point to clarify a distinction in the referential character of symbols, which will be of basic importance in later chapters. As was pointed out in the preceding paragraph, symbols have the power not only to point to events but also to call to mind other symbols with which they apparently have significant relations. This suggests that every symbol has two very important types of relationship: (1) reference to events which are not themselves normally symbols;[1] and (2) reference to other symbols. These two types of reference are approximately what are called in the traditional logic, as applied to concepts, *extension* or *denotation*, and *intension* or *connotation.* The term "intension" is here used in a slightly broader sense than it has been historically, since it applies to all of the relations of a concept to other concepts and not merely to its defining relations. Furthermore, the notions of extension and intension are generalized so as to be applicable to all symbols rather than merely to concepts. In this general sense, then, one may say roughly that extension of symbols is that property by virtue of which they assume relations with the realm of events, and intension of symbols is that property by virtue of which they assume relations with one another. The intensional relations, as applied to symbols of a verbal character, are usually called formal relations, and are illustrated by *implication*, *equivalence*, and *incompatibility;* they are the relations through which symbolic schemes are formed. The intensional relations of images, diagrams, models, and other pictorial symbols cannot be of this kind

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[1] The events may, of course, be themselves symbols, but then the symbols which refer to them are symbols of symbols.

but must be essentially pictorial in character. The extensional relations, on the other hand, are the foundation for the cognitive aspect of symbols since it is through this type of relation that one may be said to know events in terms of symbols. The character of the referential relation depends on the kind of symbol involved; images and diagrams are in some sense like their referents, hence refer by resemblance; but words do not in general either sound or look like their referents, hence refer by other routes.

(3) Symbols refer only through the intermediary of a mind. "Words, as every one knows, 'mean' nothing by themselves, although the belief that they did . . . was once equally universal. It is only when a thinker makes use of them that they stand for anything, or, in this sense, have 'meaning.' They are instruments." [1] Hence the only way to understand the referential character of words is to ask oneself what he experiences when confronted by them. Consider again the word "circle." There is, first, the awareness of the physical event, the word, which is the pattern of ink on paper; this, however, does not usually enter into clear consciousness and may not be distinguishable as an element of the experience. But from this initial experience there arises in a manner which appears to be causal a consciousness of the meaning of the symbol. Yet the causal relation is basically dependent on past experience; the awareness of the physical symbol calls up the awareness of the meaning through associative ties. Something is called to mind when one is presented with a word of which one knows the meaning. The fact that it is called to mind immediately, and without conscious effort on the part of the individual, leads him to believe that it is somehow a part of the physical word. However, the attempt to read in a foreign language soon convinces one of the contrary; here the associations must be made slowly and painfully before they arise with anything like the readiness of meanings in one's own tongue. Just what one is aware of when he is aware of a meaning, is hard

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[1] C. K. Ogden and I. A. Richards, *Meaning of Meaning*, p. 10.

to say. Eaton considers that the simplest solution to the problem is to accept a unique *meaning activity.* "The meaning activity is one of vague anticipation: the mind is poised expectantly, awaiting something other than the thing, the symbol, which is immediately before it; and this anticipation is *vague* because it is not accompanied by a belief that the object meant will appear or that it exists. When I mean an object I do, in some sense, prepare my mind for a presentation of this object. Though I cannot be said to turn my attention toward the thing I mean, since one cannot attend to something not presented to him, there is no doubt that I do more than attend to a symbol or an image. Indeed, I turn my attention away from the symbol or the image, and this constitutes the first step in preparation for the thing meant." [1] The fact that the meaning activity itself is vague creates all of the problems associated with the need for definition in language. Since one cannot say clearly what the meaning activity in general is, he cannot be sure in any given case whether a symbol is understood. Symbols may be graded in clarity, ranging from such words as "circle," where the meaning is clearly grasped, to purely nonsense symbols, where the verbal form suggests that there is a meaning yet no meaning is forthcoming. In between would be found all vague symbols, such as "love," "justice," and "self," where the meaning is relatively vague. If one uses the arrow to represent the referential character of the symbol in each case, he is obliged to say that the word "circle" has a definite arrow which points clearly to a limited area within which the referent is to be found; nonsense symbols such as "boojum" have no arrows at all, hence are not properly symbols; symbols such as "love," "justice," and "self" have arrows but they point to extensive areas within which their referents may be found. Definition removes obscurity in two ways, extensionally and intensionally. Extensionally, definition clarifies meaning by pointing to situations in which the

[1] R. M. Eaton, *Symbolism and Truth* (Cambridge: Harvard University, 1925), p. 23.

referent is given for direct awareness; intensionally, it clarifies by indicating relations to other symbols themselves presumably better known.

ADEQUACY OF SYMBOLS

As was suggested in the preceding chapter, an adequate system of symbols is presumed to have some sort of correlation with the realm of events. Presumably the highest type of adequacy would be one in which the system of events and the system of symbols are isomorphic with reference to each other. Two systems are isomorphic when there is a one-to-one correlation of the elements and relations of the respective systems. Ideally, therefore, a symbolic scheme should be constructed so as to contain a distinct element for every element of the realm of events, and a distinct relation for every relation in the realm of events. Furthermore, every event should be symbolized by one and only one elemental symbol, and every relation between events by one and only one relational symbol. This would enable one to say that in a very general sense the system of symbols mirrors or represents the system of events, and that the relation of the body of symbols or any element of it to its referent is the relation of picturing. Such a system would be highly adequate.

There is reason to believe, however, that although such a symbolic scheme would satisfy the demands of cognition perfectly, no actual system, however limited in scope, even approximates to that ideal. The reasons for this inadequacy of existent knowledge schemes are to be found in the nature of the knowing activities, and are at least two in number. In the first place, the human mind seems to be fundamentally incapable of grasping highly complex structures as a unit. Certain *Gestalts* of a very simple sort enter into direct awareness as complexes, but configurations of anything like moderate complexity must be grasped piece-meal. This fact is responsible for one of the basic scientific procedures, viz., the method of isolation. Since the human mind is incapable of grasping any event in all of its configurations, certain of its relations are more or less arbitrarily neglected, and are not included in the resulting symbol. As a consequence, every symbol is abstract in its representation of nature; it *loses some of nature*, and hence is not strictly adequate as a representative. Thus the realm of symbols is *less extensive* than the realm of events. But, in the second place, the human mind seems to be unwillingly restrained to what is given; it is prone to soar into flights of imagination and theorizing, which go beyond the limits of the obvious data and result in the formation of symbols having no known existential correlates. These methods may be purely imaginative, and guided by no recognized techniques, as in the case of poetry and much common sense reflection; or they may be subject to greater or less control, as in the case of science. But even in the latter case a greater variation in methods is possible. The operations by which the realm of the given is extended may be essentially fictive and creative, resulting in such entities as perfect levers, ideal gases, disembodied electrical charges, and mathematical points. Or they may be more closely tied down to the realm of the given in the sense that they are possible constructions out of it; a physical object, for example, may be defined as the class of its appearances,[1] a mathematical point may be defined as the series of "enclosing" events,[2] space may be defined as the totality of possibilities of relative position of bodies and phenomena.[3] Finally, the operations may be considered as semi-exploratory in character, and hence as resulting in entities which have at least a probable existence; such techniques produce what are ordinarily called hypotheses. What is important with reference to all of these entities is that they are not, at least at the moment of creation, considered as indubitably given. As a consequence one is obliged, by virtue of a characteristic activity of the mind,

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[1] Bertrand Russell, *Our Knowledge of the External World* (Chicago: Open Court, 1915), p. 155.

[2] A. N. Whitehead, *Concept of Nature* (Cambridge: Cambridge University Press, 1920), Chap. IV.

[3] V. F. Lenzen, *Physical Theory* (New York: Wiley, 1931), p. 51.

to insert into any symbolic scheme elements which are not known to have any direct representative value; the symbolic system *adds to nature*, and hence ceases to be strictly adequate. Thus the realm of symbols is *more extensive* than the realm of events.

This control of symbolic schemes by human factors, apparently working at odds to the theoretical ideal of knowledge, has seemed to many to argue for the necessity of recognizing an essentially pragmatic factor in knowledge. Knowledge, it is claimed, must be adequate as a representative of events, but it must also be adequate to the knowing demands of the human organism. Often, as in the cases mentioned above, these two types of control are in conflict, and one is obliged to make a choice between them. Consequently it is one of the tasks of the philosophy of science to make an analysis of all scientific schemes with a view to showing which of their elements are traceable to that which is to be known and which are traceable to the knowing operations. The results of some of these analyses will be discussed in Chapter VIII.

If one grants, then, that an adequate symbolic scheme is isomorphic with reference to the realm of events, and that every scientific system is more or less adequate depending on the extent to which the knowing operations transform and create, the problem immediately arises as to whether there may be degrees of adequacy within the scope of isomorphism. Are there some types of symbol which are more adequate than others? Apparently, one-to-one correlation implies only a highly abstract identity, hence there need be no specific similarity between the elements and relations of the one system and those of the other. What seems to be required is merely that if there are different elements and different relations in the one system, there must be the same differences in the other. This seems to be about all that one can say with regard to the general referential character of symbols. But what may be said as to the adequacy of specific kinds of symbols?

KINDS OF SYMBOLS

Some symbols are more directly pictorial than others. Images, for example, are more directly representative than verbal descriptions, and models convey information about their referents more directly than do mathematical equations. Hence one may conveniently arrange symbols on a scale according to their mirroring capacities. At the one extreme will be that type which Peirce calls an icon. This is defined by him as any symbol which "may represent its object mainly by its similarity." Illustrations of icons are to be found in *images* "which partake of simple qualities"; *diagrams* "which represent the relations, mainly dyadic, or so regarded, of the parts of one thing by analogous relations in their own parts";[1] and *metaphors* which represent through parallelism. "It is a familiar fact that there are such representatives as icons. Every picture (however conventional its method) is essentially a representation of that kind. So is every diagram, even although there be no sensuous resemblance between it and its object, but only an analogy between the relations of the parts of each." [2] At the other extreme will be those which are not primarily pictorial. Here will be found practically all words [3] and probably most mathematical symbols other than the direct representation of spatial figures.[4] These symbols which have only a remote pictorial value may be called, for want of a better term, *characterizing* symbols, since they point to their referents by

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[1] C. S. Peirce, *Collected Papers* (Cambridge: Harvard University, 1932), Vol. II, par. 2.276 and 2.277.

[2] *Ibid.*, par. 2.279.

[3] One must say *practically* all words, for there are many which retain their pictorial character in their sounds, such as "pitter-patter," "bow-wow," and similar onomatopoetic words, and others which retain their pictorial character in their appearance, such as Egyptian hieroglyphics.

[4] Peirce considers mathematical symbols to be icons. "A distinguishing property of the icon is that by the direct observation of it other truths concerning its object can be discovered than those which suffice to determine its construction. Thus, by means of two photographs a map can be drawn, etc. Given a conventional or other general sign of an object, to deduce any other truth than that which it explicitly signifies, it is necessary, in all cases, to replace that sign by an icon. This capacity of revealing unexpected truth is precisely that wherein the utility of algebraical formulae consists, so that the iconic character is the prevailing one." *Ibid.*, par. 2.279.

meaning them rather than by portraying them. It still remains true, as Wittgenstein has insisted, that every proposition is a picture of a fact. But one cannot ascertain the fact by a mere examination of the symbol as a physical event; one must know what the symbol means. In fact one may say that the difference between icons and characterizing symbols is that in the former the determination of the referent must be made primarily by examining the symbol itself and only incidentally by examining the reference, and in the latter it must be made primarily by examining the reference and only incidentally by examining the symbol itself.

Supposing this general distinction between icons and characterizing symbols to be valid, is there any reason why the one type of symbol should be preferred to the other? The problem has been formulated in terms of the rival claims of pictorial representation, or models, and conceptual representation, or abstractions. Whether the icon is a mental image or a physical model consisting of pulleys, wires, and rubber tubes is of no importance in the present considerations; both are types of pictorial symbolism, one somewhat ephemeral and intangible, the other permanent and material. The insistence upon this type of concrete imagery has characterized a group of English physicists, among whom are Faraday, Lord Kelvin, Lodge, and Maxwell. Kelvin, for example, says in effect that the question, Do we understand a certain physical subject or not? means Can we construct a corresponding model? [1] "I advise you all who are engaged in teaching, or in thinking of these things for yourselves, to make little models." [2] Lodge puts this plan into practice when he explains modern theories of electricity [3] by diagrams of machines involving pulleys and cords, weights and drums. As long as one limits himself, he insists, to the abstract schemes of mathematicians who are "able to live wholly

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[1] W. T. Kelvin, *Baltimore Lectures on Molecular Dynamics* (Baltimore: Johns Hopkins, 1884), p. 131.

[2] *Ibid.*, 2nd ed., 1904, p. 34.

[3] Oliver Lodge, *Modern Views of Electricity* (2nd ed., New York: Macmillan, 1892), *passim.*

among symbols, dispensing with pictorial images and such adventitious aids"[1] one does not truly understand the real phenomena. On the other hand there is a large group of scientists, among whom are Hobson, Mach, Ostwald, and Poincaré, who believe that images and models are apt to be misleading and should be replaced by abstract conceptual schemes and word-representations. Probably there would also be found in the latter group all of those who argue that physical events are simply measurable entities which can be symbolized only by numbers and functional relations between variables. The conflict may therefore be formulated in terms of the relative adequacy of concrete as over against abstract symbolism.

Unfortunately the problem is none too precise when described in this way, for the dichotomy concrete-abstract is itself ambiguous. It may refer to any one of a large number of distinctions, all of which are considered as reducible to the distinction between the perceivable and the unperceivable. This is not a very helpful analysis for there are probably many reasons why any event may or may not be readily perceived. Events which are remote in space and time cannot be readily perceived, nor can events which are very large or very small, or very long or very brief. Events which are highly complex, i.e., which exhibit many aspects, cannot enter clearly into perception, nor can events which are highly simple, i.e., the aspects of complex events considered in isolation from one another. Thus one could construct such a list of unobservable events as the following: Mars, the birth of Caesar, the solar system, genes, life upon the earth, a flash of lightning, a human organism, and length without breadth or thickness—but in each case the elusive character of the event would be due to a different factor. A thorough examination of the problem at hand would require a detailed consideration of these various dimensions of obscurity.

For present purposes, however, the problem of the relative adequacy of icons and characterizing symbols may be left

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[1] *Ibid.*, p. 13.

in the form of generalities. It seems possible to maintain that the choice between these two types of symbol is determined primarily by one's ideational habits, and secondarily by one's conception of the function of symbolism in the development of science. Many investigators seem to think habitually in terms of images and models, and do not feel that any phenomenon is understood until it has been accurately pictured. Others seem to grasp abstractions more readily, and are apparently confused by the misleading detail which is associated with concrete imagery. The extreme types are represented by the artist and the mathematician; the former has developed a sensitivity to qualitative givens in perceptual configurations; the latter, to quantitative constructions in abstract logical patterns. But considerations of the ultimate task of science also enter in. If one believes that the final goal of science is the adequate representation of that portion of the realm of events which is clearly, i.e., perceptually, given he will be inclined to favor pictorial symbolism; for if an event can be readily perceived it can be readily imagined or modeled. But if he insists that the task of science is not mere descriptive portrayal but explanation, i.e., the revelation of less clearly given entities in terms of which the perceivable realm is to be understood, he will consider pictorial representation less useful; for if an event cannot be readily perceived it cannot be readily imagined or modeled. Hence one may say that the most adequate representation of the perceptual realm is through icons, and the most adequate representation of the non-perceptual realm is through characterizing symbols. This is not to deny the legitimacy either of characterizing the perceptual or of mirroring the non-perceptual. It is merely to recognize, for example, that a lecture about one's travels does not have the same informative value as a portrayal by means of a motion picture with sound effects. It is also to recognize the dangers inherent in the concrete representation of events which cannot be readily perceived. Events which are remote in space and time must be symbolized in terms of the here and now; events which are very large must be reduced, and events which are very small must be enlarged; events which endure must be shortened as to life, and events which are very brief must be prolonged; events which are highly complex must be schematized, and events which are highly simple must be associated with possibly irrelevant detail. Hence icons lose their essential pictorial value when used in such realms, since they subtract from, or add to, their referents in important ways. The result is that unless one is very cautious in the use of these symbols he is almost certain to read out of, or read into, the referents certain properties whose absence or presence is suggested by the character of the symbol. This becomes an important source of error in science.

Before dismissing the subject of symbols, attention must be called to a peculiar type of symbol which is apparently required for every symbolic scheme. This is usually called a "proper name" or an "index," and it is needed to relate an event given in direct awareness with a symbol which is presumed to be applicable to it. Both characterizing symbols and icons refer to their referents through properties, and one locates these referents by identifying the events possessing the specified properties. But the referents which are thus specified may not exist. An alternative means of identification is through gestures, or through the employment of certain words such as "this," "that," "here," "now," and certain other "pure" proper names. Events so located must exist, since proper names in the absence of their referents are meaningless and become mere noises. Hence a symbolic scheme which has an actual application, as over against one which has only an applicability, will always contain something which functions as a proper name. A map, for example, must always contain not merely a label, such as "map of Chicago," or "map of France," but it must contain a more specific reference such as "I am now here" indicated by pointing to a spot on the map. Only through some such means can the specific application of the symbolic scheme be determined.

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CHAPTER V

PERCEPTION

Now that the tools of science have been considered, the way is prepared for an examination of the methods and techniques by means of which science attains its end. The task of science, as has been shown, consists in the examination of the realm of events and in the discovery or invention of a system of symbols having as its main function the portrayal of the essential features of that realm. How does science set about this job?

THE STARTING-POINT OF SCIENCE

It may seem a commonplace to assert that science must begin with the realm of events. Events are fixed and objective, and constitute both the origin of knowledge and the point to which reference must be made in case verification is desired. Knowledge, on the other hand, is flexible and must be molded to suit the events which it is presumed to portray. If the scientist describes nature he must start with nature.

On closer examination, however, the statement proves to be not so obvious; in fact it may not even be true. Science starts not with nature, but with a selection from nature, and the particular selection is determined, presumably, by the idea in the mind of the scientist at the moment. The scientist does not respond to all events equally; on the contrary, he attends to those which are relevant either positively or negatively to a theory which he is debating and seeking to verify. In fact, since attention is a selective response, one may well ask whether there would be any data at all were it not for specific interests. When one walks down a busy street deeply immersed in thought, it is fair to say that the busy scenes do not exist for him; and if he does react he does so selectively, and, as a consequence, one may say that only those events to which he makes a conscious response really exist for him. Hence it seems that science does not begin with uninterpreted data, but with tentative ideas, conjectures, and hypotheses, verification of which is sought in the medley of given events.

Still closer examination of this point of view, however, reveals the fact that one has not yet arrived at fundamentals. Apparently data can be selected only through the medium of hypotheses. But do either data or hypotheses have any meaning apart from *problems?* Reflection has its origin, as was shown in Chapter IV, in what might be called problematic situations—situations in which the organism experiences an interruption in its habitual and accustomed behavior, and is obliged to try new responses in order to reëstablish the smooth functioning. In highly developed organisms these situations involve the use of deliberation, which is essentially the act of selecting from among a range of possible responses whose projected consequences are prefigured in imagination, one which is judged to be the most adequate in the situation at hand. So long as there are no problematic situations there is no reflection, and so long as there is no reflection there can be neither data nor hypotheses. Data and hypotheses are elements which are selected from the given as tools for solving problems, and until there are problems there is no need for tools.

The difficulty with this point of view, however, is that even problems are such only within the range of sensitivity of the organism. What is a problem for one organism may not be for another, and what is a problem for an organism at one time may not be at another. Hence what makes a situation into a problem seems to be something in the organism itself—on the one hand an organized system of responses which proves adequate to situations which for other organisms might be occasions for the breakdown of reactions, and on the other hand a sensitivity to certain types of situation which for other organisms might be occasions for uninterrupted behavior.

Fortunately these somewhat confusing features of the scientific method may be neglected for the purposes of the present study. Problems of this kind belong to the larger field of the psychology and the physiology of thinking, and no serious difficulties will arise if they are disregarded in accordance with the general principle of isolation. Certainly one can say that the issue as to whether a problematic situation or a general condition of sensitivity constitutes the prior element in reflection is pre-scientific rather than scientific. By the time the scientist reaches the stage in which he responds to the delicate situations which one calls scientific problems he has already developed a high sensitivity. It seems rather futile, therefore, to ask whether he is sensitive because he has recognized the problems, or whether he recognizes the problems because he is sensitive. Furthermore, the issue between data and hypotheses as prior elements cannot be settled without a knowledge of the stage of development of the science under consideration. A science which is still in its infancy emphasizes the collection and arranging of data, though even here one cannot say that these activities take place without regard for anterior meanings and interpretations. On the other hand, a science which has reached maturity adopts explanatory notions very readily and employs them as guides for the selection of data; however, here, again, one must insist that the hypotheses are not drawn from the blue but are suggested by the data. The only point to be emphasized with reference to this conflict is the fact that data and explanatory conceptions are not on precisely the same level in science. Theories depend upon data in a way in which data do not depend upon theories. Though one may say that the data are principles on the basis of which he may select theories, and theories are principles on the basis of which he may select data, nevertheless he must acknowledge that data are fixed while theories are flexible. Data may never be refuted, nor may they be modified to fit theories; theories, on the other hand, are often refuted, and are frequently modified to fit data. Theories, in other words, are the effect of data in a way in which data are not the effect of theories.

In view of this conclusion it does seem safe to say that science starts with facts, in the sense that it founds its structure ultimately on facts. "Any scientific idea arising in the mind of a scholar is based on a concrete experience, a discovery, an observation, or a fact of any kind, whether it is a physical or an astronomical measurement, a chemical or a biological observation, a discovery among the archives or the excavation of some valuable relic of an earlier civilization." [1] Whether the events which thus constitute the basis for science should be called "data" is, perhaps, questionable. Properly speaking, an event becomes a datum only when it is considered in the context of a problem, i e., only when it is responded to by an organism. An event which is isolated from the background of associated events by the selective response of an organism becomes a datum. Hence a fact cannot be a datum until it has been selected. But this may be an attempt to introduce an over-precise terminology. Certainly there can be no confusion in asserting that science rests upon data. The problem which then arises has to do with the methods for getting data.

THEORIES OF PERCEPTION

The act of getting data, at least in the natural sciences, is ordinarily called observation, or *perception.* Clearly, if knowledge is to be justified there must be some theory as to the nature of the perceptual act. Prominent among such theories is that of the ordinary man on the street. This may be called the common sense theory, though it represents a somewhat sophisticated rather than a purely naïve theory. In other words, it is the position taken by any observer who has recognized the fact of error, and who therefore realizes that objects cannot be perceived directly but must be known through the intermediary of something which makes error possible. The theory is not at all clear

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[1] M. Planck, *Philosophy of Physics* (New York: Norton, 1936), p. 89.

in its formulation. In fact one may say that when the common sense theory becomes formulated it has already become critical and has ceased to be the position of the ordinary man. The purpose in examining it here is to draw conclusions as to the possibility of making it a foundation for scientific procedure. Science is often described as continuous with common sense and as differing from it only in the degree of criticism which is introduced. This suggests that some light may possibly be thrown on science by examining common sense.

The theory is usually described as representative in character. It maintains that what is known through perception is a realm of material objects existing in space and time, and possessing certain properties such as shape, size, rest or motion, color, sound, hardness, and on certain occasions, taste, and odor. Sense perception is a process of receiving from the material objects physical stimuli which enter the sense organs and are transmitted through the nervous system to the brain and thence into the mind. Knowledge is a complex of immaterial entities called ideas, located somehow in the mind, which is itself vaguely associated with the brain and therefore presumed to be in the skull. The ideas are consequently not in space or time in any precise sense and they do not exhibit any of the properties of physical objects such as hardness, rest, motion, shape, or color. But they have the peculiar power of mirroring the properties of physical objects, and are thus instruments of knowledge. When they correspond to objects they are true and constitute knowledge; when they fail to correspond they are false and constitute error. The knower functions as the recipient of the stimuli and as the residence of the ideas.

It requires no careful analysis to justify the conclusion that such a theory of cognition can hardly be adequate as an interpretation of science. Philosophy has long been wrestling with this problem and has brought certain of the difficulties inherent in it clearly to light. Two of its inadequacies may be suggested here. In the first place, no satisfactory test of truth is possible upon its grounds. If one knows the material world only through immaterial ideas and if the truth of an idea consists in its correspondence with an object, then truth can never be ascertained. One can get ideas but one cannot get material objects, hence no comparison can be made. One cannot compare his image of a piece of gold with the piece of gold itself, for he can know the latter only through the intermediary of an image; hence he finds that he can only compare one image with another. The result is that he is isolated within the realm of ideas and can never know an external world. The second inadequacy of the common sense theory is its failure to find a place for thought creations, fictions, constructions, abstractions, and the like, in the cognitive situation. From the point of view of science this is a serious limitation. Without idealizations, isolated systems, serial extensions beyond and interpolations between the elements of the given, science would find it impossible to proceed. In other words, science finds it useful to convey information about something which does exist by talking about something which does not exist. According to the common sense theory this would be impossible; all ideas of things which do not exist would be false and could not contribute to knowledge. Common sense denies any creative activity to the knower, since any transformation which is due to his activity could only result in error. The perceiver is merely the passive recipient of stimuli, and his efficiency is to be measured in terms of his receptivity.

As a result of these inadequacies various attempts have been made in the history of thought to replace the common sense theory of perception by one which allows for the possibility of science. A detailed discussion of these theories would be out of place in an introductory book of this character. Two of them may be briefly considered here, viz., *positivism*[1] and *subjectivism.*

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[1] Positivism is both a theory of perception and a theory of description. It will be considered only in the former sense here. Positivism as a theory of description will be discussed in Chapter VIII.

Positivism insists that the fundamental error in the common sense interpretation lies in the supposition that all that any observer knows is ideas, private to his own mind. One knows neither ideas nor objects, says positivism, but neutral entities which are in themselves neither mental nor physical. According to Ernst Mach, one of the most significant representatives of the position, the stuff of knowledge consists of such neutral entities—"colors, sounds, temperatures, pressures, spaces, times, . . . feelings and volitions."[1] These elements are not psychical in character, nor private to the mind of the observer. Nor are they physical, residing in a public world. Strictly speaking, they are in themselves neither the one nor the other. However, they have the capacity to occur in certain types of complexes and in certain functional relationships to one another. "A color is a physical object as soon as we consider its dependence, for instance, upon its luminous source, upon other colors, upon temperature, upon spaces, and so forth. When we consider, however, its dependence upon the retina it is a psychological object, a sensation. Not the subject-matter but the direction of our investigation, is different in the two domains."[2] "These elements—elements in the sense that no further resolution has as yet been made of them—are the simplest materials out of which the physical, and also the psychological, world is built up."[3] Hence knowledge cannot be an affair involving correspondence between a psychical entity and a physical entity, for there is no such difference of stuff. An element in one context is psychical, and in another is physical; but it is the *same* element in both cases.

Such a theory claims to avoid the unfortunate dualism of common sense. When a color is known, there is not the idea of the color which is known directly, and the physical color itself which is outside of the mind and known only indirectly. The color and the idea of the color are identical.

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[1] *Analysis of Sensations* (Chicago: Open Court, 1914), p. 2.

[2] *Ibid.*, p. 17.

[3] *Ibid.*, p. 42.

There is only one thing—a neutral color, which is known directly. The fact that the color occurs in varied and complex relations to other "elements" does not have any immediate effect on the knowing, and does not change the character of the color. Colors are acted upon by light sources and various media, and the laws of such operations are called physical laws. But colors are also acted upon by the retina and optical nerve, and the laws of such operations are called psychological laws. According to such a theory one is no longer entitled to say that physical objects produce ideas. One ought rather to say that certain elements which are neither physical nor ideational occur in associations which are sometimes physical and sometimes ideational. The idea is different from the physical object only in the sense that a man in the associations of his home life is different from the man in the associations of his business. There need be no problem of getting from one to the other, for there is an identical element—the man himself. For positivism, therefore, the dualism of common sense has been avoided, and the problem of perception has been solved.

Positivism, however, represents an unstable position. The neutral entities will not remain persistently neutral, but tend to take on a private, subjective character. Unless positivism is continually on its guard it passes imperceptibly into a solipsistic idealism. This leads to the consideration of a third theory of perception, viz., *subjectivism.* One of the best expressions of this subjectivistic theory of perception is to be found in Eddington, at least in his later phase.[1] His position claims to be founded on the actual procedure of science, and is presumed to be the only interpretation of observation which is in accord with the facts both of science and of common sense experience. "Mind is the first and most direct thing in our experience; all else is remote inference." [2] The first task of science consists in the descrip-

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[1] Eddington's position becomes more solipsistic as he passes from *The Nature of the Physical World* (1929) to *New Pathways in Science* (1935). His point of view as developed in the earlier of these two works will be discussed in Chapter XX.

[2] *New Pathways in Science* (New York: Macmillan, 1935), p. 5.

tion of "regularities and recurrents" which are noticeable in this immediately given content. "We call these regularities of experience laws of Nature."[1] But science does not remain in this limited realm. "Broadly speaking the task of physical science is to infer knowledge of external objects from a set of signals passing along our nerves."[2] This proves to be necessary because "it is the inexorable law of our acquaintance with the external world that that which is presented for knowing becomes transformed in the process of knowing."[3] From this, the character of the knowing situation is clearly ascertainable. So far as immediate and certain knowledge is concerned, we are limited to our own individual sense experiences; we can classify them and formulate the laws of their interrelationships. But on the basis of this knowledge we may make—indeed, we *must* make—inferences to an "external" world, since we apparently know that these sense experiences are the results of acts of transformation and cannot therefore afford reliable information as to their causes. Our knowledge of an external world is wholly inferential in character.

If one examines the three theories of perception here considered, viz., the common sense theory, positivism, and subjectivism, he can recognize that each represents, in a sense, an attempt to overcome the inadequacies of the preceding. The common sense theory is unsuccessful because of two features: it makes all knowledge indirect and thus denies the possibility of knowing an external world, and it forbids the mind by its own creative acts to make any contributions to knowledge. Positivism apparently recognizes at least the first of these difficulties and attempts to avoid it by supposing that knowledge is *always* direct. There is reason to believe that this is an improvement on the common sense position. But a new difficulty arises. If knowledge is always direct, how is error possible? If there is nothing which comes between the observer and the object, and if observation is the passive response to the

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[1] *Ibid.*, p. 8.

[2] *Ibid.*, p. 6.

[3] *Ibid.*, p. 7.

object, how can the observer ever be mistaken? The answer is that he cannot be. As a consequence positivism must give erroneous objects the same status as neutral entities; the world then becomes populated with all kinds of fanciful and imaginative objects. Subjectivism feels that it avoids this difficulty, for it locates the things which are known directly in the mind. This seems to place unreal objects where they properly belong. But where does it place *real* objects? It gives them an inferred status beyond the contents of the individual consciousness. The world of science is precisely the result of such inference. By means of direct knowledge and an inferential act one may obtain indirect knowledge of something real and external. But a serious difficulty immediately arises—a difficulty which seems to be inherent in the subjectivistic position. If one can know directly only the contents of his own consciousness, how can he ever know the *principles* which will guide and justify an inference leading outside that consciousness? According to Eddington the justification for such an inference is to be found in the laws of nature. But, as has already been seen, the laws of nature are simply the regularities and recurrencies which are noticeable among the data of the individual consciousness. Hence the individual must be presumed to be able, by means of a law which is private and subjective, to infer the existence of something which is public and objective. This is clearly an attempt on the part of the individual to lift himself by his own bootstraps. It proves to be, therefore, a very unsatisfactory technique for locating real objects. A pure subjectivism is thus no better off than a pure positivism. Whereas the latter has difficulties in locating *unreal* objects, the former is unable to locate *real* objects. Positivism seems to give unreal objects a substantiality which does not belong to them, and subjectivism seems to deny real objects a substantiality which does belong to them.

From this examination of the problem of perception, one seems obliged to draw certain conclusions. These, it will be noted, are identical to those expressed in Chapter III where cognition in general rather than perception was the topic of discussion. It was found there, as it is now found here, that knowledge is impossible unless there is (1) something which may be called *direct* awareness, and (2) something which may be called *indirect* awareness. The former is required to make *truth* possible, and the latter to make *error* possible. But one is now able to see more clearly what this indirect or symbolic awareness is. *The symbolic function of mind is identical with its inferential function.* Symbols are the essential instruments by means of which one extends the immediately given. Hence symbolic awareness is an inferential awareness of something not immediately given in direct awareness, but hinted at or suggested with a greater or less degree of clarity. Many of my "private" sense-data, which I know immediately, indicate "public" objects, and it is through the instrumentality of symbols that I become aware of such objects. The objects enter into my knowledge through symbols, hence the symbols occupy an intermediary position between the "private" sense-data which are given immediately, and the "public" objects which these sense-data seem to demand.

SCIENTIFIC REALISM

The correct understanding of the part which symbols play in this general situation enables one to construct a theory of perception which seems to be more adequate than any of the three considered above. It may be called *scientific realism.* Its fundamental claim is that the elements of perception should be defined in terms of certain neutral particulars, as in the case of positivism. But it argues that the structure of perception is expressible only in terms of complex relations between certain clusters of these neutral elements. Hence its first problem is to show how these elements become associated into groups which are called objects of certain kinds.

Very early in the individual's experience he must have noticed that elements such as colors, shapes, tastes, odors, tensions, feelings, decisions, tendencies, and the like unite into clusters having a certain permanency of existence and of recurrence. Knowledge reveals not merely the fact of such elements as these, but also the fact of their togetherness in certain associative wholes. The unitary character of such wholes is determined not merely by the fact of persistence and recurrence but also by the fact that changes tend to occur in clusters, i.e., changes in certain of the elements tend always to be associated with changes in other elements. By the observation of these persisting and recurring associations, undergoing collective changes, the individual selects certain wholes to which he attaches an individuality. These complexes he calls "objects," though it must be recognized that such a term tends to suggest an opposition to "subjects" and "subjective"—an implication which is misleading at this point. Certain complexes exhibiting shape, color, size, spatial and temporal location, hardness, rest or motion, and the like are called "physical objects," changes in such objects being illustrated by breaking, dissolving, and burning. Certain complexes of this kind are also called "physical media," since they have the peculiar power of affecting other physical objects in certain describable ways; for example, a glass of water is such a medium since it causes a straight stick to appear bent when immersed in it; also a microscope is such a medium since it causes a small object to appear large when placed under it. Certain other clusters are called "biological objects," one of which is the individual's own organism; this also has the power to induce changes in physical objects, as in the case of an object which appears double when the individual is under the influence of drugs or subjecting his eyeball to pressure. Another cluster the individual calls "mind" or "consciousness"; this again has the power to induce changes in physical objects; for example, an object may be changed by suggestion or by alteration in emotional attitude. Finally there is a cluster, relatively simple in character, which is perhaps part of the previous complex, and may be called "awareness"; this is also capable of affecting objects, since when it is absent objects disappear completely. This is the *direct acquaintance* whose essentiality in knowledge was demonstrated in Chapter III.

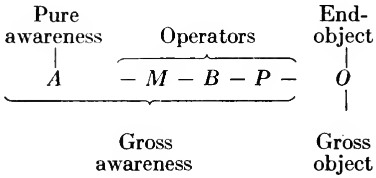
These types of object unite into perception in a way which may be symbolized as $A - M - B - P - O$. O represents something which may be called the end-object; it is that which is presumed in any perceptual act to be the ultimate thing known at the moment, i.e., the *content* of the awareness. Attention has already been called to the fact that all knowledge demands a situation of the kind described by saying, "I am now aware of such-and-such." The end-object is the final "such-and-such" of perception, i.e., it is that element of the perceptual act which cannot in the given situation be considered as a tool or instrument of perception, and hence as a part of the awareness rather than of the content. An end-object is that which one perceives, not that by means of which something is perceived. P represents that complex of elements which constitute the physical medium in which O is found. This includes its spatial and temporal location, its illumination, the atmosphere or other gas surrounding it, and so on; it also includes any instruments placed in that medium for the purpose of recording, measuring, magnifying, analyzing, intensifying, and otherwise conveying information, through physical stimuli, of the character of the end-object. Objects which function as physical media may also function as end-objects, though not usually in the same situation; for example, one may make a physical study of a thermometer. But if a thermometer is end-object of an act of perception, it is not ordinarily at the same time a medium for conveying information about some other event. An end-object indicates, at the moment, nothing beyond itself; a medium is always a vehicle. B represents that complex of elements which constitute the body of the perceiver, i.e., the nervous medium through which the stimulus from the end-object, or from the medium, must pass before it is capable of entering into awareness. It functions exactly as does the physical medium. The human organism may itself function as an end-object, though not when it is also functioning as a medium—unless one is prepared for the difficulties to which this gives rise. *M* signifies that complex of "mental" elements—conceptual background, emotional set, purpose, and interest—which constitute a third medium through which the stimulus must pass. This, again, functions exactly as do the physical and bodily media. *A* indicates the pure awareness by which an object becomes a *perceived* object. It is non-transforming in character, and therefore neither contributes to nor subtracts from the object known; it is "external" to the object. Even here one may say that awareness may become an end-object, though the occasions in which it does so are rare; the present situation is presumably such a case, since we claim to have discovered awareness as an element of perception. But if awareness is an end-object, there must be another awareness which must be of it, for one cannot get away from the fact that the awareness of a content is the minimum situation in which knowledge can exist.

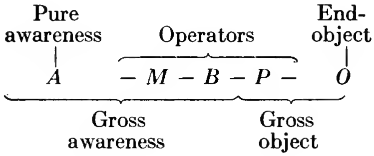
The most important feature which emerges as a result of this analysis is that the five factors, *A*, *M*, *B*, *P*, and *O* are not, so to speak, on the same level. Since every perceptual act must contain an awareness and an end-object, *A* and *O* function uniquely; *A* functions as the minimum awareness, and *O* as the minimum object, i.e., *A* can never be part of the end-object (without assuming another pure awareness), and *O* can never be part of the pure awareness (without assuming another end-object). The three factors, *M*, *B*, and *P*, on the other hand, are parts neither of the pure awareness nor of the end-object, but seem to lie between the two. They are not that which is perceived, nor are they the perceiving, in the strict sense of this word; they seem to function as the conveyors of stimuli from the end-object to the place where it may enter into awareness, and as devices of inference by which one may gain information as to the character of the end-objects. In this function they may be called *operators*, to distinguish them from A and O. Then the perceptual situation may be symbolized as follows:

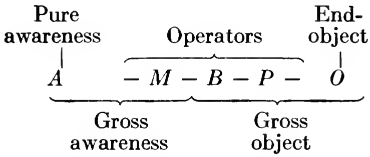
$$\overset{\substack{\text{Pure}\\ \text{awareness}\\ |}}{A} \qquad \overbrace{-\,M-B-P\,-}^{\text{Operators}} \qquad \overset{\substack{\text{End-}\\ \text{object}\\ |}}{O}$$

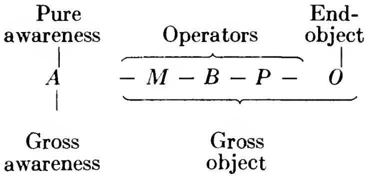
But to describe the operators as coming between A and O is somewhat confusing and may lead to all of the difficulties of the common sense theory unless one specifies clearly just what is meant by this. The operators, M, B, and P, are more properly described by saying that they are those elements of perception which may be considered either as parts of the gross object or as parts of the gross awareness. In the former case they function in the determination of the *appearances* of the end-object in the various contexts; in the latter they function in the determination of the *complexity* of awareness. Nothing can come between awareness and its object, but there is no reason why both awareness and object should not themselves be complex. By the object of awareness one may mean only the end-object. But one may mean on other occasions the end-object as operated upon by the physical medium, or as operated upon by the physical medium plus the sense organs and nervous system, or as operated upon by the physical medium plus the sense organs and nervous system plus the mind. In each case the entity of which one is aware is the end-object as it manifests itself through the influences of various operators. It may be called the appearance of the object in the described context, and awareness may be said to be of the appearance of the object. This retains the directness of perception, provided the appearances of an object are presumed to be part of the object. But what is to be done with the operators in case one wishes to speak merely of the end-object? The operators are unavoidably present and in such a case would appear to come between the awareness and the object. But this can be avoided by supposing that when the operators are not part of the object they are part of the awareness. Any loss in the object must be equalized by a gain in the awareness, and vice versa. If the object of awareness is reduced to a bare minimum the awareness must be increased to include all of the operators; one is then still directly aware of an object, though the mode of his awareness is highly complex. On the other hand, if the awareness is reduced to a bare minimum the object must be increased in such a way as to include all of the operators; one is then still directly aware of an object, though the object of his awareness is highly complex.

What should be emphasized here is the fact that the place where the line is drawn between awareness and object is arbitrary. Whether physical instruments, sense organs, and conceptual background are considered to be part of the gross object or part of the gross awareness makes no essential difference so long as one recognizes the inverse ratio holding between the complexity of the object and the complexity of the awareness. What is and what is not to be taken as "part" of an object is a relative matter; properties of events are functions not only of the "events themselves" but also of the total situations in which they participate; these situations may include not only extended spatial and temporal volumes, but physical instruments, human organisms, and minds contained therein. This recognition is important, for it means that events are, in a sense, everywhere. But the possibility of an alternative interpretation somewhat destroys the uniqueness of this fact. One must admit also that what is and what is not to be taken as "part" of the awareness is a relative matter; the perceiver functions not only "where he is" but wherever an object perceived by him exists. This means that the perceiver is, in a sense, everywhere. As a result, there are alternative representations of the perceptual situation:









But it remains true that in any specific situation the object is only where its end-object is, though its causal influences may be widespread; and the perceiver is only where his body is, though he may make inferences as to other localities. The end-object can pass from its locus to that of the perceiver only through causal transmission, and the perceiver can get from his locus to that of the end-object only through inferential acts. This enables one to see the precise sense in which the operators come "between" the end-object and the perceiver, and the precise sense in which a perceiver can become aware of an end-object. Clearly, the perceiver can be *inferentially* aware of the end-object, i.e., he can be directly aware of the M-appearance of O, and from this and a knowledge of the nature of the M-, B-, and P-operators he can infer the character of the end-object. But his knowledge of O can not be *solely* inferential, for he could then never check the validity of his inference. In order to do this he must, at least in certain assumed situations, know O directly. Similarly, in order to know the ways in which the M-, B-, and P-operators function, he must have a knowledge of them which is other than inferential. Hence, he must, at least in certain assumed situations, know M, B, and P directly. The character of these assumed situations has to do with the notion of normal operators, and will be discussed presently. Here all that need be insisted upon is the double function of the operators. They may be considered, indifferently, either as parts of the object, justifying inference to other parts, or as parts of the awareness, conveying direct information about end-objects.

The variability in the position of the "screen" separating the awareness and the object can be made clear by an example. Suppose one is attempting to determine the temperature of a gas by immersing a thermometer in it. He inserts the thermometer, waits a few moments, and reads off the number coincident with the height of the mercury column. He then says that he is aware of the heat of the gas. But this is not quite accurate. What he is directly aware of is only the reading of the instrument; properly speaking, he should say that from this reading he *infers* the heat of the gas. But he soon realizes that there may be distortions in what he judges to be the reading of the instrument, since he is obliged to observe this through the intervening air and relative to his own spatial position. Hence, he says that he is directly aware only of the coincidence as it appears very close to his eye and with all distorting influences removed. By these two corrections the perceiver has moved the "screen" separating the awareness and the object from the surface of the end-object to the surface of the physical medium. He is directly aware of the appearance of the recording instrument, and from this he infers the character of the end-object. However, he soon realizes that even this is not quite accurate. He knows that his sense organs often transform the stimuli which they receive; if his eyes are not good he may misread the instrument. Hence, he says that he is directly aware only of the coincidence as seen by his eyes, and under the influence of his whole nervous mechanism as it exists at the particular time of perception. He then corrects his judgment by saying that on the basis of his own visual interpretation of the reading of the instrument he infers the actual reading and from this the heat of the gas. The "screen" has again been moved and now is located between the *M*- and *B*-operators; the end-object is now "appearing" in a still more inclusive object. But even here a further correction is possible. He recognizes that what he observes in any situation is often influenced by unconscious associations, emotional attitudes, and expectations; he often "sees" what he wants to see or what he has been prepared to see. Hence he says that he is directly aware only of the coincidence as he interprets it at the moment; from which he infers the way in which he must actually have seen it; from which, again, he infers what the reading must actually have been; from which, finally, he infers the heat of the gas. At this stage he is obliged to correct his judgment by saying that there is, at any rate, *something* of which he is directly aware at the moment, and that if he allows for mental, bodily, and physical transformations he can infer the character of the end-object. The "screen" which separates the object and the awareness has again been moved, for the end-object is now considered in its mental context, and the awareness has been reduced to a bare awareness of something given.

It should now be clear why attention was called at the close of the last section to the need for admitting into knowledge some type of awareness other than the direct sort. Whether this additional aspect of knowledge which is inferential rather than direct in character is properly a part of perception, or something over and above perception, is debatable. In a sense, reference to this aspect of knowledge is somewhat premature at this point. Not until the end of the next chapter will consideration be given to the symbolizing of data obtained through perception, and the making of inferences represents a still more advanced stage of knowledge. But since the proper understanding of perception itself demands reference to the possible inferences which may be made from it, consideration of this feature will be helpful at this point. If truth is to be possible there must be direct awareness of objects. But if error is to be possible there must be knowledge of an inferential sort. Inference functions in knowledge as the technique by which from the appearance of an end-object in a certain context its appearance in another context, or the end-object itself, may be ascertained. Error arises when one infers incorrectly from the appearance of an end-object in one context to its appearance in another, or to the end-object itself. Awareness is always directly of objects, and cannot be erroneous; if awareness and the object vary inversely awareness may be complex and the object simple, or the object may be complex and the awareness simple—yet awareness is always of the object. But at any given moment what may be desired is knowledge of the object in some context other than that in which it is given for direct awareness. This additional knowledge must be inferential. Perception permits one to have direct awareness of an object in one context, on the basis of which he constructs inferential knowledge of the object in some other context, or of the end-object itself. One of the important tasks of science is the justification of precisely such inferences.

Clearly, this inferential act is of importance in the understanding of perception, and certain of its features must be called to attention. In the first place it must be based, as Eddington suggests, on the laws of nature. Inference beyond the object which is given in direct awareness becomes possible only if one knows the laws according to which the end-object manifests itself in the physical, bodily, and mental media. One must know the laws of operators. One can infer from the appearance of an object in a certain physical medium to its character apart from such a medium only if he knows the laws according to which radiations from objects are transmitted by media. He can infer from a pointer reading to the character of the object measured only if he knows the principles according to which the measuring instrument was constructed and calibrated. He can infer from a sensation to the character of the object sensed only if he knows something of the nature of nervous impulses. Finally, he can infer from data as interpreted by mental factors to the data as uninterpreted only if he knows the principles according to which mental operators work. The totality of such laws should make one acquainted with facts like the following: the distinction between normal and abnormal functioning of the operators; the range of sensitivity, delicacy, extent, etc., of the physical media (instruments), sense organs (considered as recording instruments), and states of mind; the nature of the transformation which any stimulus undergoes as a result of the functioning of the operator, and hence what may be lost from or added to the object; and so on. It is the task of physics, biology, and psychology to reveal laws of this kind. On the basis of them inference may be justified, for the principles by which one infers the character of an object in a less extensive context from its character in a more extensive context are the reverse of the principles according to which the object in its less extensive context causes its appearance in the more extensive context.

But can one learn the laws of operators? Apparently not, due to what has been commonly called the ego-centric predicament. Since one cannot get away from his own sense organs he can be aware of objects in the physical context only as they appear in the bodily context. Furthermore, since one cannot get away from his own mental state he can be aware of objects in the bodily context only as they appear in the mental context. In order to know how *B*-operators act one would be obliged to get an object in the *P*-context, first without the intermediary of B-operators, and second through B-operators; then by a comparison of the two he could determine the laws according to which B-operators work. But this is just what he cannot do, since he cannot get outside of his own organism. And if he wished to determine the way in which M-operators act he would be obliged to get an object in the B-context, first without the intermediary of M-operators, and second through M-operators; then by a comparison of the two he could determine how M-operators work. This, again, he cannot do, since he cannot get outside of his own mind. Hence, since one cannot learn the laws of the operators he cannot use such laws as a foundation for inferring from objects in one context to objects in another.

Though this difficulty cannot, perhaps, be avoided theoretically, it can be and is avoided practically. This is done through the notion of *normal* operators. One decides, more or less arbitrarily, that a certain type of situation may be defined as a normal observational situation. Psychologically, such a situation would be defined as one in which the observer is attentive, deliberate, free from prejudices and preconceptions, and without emotional biases. A situation of this kind can be empirically distinguished from one in which the designated attributes are lacking. Biologically, a normal observational situation would be one in which the observer is in possession of sense organs which have met certain specified tests. These tests are, of course, comparative in character and assure one only that the individual in question, as indicated by the verbal responses which he makes when he is presented with varying objects, can make distinctions where the majority of individuals make distinctions, and fails to find differences where the majority of individuals fail to find them. Physically, a normal observational situation would be one in which the object is at the proper distance from the sense organ, in which there are no intervening recording instruments, and in which the unavoidable media such as light, atmosphere, and electro-magnetic and gravitational phenomena are present in ascertainable amounts and kinds. Then a normal observational situation would be one which is psychologically, biologically, and physically normal.

The possibility of determining laws of operators is then based upon a practical assumption: *in normal observation the transforming effect of the operators may be neglected.* This is equivalent to saying that in this type of situation the operators are considered as non-transforming. On this assumption one is able to become "directly" aware of end-objects, pointer readings, and sense-data. He may therefore compare end-objects with pointer readings and by this means determine the laws of *P*-operators; he may compare pointer readings with sense-data and thus determine the laws of *B*-operators; he may compare sense-data with "interpretations" and, as a result, formulate the laws of *M*-operators. He then sees that his assumption with reference to the non-transforming power of operators represents a first approximation, and he corrects it by means of the laws already established. This then becomes a second approximation, and he corrects the laws of the individual operators accordingly. By this means he reaches a progressively more accurate and reliable knowledge of the laws of nature, and feels himself justified in using them as principles of inference.

The second feature of inference which must be called to attention has to do with its relation to symbols. Inference functions in observation in affording a technique on the basis of which from a knowledge of an object in a certain context one may determine its character in another. However, although the appearance of which one is directly aware is the basis for an inference to the character of the object in a different context, the appearance should not properly be called the *symbol* of the inferred object. The greatest confusion is introduced into Eddington's position by this terminology. For example, he fails to make a distinction between pointer readings—which are clearly events in nature—and mathematical symbols—which, although they are events in nature in the sense that they are physical counters, are more properly described as symbols for understanding nature. There are superficial resemblances between the two types of symbol. One might say, for example, that red sky in the evening and the words "fair weather tomorrow" are both symbols of tomorrow's weather; but they are not so in the same sense. It is preferable to say that the red sky is a *sign* of the ensuing weather, but the words "fair weather tomorrow" are *symbols* of it. This is in conformity with our earlier use of the word "symbol" in its limited sense. Granting this distinction, one should say not that the appearance of the object in one context is a symbol of it in another context, but that the former calls to mind a symbol—word, image, or other symbol—which is then taken as having reference to the object in the other context. This suggests the intimate association between the symbolic and the inferential functions of thought. When one infers an object he anticipates it by means of a symbol; the symbol is the substitute for the object inferred, and acts in its stead until the latter can be itself observed. But when one anticipates an object through inference, he should not confuse the object which is the origin of the inference with the symbol through which the inferred object is grasped. On the basis of such things as pointer readings, sense-data, and interpretations, together with the laws of *P*-, *B*-, and *M*-operators, one infers the character of objects in other contexts; these objects, since they do not enter into direct awareness at the moment, must exist for thought merely as symbols. Pointer readings and the like are not themselves symbols but foundations for inferring to symbols which designate absent objects. From this point of view Eddington's terminology is confusing.

The results of this chapter may be briefly summarized. Science begins with data, at least in the sense that every theory arises out of data and receives its verification in data. The act of getting data is perception. Attempts to explain perception by means of the common sense theory, by means of Machian positivism, and by means of subjectivism prove unsuccessful. An alternative theory, called scientific realism, analyzes perception into three kinds of elements: pure awareness, operators, and end-object. Operators do not come between awareness and its object, but constitute a part either of the gross awareness or of the gross object; hence awareness is always in "contact" with its object. The task of science is to ascertain from the appearance of the end-object in direct awareness the character of the end-object or of its appearance in another context. This is done by inference, based upon the laws of operators. Perception may be defined either as the direct awareness of an object, or as this direct awareness plus the inferential act by which something of the character of the object in another context may be determined. This is approximately the structure of perception. The problem, now, is to determine how it functions in science in the actual collection and interpretation of data. This will be the topic of the next chapter.

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CHAPTER VI

DESCRIPTIVE TECHNIQUES

The present chapter is essentially a continuation of the preceding one. The problem under consideration is that of getting the data. At this point, however, the emphasis changes slightly. Interest is no longer directed so much to the understanding of the *passive receptivity* of the observer, i.e., his capacity to receive messages from external objects, as to his *activity*, i.e., what the scientist does to himself and to nature in order to make it yield its secrets.

Attention should be called immediately to the fact that there is no contradiction between these two aspects of the scientific method. It seems obvious, on the one hand, as has already been indicated, that man must proceed by listening to nature. Nature is a given, stubborn, and brute fact, and one must take it as it is. Fortunately, it chooses on somewhat rare occasions to speak about itself—often haltingly, and usually enigmatically. But one must be continually on the watch, lest through his inattentiveness he lose one of those golden opportunities when it discloses its secrets. At best, therefore, one can only wait.

But it seems equally obvious, on the other hand, that nature often responds to prodding. Through properly administered techniques it may be forced to speak. Nature is not only what takes place, but what may be made to take place. By putting one's finger into nature one often succeeds in bringing about situations which would occur rarely or not at all without his intervention. Thus by combining, separating, moving, increasing, diminishing, heating, cooling, and a thousand other operations he incites nature to manifest itself in unaccustomed forms. He suspects nature to be more than it obviously is, and he finds the variety of its responses to be capable of almost indefinite increase. He soon recognizes that the task of science is not merely getting the data, but increasing the variety and scope of data by definable techniques. It is what man does to nature that counts, for what nature tells him is a function of his own exploratory operations.

The descriptive techniques of modern science are many and various. A detailed discussion of them would belong properly in a laboratory manual rather than in a book on scientific methodology. The details of the laboratory procedure will not, of course, be discussed here. The problem is rather to consider these techniques only in their most general features, and in so far as they have bearing on a logic of science.

For the purposes of rough classification one may divide descriptive techniques on the basis of the contexts within which they have major application. If the function of the descriptive methods is to increase the range and variety of data without introducing "abnormal" appearances, a discussion of such methods may conveniently follow the analysis of the perceptual situation into its elements. Accordingly, descriptive techniques may be considered under the following heads: (1) control of the end-object; (2) control of the physical medium; (3) control of the sense organs; and (4) control of the state of mind.

CONTROL OF END-OBJECT

A strictly descriptive science would be passive toward nature. It would neither manipulate nor control, neither move about nor transform objects. For on the presuppositions of a purely positivistic approach to nature, the world must be only what it *obviously* is; nothing conjectural or doubtful can be included in science. But there is no science which is purely and simply descriptive. The implication of the study of nature is that objects are not only what they are apart from man's intervention, but what they may be made to become through his intervention. *Objects are the totality of their manifestations.* Hence anything which one can do to an object to make it otherwise conveys important information about that object, for the object becomes the potentiality of precisely that transformation. Sodium is not only metallic in appearance, but readily combustible; common salt is not only white in color, but soluble in water; gold is not only yellow, but highly malleable, etc. In each of these cases important information about the substance is conveyed by describing the transformations through which it passes when subjected to certain operations.

In the present section mention will be made only of those operations which may be said to bring about *changes* in the end-object. It is obvious that the distinction between operations which transform, and operations which do not, is relative and practical. Modifications in the physical medium are not ordinarily presumed to affect the end-object, though they do, of course, affect the appearance of the end-object in the physical medium. The insertion of a microscope, for example, between one's eye and a bit of cell tissue does not change the tissue in any appreciable way, though it does change the appearance of the tissue to the observer. So, in general, it is possible to multiply the manifestations of an end-object through the introduction of measuring and recording devices without "doing anything" to the end-object itself. The relative character of the distinction here maintained can be seen in the fact that the observation of an object may involve such a transformation without one's being aware of it. "In the street white flakes are falling steadily. I stretch out my warm hand to examine them, and discover that they are merely drops of water. Allowed to fall on a sheet of cold metal, they are small flakes of snow. A sheet of ice is stretched out and the particles that fall are frozen. A red-hot plate shows them up as hot spheres of liquid, while a fire as puffs of steam. What, we ask, is this mysterious and fickle entity, that may show itself as ice, snow, water, or steam according to how it is examined? The answer to the riddle is, of course, obvious in this case. These instruments for examining the white flakes have not been neutral."[1] Similar difficulties of a more significant character are revealed by the Heisenberg principle of indeterminacy. In the case of the observation of small particles the method of observation, e.g., the use of a light wave, may influence their states in such a way that one cannot assign to them with a high degree of precision both location and velocity. It seems likely that as science advances there may be disclosed an increasing dependence of the end-object upon all kinds of environmental changes, including the introduction of instruments and other recording devices.

A complete listing of the operations which may be performed upon objects would be impossible. Characteristic among them are analyzing and synthesizing, speeding and slowing, intensifying and diminishing, melting, burning, solidifying, dissolving, boiling, condensing, pulverizing, reshaping, magnetizing, stretching, compressing, dyeing, decomposing, and ionizing. All of these may be said, in some sense, to introduce changes in the object—changes which are in some cases reversible and in some cases not. In each case the aim is to bring about a new manifestation or appearance of the object, but by doing something to the object itself rather than merely to the physical medium.

An interesting feature of this type of technique is that it tells the observer not what objects *are* but what they *were*. It defines objects not in terms of their actual characters but in terms of their potentialities. It identifies objects only by destroying them in the very act of identification. Though such a method does give the observer information about objects, this knowledge is of the kind which Eddington[2] calls "retrospective inference." The presumption in each case is that the objects in question are defined not merely in terms of retrospective inference but exhibit some properties which can be directly observed, and hence permit knowledge of the present rather than merely of the past. Clearly if one's knowledge of objects were obtained *only*

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[1] H. Levy, *Universe of Science*, p. 66.

[2] *New Pathways in Science*, p. 93.

by retrospective inference he could never identify objects without destroying them, and they would reduce to mere bundles of potentialities. The very fact that the observer speaks of these operations as *changes* indicates that he is able to become independently aware of the objects prior to the changes. Practically, one avoids difficulties of this kind by employing as materials upon which operations are performed not all of the objects of a given kind, but merely *samples* from them. In destroying the samples, therefore, he still has left objects of the kind which have been destroyed, and which can be characterized in terms of their capacities to behave in this particular way when subjected to this particular kind of operation.

The manipulation of objects in this way constitutes what is ordinarily called *experimentation.* But a rather sharp distinction may be made between experiments for *discovery* and experiments for *verification.* The type which is here being considered belongs in the former category, but the more usual sort belongs in the latter. Experimentation for discovery belongs among the descriptive techniques, and is analogous to the more or less random toying, pinching, turning, and tasting activities which the child uses to explore a new object. The operations are performed without specific direction or control, and without any well-defined idea as to what they may reveal. They are thus activities in which one discovers something which he had not previously known about the object. Verificatory experiments, on the other hand, as will be seen in Chapter XI, although they involve discovery, are definitely planned in advance, and involve a more or less precise prediction that a certain outcome will ensue. The latter presuppose that one has already learned enough about nature to anticipate certain other of its features; the former presuppose nothing but the general capacity of nature to reveal itself under different guises.

There is some doubt as to whether changes in the *location* of an object should be included among the controls of the end-object or among the controls of the physical medium.

Is an object changed by being moved about? On the one hand, the presumption is that a simple change of location—either of space or of time—does not modify an object in an appreciable manner; *where* or *when* an object exists is not an important fact about it. Though such a change would bring about an alteration in the location of the appearances it would presumably involve no modification in the object itself. But, on the other hand, it must be recognized that change in location is inseparable practically from change in events; space and time are relations not merely between empty points but between objects which may act causally on one another. Change in location amounts practically to change in environment, and this is often of great significance. A piece of metal in a magnetic field is not the same as a piece of metal outside of such a field, and if Julius Caesar were alive today he would be a different man. The issue cannot be settled by asking whether the operation is performed on the object or on its physical medium, for often the same result may be brought about either by moving the object into a new medium or by setting up a new environment about the object. The dispute is not important so long as one recognizes the intimate association between position and causal influences. One of the important advantages of the laboratory is that it permits one to place objects in situations in which he has a more or less complete control over possible disturbing factors.

In a great many situations, however, change of location cannot be brought about in nature. Change of temporal location is, except in a very limited sense, impossible. One cannot reënact history, nor can one transport himself back into the past. The possibilities of change in spatial location are somewhat more extensive, though even here there are definite limitations. Man cannot bring the stars down to earth, nor can he travel far either above or beneath the surface of the globe. Hence his information about certain kinds of events is largely indirect in character, and occurs through the instrumentality of other events. The intermediary events are called *records* in the case of historical objects, and *messages* in the case of spatially remote objects. Both records and messages may be defined as appearances of their respective end-objects at certain points in a physical medium—in the one case the medium being essentially temporal in character, and in the other essentially spatial. If the transmission of messages always requires time, as the theory of the maximum velocity of light requires, and if historical records are simply more or less permanent impressions of messages from past objects, it follows that there is no essential difference between messages and records. They are both appearances of their end-objects. Hence there will be no significant difference between historical documents, verbal reports, and scratches on rocks, on the one hand, and telescopes, spectroscopes, and photometric devices, on the other. In both cases one has something which is given directly, viz., written and spoken words, marks, light images and light bands, on the basis of which, knowing the character of the recording device, he infers the corresponding end-objects, e.g., the Golden Age of Greece, the North American glacier, the moons of Jupiter, and helium in the sun. The fact that a human being is simply a somewhat complex recording instrument has been greatly emphasized in recent behavioristic psychology. It has implications to be referred to later in the chapter.

CONTROL OF PHYSICAL MEDIUM

Modifications in the physical medium, i.e., in the *P*-operators, serve to multiply the appearances of the end-object. Since the object is, in its broadest sense, the totality of actual and potential appearances, modifications in the physical medium are techniques for disclosing more and more of the object. The end-object itself, in fact, is simply those of the total appearances which are observed under so-called "normal" conditions of perception. Those appearances which constitute the end-object itself are selected, as was suggested in the preceding chapter, by designating, more or less arbitrarily, a situation in which the physical medium is presumed to produce a minimum of transformations. Then changes are introduced into the medium with a view to determining how the appearances of the end-object are affected by these modifications. Every new appearance affords further insight into the character of the object. Most important of these modifications are those which involve the insertion into the medium of recording and measuring devices, such as thermometers, ammeters, balances, micrometer screws, microscopes, telescopes, spectroscopes, phonographs, hydrometers, counting and sorting machines, clocks, and compasses. Other modifications include manipulative techniques, such as applying meter bars to objects, isolating objects from "disturbing" environmental factors, changing illumination, and varying the position of the observer. The function of these instrumental and manipulative methods is to multiply and vary as greatly as possible the appearances of the end-object to the observer. If the object is the totality of its appearances, one learns more about the object every time he discovers it under a new manifestation. Hence it is to his advantage, in his desire to explore the realm of nature, to increase the range of possible appearances. If one limits the modifications to such as are introduced through measuring instruments, then he is justified in defining physical objects, in the words of Eddington, as "schedules of pointer readings."[1] But if one includes modifications which are not of this specific kind, he cannot define physical objects so narrowly. It seems better to employ the more general definition and to say that a physical object is a schedule of its various appearances at different places and times and under the influence of controlled modifications in the intervening medium.

One of the most important of the transformations introduced by the *P*-operators is the act of *isolation.* An isolational act is the foundation not only of science but of common sense as well. All observation is analytic and

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[1] *Nature of the Physical World,* p. 259.

neglective. Perception is incapable of grasping events of more than a moderate degree of complexity, whether the complexity is spatial, temporal, or qualitative. Hence attention proceeds by focusing itself upon that core of the perceptual field which is considered to be especially relevant, and by either neglecting the remainder entirely or else merging it into the background of consciousness and attending to it only vaguely. In this way simple events, and events of only a moderate complexity, become isolated from their environmental associates and considered as individuals. It is recognized that the neglect is not necessarily a denial of the existence of the associates but merely a disregard of them for the purpose of the problem at hand. One notes, simply, that the complex seems to exist and vary in relative independence of its environmental factors, and one concludes, on this basis, that the complex is neutral to the system in which it is found. "We talk of objects, tables and chairs, as if they were systems neutral to their environments. We shift our furniture from one house to another, and the shape of the table and the comfort of the easy-chair are unaffected by the change of location. We take it for granted because we have found it so. If the softness of an easy-chair varied with the room in which it was placed it is possible that we should not regard it as the completely separate and isolated object we do. We might possibly widen the conception of chair to chair-room. . . . *The first function of experimental inquiry is, if possible, to find precisely how little of an environment need be included to render a system neutral.*" [1] The more refined and controlled activities of science are continuous with the common sense procedures. The concept of an isolated system, which plays so large a part in scientific enquiry, is simply the notion of perceptual object made precise.

Is isolation a transforming operation? This question cannot, perhaps, be answered in general. There is reason to believe that every event is related to every other event

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[1] H. Levy, *Universe of Science*, pp. 50, 51, 52.

in a way which may be significant for each of the events; if so, only the entire universe could be an isolated system. For science, however, the notion of isolated system is relative. Scientific procedure does not demand that one start with isolated situations. Isolation is "a first step in the process of finding out the truth about the universe by examining it in chips. The first step is succeeded by a second more detailed, more refined. It embraces yet a little more of the changing environment as soon as it finds that its initial law is not precisely fulfilled, for by that failure it recognizes that the isolation was not neutral." [1] Hence by a method of successive approximation one makes his systems more and more inclusive.

The isolational act which is here indicated is physical in character, and refers to the actual elimination of disturbing factors from the physical situation. This may involve either moving the end-object into a new environment, such as a laboratory, where the medium can be more or less completely controlled, or merely removing from the given environment as many as possible of the disturbing factors. Often the environmental forces cannot be eliminated but must be neutralized or balanced by counteracting forces. But what is important here is that the isolational acts here indicated are physical in character. There are also selective acts which are of a biological and others which are of a psychological character.

Very important for physical science are *measuring techniques*. The problem of the nature of operations of measurement is too extensive to permit consideration here.[2] Measurement may be defined, in its most general sense, as any technique by which numbers are correlated with events. (This definition makes numbering, or counting, a special

[1] *Ibid.*, p. 54.

[2] The problem will be considered again in Chapter XIII. Some important subsidiary references are the following: W. S. Jevons, *Principles of Science*, Book III; Norman Campbell, *Physics, the Elements* (Cambridge University, 1920), Part II; J. Venn, *Empirical Logic* (London: Macmillan, 1907), Chaps. XVIII, XIX; A. D. Ritchie, *Scientific Method* (London: Kegan Paul, 1923), Chap. V; M. R. Cohen and E. Nagel, *Introduction to Logic and Scientific Method* (New York: Harcourt, Brace, 1934), Chap. XV.

case of measuring.) The actual operations by which numbers are applied to events vary according to the kinds of events to be measured, and according to the range within a given kind of events. For example, one measures the length of an object at rest in one way and of an object in motion in another way; one measures ordinary lengths by yard and meter sticks, but he measures great lengths by triangulation; one measures the distance of the stars from the earth by parallaxes, but he measures the size of the small particles of matter by micrometer screws. Events which are not readily measurable by direct methods are measured by means of other events with which they vary concomitantly, as in the measurement of heat by the height of a column of mercury. Often the attaching of numbers to events indicates nothing more significant in the event itself than the possibility of placing it at a certain point in a series of events similar to it but differing in the degree of some quantitative manifestation; this is well illustrated in the measurement of hardness. All operations of measurement, whether techniques of applying standards to events themselves, or operations of inserting automatic recording instruments at the proper points in the physical media, are methods for increasing the manifestations of events. In the broadest sense of the word, the numbers which are obtained through measuring techniques are simply appearances of the end-object when the corresponding modifications are introduced into the environment.

Three features of measurement may be singled out for brief attention. In the first place, the measuring instrument must be relatively unaffected by the event measured. In general one does not employ a red-hot meter stick to ascertain the length of a block of ice. As was pointed out earlier in the chapter, the strict neutrality of the measuring instrument cannot always be guaranteed. In quantum phenomena the mere introduction of a light impulse interferes with the particle to be measured; a micrometer screw, however delicate, exerts a slight pressure upon the object and therefore flattens it slightly; a thermometer, unless of exactly the same temperature as the object whose heat is to be recorded, modifies its temperature and consequently does not indicate accurately the character of the phenomenon which it presumes to measure. Hence one is obliged to remain satisfied with a relative neutrality. In the second place, the system which is made up of the event and the instrument must itself be a relatively isolated complex. "Every good experimenter has to compensate for all sorts of extraneous effects that enter into his experiments—slight changes in temperature in the room during the progress of the test, draughts, vibration of the building, heat radiation from the body of the experimenter himself, and so on." [1] Finally, no value obtained through an operation of measurement can be divorced from the technique used in obtaining it. Numbers are always *applied* values and one should never lose sight of the character of the application in each specific case. This implies that in *measurement acts* one should recall what he has done, and in the employment of *measuring devices* one should be aware of the principles according to which the instrument is constructed and operates.

An important problem, which may be merely mentioned at this point, arises in the attempt to give measurement a sound theoretical foundation. Is the meaning of any physical concept exhausted when one has stated the techniques by which its measured value may be ascertained? Does time *mean* simply clock readings? Does space *mean* simply numbers on scales? Does force *mean* simply readings on the indicators of balances? More detailed examination of the theory which is implied in an affirmative answer to these questions will be made in connection with the analysis of the basic concepts of the sciences in Part II. Probably the issue can be decided only by examining completely the techniques which are involved in the measurement of any given event, and determining whether the methods of constructing and calibrating instruments, as well as the methods

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[1] H. Levy, *Universe of Science*, pp. 49–50.

of setting them up and reading them in any given case, properly belong in the group of measurement techniques and hence in the definition of the concept under consideration. Bavink criticizes the view that the meaning of a physical concept can be completely described in terms of its measured values. All such concepts, he insists, have meaning before the ascription of numbers to their corresponding objects. "The difference between two lengths of time, or two degrees of loudness of sound, or a high and low musical note, already forces itself upon the child and the savage, before they have any notion of counting and measuring, and pairs of concepts such as loud and soft, long and short, bright and dark, are therefore found at every stage of human thought. Are we then to say of a savage or a child, that they have no concept of loudness of sound or intensity of light, because they are not able, as is the trained physicist, to ascribe numbers to the scale of their sensation? This is surely going too far." [1] The fact to which Bavink refers is frequently forgotten by those who insist that science is aware only of pointer readings. An instrument exhibiting pointers must be both constructed and calibrated. Unless one knew independently of the instrument what the instrument was designed to measure, it could never have been created. Thermometers are meaningless unless one knows what heat is; spring balances have no significance unless one knows what force is; lie detectors are valueless unless one knows independently of them what is meant by a lie. A measured value is always a value of something, and obtained from that something by a describable technique.

CONTROL OF SENSE ORGANS

It is important to notice that the sense organs function in knowledge in the same general way that the physical medium does. In fact the sense organs and nervous system of the observer constitute a new context within which events may be observed. Hence the eye, for example, may properly

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[1] Bernhard Bavink, *The Natural Sciences* (New York: Century, 1932), p. 226.

be considered as a recording instrument which discloses the appearance of the end-object in the biological medium in much the same way that a camera reveals its manifestation in the physical medium. In other words, the visual image of an end-object is simply that object as it appears in the bodily context. The same is true of all sense-data. Hence the aim of the scientist should be, at this point, as in the case of the physical media, to increase as much as possible the range and variety of appearances.

That this is in accord with actual procedure, both of common sense and of science, seems clear. One observes the end-object in the widest possible range of contexts. He supplements the information received through vision by that obtained through audition, touch, smell, and taste, and he supplements his private information by calling upon other observers to report their findings. In this way he multiplies the appearances of the end-object by inserting different sense organs and different observers in the medium.

But in this control of the organism, a new motive has emerged. This motive centers about the desire to establish, so far as is possible, *normal* conditions of observation. It is commonly recognized that the sense organs are important sources of error. Unfortunately, however, one is not always able to detect the presence of error. This is due mainly to ignorance of the nature of the *B*-operators. Nerve physiology has not yet reached a stage of development which permits the accurate description of the physical impulse in its course through the organism. Thus one is in the position of being able to read the recording instrument, but unable to interpret its information since he knows neither the nature of that which it was designed to record nor the principles according to which the instrument operates. All that he can do is to multiply the reports of the instrument, and determine their consistency with one another. For this reason the multiplication of appearances serves not so much to increase information about the object as to test the accuracy of some one bit of information about which the observer may be in doubt. If I am not sure as to what I hear, I look; if I am then not sure, I touch; if I am still in doubt, I call upon others to corroborate my judgment. If the results are consistent, I conclude that my sense organs are functioning normally; if they are not, I decide that they are operating abnormally. Thus control of the physical situation differs from control of the observer in that the former serves to provide him with more information about the end-object whereas the latter serves to check the accuracy of observation.

It should be pointed out, further, that the variation in possible appearances is much more limited when one is concerned only with *B*-operators. At best man is limited to five senses. But although practically all objects of science manifest themselves visually, a more limited group exhibits touch and sound properties, and very few have taste and smell properties. Hence there is really only one type of appearance which is important. The scientific observer, says Eddington, "has one eye (his only sense organ) which is color-blind. He can distinguish only two shades of light and darkness so that the world to him is like a picture in black and white. The sensitive part of his retina is so limited that he can see in only one direction at a time . . . we have left the observer power to recognize that a pointer coincides with a gradation on a scale."[1] It is somewhat doubtful whether an observer of this kind would be able to progress very far, even in astronomy. Certainly he would make a poor biologist. But the important point is that significant information about objects comes through a very limited number of routes.

CONTROL OF STATE OF MIND

The essential problem in the control of the *M*-operators is, again, that of assuring oneself that he has *normal* conditions of awareness. Though present knowledge of the psychological medium is even less adequate than that of

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[1] *New Pathways in Science*, p. 13.

the physiological medium, nevertheless one can easily convince himself of the existence of certain factors which may be lumped under the term "state of mind" having a potentially disturbing influence. His problem is either to eliminate these elements or to ascertain the character of the modification which is due to them. What must be assured is that there is no error in the location of the appearances of the object in their proper contexts.

The first of these factors is the *selective activity of attention.* This can hardly be called a disturbing factor, since only by means of it can one become clearly aware of anything. Furthermore, it is not an abnormality in the sense that it would be absent from normal observation. What should be emphasized with reference to it is simply that it determines isolated systems in much the same sort of way that physical operations do. The isolation, however, is not one which eliminates or counteracts environmental influences, as is the case with physical operations, but rather one which neglects. The observer pays no attention to the associated factors, and thus considers them as non-existent. When there is a conscious recognition of this act of neglect, the selective act of attention cannot be called disturbing. But attention is often unconsciously selective. Frequently one neglects environmental factors without being aware that he is doing so. For example, one easily overlooks misspelled words even when he is reading proof. It is a common experience that one observes in any situation what he expects to observe and fails completely to note other elements, even when they are of great intensity. Hence the selectivity of attention must be admitted as a potential source of error.

Second, and most important among the factors in the control of the state of mind, are those associated with *symbolizing the data.* As was indicated in Chapter III, knowing is not merely acquaintance but description as well. Science must not only *get* its data; it must endeavor to portray them by various symbolic devices. Events are private and transitory as they enter into awareness, and if they are to be used in science they must be made communicable and fixed. This is accomplished by the important device of the symbol.

The initial difficulty in discussing this problem is the fact that direct awareness may, perhaps, never occur apart from symbolic awareness. There is reason to believe that merely attending to an event requires some conception as to *what* the event is. A pure sense-datum probably never occurs apart from some interpretation. It may be that one cannot be aware of a blue object unless he knows what blueness is, or of a round object unless he knows what circularity is, and—since one could be aware of blueness and circularity only as symbols—the presumption would be that a non-symbolic awareness of events could not exist. It has already been shown that direct awareness without inference is of comparatively rare occurrence. Certainly inference always involves symbols, and there is probably some truth in the statement that all symbolism involves inference. For example, to symbolize an object as "blue" is to attribute to the object all of the meaning of the symbol—its relation to wave lengths, to other shades in the color chart, and so on. Since all of these associations are called to mind immediately in the presence of a blue object, there is reason to doubt whether one ever gets such a pure awareness of an uninterpreted object.

However, although the two types of awareness may never occur in isolation from one another, they do obviously occur in distinction from one another. This is all that is required for purposes of analysis. It is possible in some sense to compare the symbol with the event, and thus to test its adequacy. This act of comparison is accomplished, as has already been seen, through a third distinct type of knowing. What is important is merely that if the symbolic interpretation is more or less automatically applied to the event, error can be detected only by separating the two. Practically, this means that one's interpretations must be deliberate and critical rather than hasty and unreflective.

Granting that data may be symbolized by a more or less deliberate act, there arise the problems as to what this operation is and as to what form the symbols take. These two problems may be discussed briefly in the order given.

The nature of the operation is determined by one's conception of the general task of science. Does science merely *describe* or does science also *explain?* The more detailed analysis of these two basic notions will be given later. For the present it may suffice to point out that in description one endeavors to symbolize the *obvious* features of events, whereas in explanation he tries to represent their *less* obvious features—their causes and effects, their constituents, their appearances under different conditions, and so on. Now since a knowledge of the less obvious features is presumably based on a knowledge of the more obvious, description must precede explanation. If one adopts the positivistic point of view that the task of science is merely to describe and not to explain, he will insist that science has completed its work when it has pointed out the obvious features of events. But since, presumably, no one would maintain that the task of science is merely to explain and not to describe, one may conclude that the very minimum which science can do is to symbolize the obvious features of events. Hence science is at least description.

Description may be defined as the operation by which the more obvious features of an event are symbolized. Gold would be described as a hard, yellow, lustrous substance; a robin as a bird about ten inches in length, slate-gray above and chestnut-brown beneath; an orange as a spherical fruit, orange in color, having a spicy smell and a sweet taste. If the complex term in each of these cases means simply the properties enumerated, the application of the term itself to the event would also be description. Since description passes imperceptibly into explanation, an enumeration of some of the less obvious properties might be called description by contrast with a characterization of the highly obscure features. Thus one might also describe gold as a substance which is highly malleable, melting at 1075°, having a specific gravity of 19.3, and so on, which would contrast with its explanation in terms of atomic structure. Description may be inventive in character in case one wishes to devise a symbol for a property not yet adequately symbolized in the language system. This occurs commonly in the naming of heavenly bodies, chemical elements and compounds, scientific instruments, and animal and plant forms. Or description may involve simply the application to the event of a symbol which is already a part of the language system and used to represent an event of the same kind as that under consideration. Concepts fluctuate in meaning by virtue of this general applicability. In the cases both of invented and of borrowed symbols the presumption is that the event as a whole, or certain of its apparent qualities, has been directly observed, and that the symbolic activities of the mind are then called into operation with a view to constructing a representative of the event which will preserve it in a more permanent and communicable form.

The particular form which the symbol takes will be determined by the problem. It has already been seen that there are different types of symbol, each having its own function. If one is interested in the vividness and accuracy of his symbol, he will employ an icon, i.e., he will form a clear image, or draw a picture or diagram, or construct a model. But if one is interested rather in talking or writing about the event he will employ the symbolism of ordinary language, i.e., he will employ concepts or propositions.

Supposing that one has adopted linguistic symbols, he then finds that there are at least three important descriptive techniques. These are *classifying*, *ordering*, and *correlating*. These may be called operations, since they are of essentially the same kind as the manipulative operations in the physical sphere. They are various things which one *does* to events. But they should properly be called *mental* operations, since they clearly are not operations on events in the ordinary sense of the word, i.e., they do not transform the events physically. They are ways of thinking about events, though they are presumably based upon the characters of the events themselves, and there are right and wrong ways of performing them.

Classifying may be briefly described as the operation of representing an event or its properties by a concept. The concept may be chosen because of the general character of the event as a whole, as when one describes a substance as gold, or it may be chosen because of some obvious feature of the event, as when one describes gold as yellow, lustrous, and hard. Concepts are class-notions, having *generality* of reference. A concept refers extensionally or denotatively to the totality of events exemplifying or illustrating it. This totality is called a class. Classes are determined extensionally if one enumerates the members and then searches for the concept which will describe all of them; classes are determined intensionally if one starts with a concept and then looks about for events exemplifying it. When a given event is classified it is placed (mentally) with other events exemplifying the same concept. When a concept is available for use, classification is identification or recognition; but when no concept in the language has precisely the required meaning, classification is the devising of a new symbol which will prepare for future identification or recognition. An event may lie in many classes if it has many properties; gold, for example, lies in the class of yellow things, of lustrous things, and of hard things.

Ordering may be described as the operation of representing a certain specific relational property of events by a concept. Events may be ordered if (1) they may be classified, and (2) there exists between every two members a relation which is (*a*) asymmetrical and (*b*) transitive. This is the technical definition of the notion. Property (1) states that events which are capable of order must be like one another in some respect, usually the respect according to which they are to be ordered. For example, all hard objects may be put into a class, or all lengths. Property (2) states that events which are capable of order must bear a certain type of relation to one another. This relation must be (*a*) asymmetrical. A relation is asymmetrical when the order of terms is significant, i.e., when an interchange of the terms would produce a relation-complex incompatible with the original. For example, if a substance *A* scratches *B*, then *B* cannot scratch *A*; or if *X* is longer than *Y*, then *Y* cannot be longer than *X*. The relation must also be (*b*) transitive. A relation is transitive when, if it holds between *P* and *Q*, and between *Q* and *R*, it then holds between *P* and *R*. A transitive relation is one which has a carrying-over power. For example, if a substance *A* scratches *B*, and *B* scratches *C*, then *A* scratches *C*, or if *X* is longer than *Y*, and *Y* is longer than *Z*, then *X* is longer than *Z*. The presence of this common property and such an ordering relationship permits the members of the class to be arranged into a series. Events have not only membership in the class, but position. For example, hard objects may be arranged in a series, beginning with *A* which scratches all other members, and ending with, say *K*, which is capable of being scratched by all members—all other members being properly placed between. Also all lines may be arranged on the basis of length. In the same way events may be arranged according to weight, size, temperature, specific gravity, melting point, etc. Each event capable of such arrangement possesses a relational property by virtue of which the ordering is possible. When this property has been conceptualized, the event may be placed in its proper position in the series. The placing in a series is, of course, a mental operation, though it may take on pictorial representation as well, as in the case of museum displays.

Correlating may be described as the operation of representing the association or the connection of events. Events have the property of occurring together, and thus uniting into complex events. Usually the "togetherness" implies proximity in time and space, though remote events may be connected by intermediate events. The presumption of any association is that there is some dependence of one event on the other. When the association is of a very close spatial or temporal kind, there is almost always conceived a new event which is the associative union of the two; in this respect the correlation of events is the opposite of their isolation, discussed earlier in the chapter. When the association of the two events is infrequent, the dependence of the one upon the other is slight; when it occurs often the dependence is more pronounced; when it occurs universally the dependence is absolute. Correlation is the mental operation by which one symbolizes the fact of connections in nature, and symbols for such correlations, when they occur repeatedly, are called scientific laws. If one should note that two or three of his acquaintances who have red hair are quick tempered, he might be led to believe that the properties are related, though he would attach slight probability to it; if he found it to be illustrated in a much more extended group he would begin to suspect a connection; if he found it to be true universally he would be obliged to express it in a universal law. (The problem has, of course, been enormously simplified for purposes of illustration.) There is reason to believe that the correlations which occur in descriptive science, i.e., where one is concerned only with the most obvious features of nature, are not universal. That events are *universally* connected in nature is not one of its apparent features, but discernible only through elaborate exploratory devices. Hence correlations at this stage of study are primarily statements of frequencies.

The third factor in the control of the state of mind is *unconscious inference.* Here, again, there is some doubt as to whether this should be called a disturbing factor since it may be present in all observation. But that it is a very common source of error cannot be doubted. One reads into situations and supposes to be actually present elements which are the results of unconscious inference. A snow scene looks cold, and velvet looks soft; a table-top which is observed as a parallelogram is assumed to be rectangular;

a reading on a thermometer tells one that the day is hot. It is a common experience that one may observe as present in a situation elements which have been suggested in advance as likely to be present, or elements which the observer would prefer to have present, or even elements which he would prefer not to have present. In all of these cases the ascertainment of the strictly *given* is quite impossible, for the inferred objects are fused with it or substituted for it, and no act of attention is able to single it out.

This inferential act is precisely that to which reference was made in the preceding chapter in connection with the discussion of perception. It was suggested at that point that one's concern is not always with the immediately given but with the appearances of the end-object in other contexts. Such information can be gained only through an inferential act. Now there is no danger in this inferential act provided it is recognized as present and, consequently, as a possible source of error. One of the essential tasks of science, in fact, is the making and justifying of precisely such inferences. But when the inferences are unconscious they are important sources of error, for one supposes to be immediately given something which is only inferentially present. This results in a confusion of contextual realms, and hence in an erroneous conception of the part which *P*-, *B*-, and *M*-operators play in the determination of appearances. So long as the inference is conscious, justification is sought for it in the knowledge of the laws of operators; so long as it is unconscious, it enters as a "disturbing" factor and becomes an important source of error.

The outcome of these physical, physiological, and psychological considerations is the recognition that the task of getting and increasing data is one of great difficulty. In addition to the problem of controlling the object itself and the environment, the aim of which is the production of the maximum of manifestations, there is the difficulty in assuring oneself that what he gets through these activities are indeed genuine data rather than aberrations of his sense organs and states of mind. Such considerations serve to show that the collection of data is hardly a preliminary scientific technique, in the strict sense of the word. More accurately, the accumulation and testing of data constitute a task which is part and parcel of the scientific method itself. One does not start with data, for one never knows whether his data *are* data until he has made a critical analysis of them. And this very analysis involves an assumption of many of the laws of science, which are themselves demonstrable only in terms of such data. In order to assure oneself that he has normal conditions of observation he must use the laws of physiology and of psychology. But these laws are themselves simply generalizations from other data which are questionable upon similar grounds. Hence, in order to ascertain the correctness of data one must assume other data—which may themselves be incorrect. This difficulty is unavoidable.

When one has obtained his data through perception; when he has increased them by operations performed upon the end-objects and the physical media; when he has controlled observation by attention to the possible disturbing factors in the physiological media; finally, when he has discerned the various psychological factors, such as isolational, symbolic, and inferential acts, he has completed the so-called preliminary operations of science. What he gets through this series of operations is a system of symbols presumed to have the power of representing the data which first called them into being. This system of symbols constitutes a science. The next chapter will be devoted to an examination of such an embryonic science.

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CHAPTER VII

DESCRIPTIVE SCIENCE

Science, in the broadest sense of the word, is any body of symbols presumed to be applicable to the realm of events. In this definition the term "applicable" is conveniently obscure. The widest disagreements exist among the logicians of science as to the precise nature of the relation which the symbolic scheme bears to the world. The symbols may "describe" or they may "explain"; they may be merely "conventions" or they may be "instruments of practical adjustment"; they may be mere "constructions" or they may "reveal the true nature of things." It will be the task of the next chapter to make a critical examination of some of these notions.

For the present, attention will be directed to a more empirical question. There are, clearly, certain sciences which claim to be empirical or descriptive in character; biology, geography, sociology, economics, and psychology are presumably such sciences. If there is any meaning in this characterization, it seems reasonable to suppose that such sciences as these exhibit certain features by virtue of which they may be thus grouped together. Are there any aspects of subject matter, or method, or symbolic reference, or integration, by which an empirical science may be recognized?

An alternative approach to the same problem is indicated by the considerations brought forward in the preceding chapter. The initial stage of every science, as was shown, lies in the accumulation and symbolism of data. Supposing that these preliminary processes have been completed, is one justified in saying that the result is *science?* If the term is defined sufficiently broadly, there seems little doubt as to the answer. Such a body of symbols is at least an embryonic science, and should reveal many of the features of more highly developed knowledge. It may thus be worth while to make an analysis of such a system with a view to determining its significant features.

Furthermore, even the sciences which are characterized as non-empirical, or rational, contain aspects which suggest that they are not to be sharply divorced from the descriptive sciences. Jevons argues that every science contains facts of four different kinds. "(1) We may be acquainted with facts which have not yet been brought into accordance with any hypothesis. Such facts constitute what is called *Empirical Knowledge.* (2) Another extensive portion of our knowledge consists of facts which having been first observed empirically, have afterwards been brought into accordance with other facts by an hypothesis concerning the general laws applying to them. This portion of our knowledge may be said to be *explained*, *reasoned* or *generalized.* (3) In the third place comes the collection of facts, minor in number, but most important as regards their scientific interest, which have been anticipated by theory and afterwards verified by experiment. (4) Lastly, there exists knowledge which is accepted solely on the ground of theory, and is incapable of experimental confirmation, at least with the instrumental means in our possession." [1] If one admits, with Jevons, that every science contains facts of the type (1), he is obliged to conclude that it is, to that extent, empirical or descriptive. Furthermore, if one admits that the lines separating these four kinds of facts cannot be sharply drawn, he is obliged to conclude that the difference between a descriptive and a rational science is itself a relative matter. This argues, at least, for the importance of clarifying the distinction as much as possible.

Granting, then, that there are descriptive sciences, one may attempt to determine their features. Such a science, according to Jevons, is any body of symbols which is "derived directly from the examination of detached facts,

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[1] *Principles of Science*, p. 525.

and rests entirely on those facts, without corroboration from other branches of knowledge. It is contrasted with generalized and theoretical knowledge, which embraces many series of facts under a few comprehensive principles, so that each series serves to throw light upon each other series of facts. Just as, in the map of a half-explored country, we see detached bits of rivers, isolated mountains, and undefined plains, not connected into any complete plan, so a new branch of knowledge consists of groups of facts, each group standing apart, so as not to allow us to reason from one to another." [1]

Allowing this to serve as a preliminary, one may attempt to discuss somewhat more systematically the features of descriptive science. Since it is a body of symbols, and every symbol has, as was shown in Chapter IV, extensional and intensional relations, one may suggest the examination of descriptive science from the point of view of (*a*) extension, i.e., the character of its reference to events, and (*b*) intension, i.e., the character of the interrelations of the symbols among themselves.

EXTENSIONAL FEATURES OF DESCRIPTIVE SCIENCE

Extensionally every descriptive science aims (1) to represent the most clearly given events, by symbolizing the more obvious of (2) their classificatory, (3) their serial, and (4) their correlational features. The precise meaning to be attributed to each of these aspects is important, and warrants a more detailed consideration.

(1) Since every descriptive science is presumably a linguistic system, the representation involved in any such science is that kind which is peculiar to word symbols. (Numbers and other mathematical symbols may here be considered as condensed word symbols.) No word symbol, as has already been seen, portrays its referent in any direct or pictorial sense. Words do not look or sound like the events to which they refer. But they have the capacity, by virtue of

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[1] *Ibid.*, p. 526.

peculiar meaning properties which are attached to them, to refer to events in a less direct way. If one knows the meaning property of any word symbol, he knows in advance the kind of event which is being referred to, though he need not be aware of the event at the moment. The symbol enables one to ascertain whether any event which may be given is or is not the referent of the symbol. This selective feature of the symbol permits it to be correlated with its referent. Ideally there is a one-to-one correlation between the elements of a system of symbols and the elements of a system of events. For every event there should be one and only one symbol, and for every symbol there should be one and only one event. Actual language is far from ideal in this respect. There are words which are ambiguous in meaning, and there are diverse linguistic forms which are practically synonymous in meaning. Descriptive science aims to be representative only in the sense that it tries to set up a one-to-one correlation between events and symbols. Hence such a science is inadequate either if there are clearly given events for which there are no symbols, or if there are symbols which refer to no events or to events ambiguously. The former inadequacy would be illustrated by a botanical science which has not yet identified and classified all known plant forms, and the latter by a physics which talks about perfect levers and gases or by a chemistry which has only one symbol to designate the various isotopes of a substance.

The essential difficulty in determining precisely what is meant by a descriptive science is the fact that data are given with varying degrees of clarity and obviousness. One of the most significant conclusions to be drawn from the history of thought is that investigators disagree among themselves as to what is and what is not clearly given. The problem, what are the ultimate data for knowledge, has never itself been satisfactorily solved. What is clearly given for one individual may not be so for another; and what is clearly given for an individual at one time may not be so at another. One cannot draw the limits of the given, and relegate all beyond the limits to invention and fancy. One of the facts which seems to be given clearly is the fact that some things are given more clearly than others, and one of the facts which seems to be given quite obscurely is the precise location of the line separating these two realms. As a result it becomes impossible to say with precision what the limits of a descriptive science are. If descriptive science is defined in terms of a criterion which is itself variable, the result must be a recognition that sciences are not properly descriptive or non-descriptive, but more or less descriptive according as they endeavor to symbolize the more or less obvious data.

This fact of the variability in the given may be clearly indicated by illustrations drawn from recent and contemporary writers on the philosophy of science. The problem under consideration is to determine precisely what kinds of things the data of science are. About what, in other words, does the scientist really talk? The question is so simple that one would be led to expect a simple answer, and a general agreement among investigators. But the contrary proves to be the case. Hear the report, for example, of an eminent scientist. "Science is in reality a classification and analysis of the contents of the mind; and the scientific method consists in drawing just comparisons and inferences from the stored impresses of past sense-impressions, and from the conceptions based upon them."[1] "An external object is in general a construct—that is, a combination of immediate with past or stored sense-impressions."[2] The only events which are clearly given, therefore, are sense-impressions, which are both subjective and private. All else is inference and construction. But Planck is not satisfied with this. It is one of the basic theorems of science, he insists, that "there is a real outer world which exists independently of our act of knowing."[3] This is no longer

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[1] Karl Pearson, *Grammar of Science*, p. 52.

[2] *Ibid.*, p. 41.

[3] *Where Is Science Going?* (New York: Norton, 1932), p. 82.

solipsism. According to Norman Campbell what the scientist investigates is not a private world but a public one, for "science is the study of those judgments concerning which universal agreement can be obtained."[1] Again, Emile Meyerson, one of the most energetic of the critics of subjectivism in science, writes, "There can be no doubt from the start as to the mental attitude of the physicist who studies *nature;* he does not believe at all that he is investigating merely the relations between sensations, but, on the contrary, that he is penetrating into a mystery which is independent of his sensation."[2] The given, therefore, includes not merely sense-impressions, but public and external objects, beyond which all else is inference and construction. But still other writers, recognizing the variability in the clarity with which objects may be given, insist that the data of science are even more extensive. Hypothetical entities, such as molecules and electrons, are, according to Whitehead, natural objects. "Scientific laws, if they are true, are statements about entities which we obtain knowledge of as being in nature . . . if the entities to which the statements refer are not to be found in nature, the statements about them have no relevance to any purely natural occurrence. Thus the molecules and electrons of scientific theory are, so far as science has correctly formulated its laws, each of them factors to be found in nature."[3] Since nature consists of all those events which are given in a definable sense, and there is a definable sense in which molecules and electrons are given, it follows that they are natural objects. The given, therefore, includes not merely sense-impressions and external objects but a large number of entities not even theoretically observable. But if the given is sufficiently flexible to include both an extreme solipsism and a radical realism, there seems only one conclusion to be drawn. The given must be a variable.

But even if one grants that science is bound to admit the

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[1] *What Is Science?* (London: Methuen, 1926), p. 27.

[2] *De l'explication dans les sciences* (Paris: Payot, 1927), p. 33.

[3] *Concept of Nature*, pp. 45–46.

existence of molecules and electrons, he is not obliged at the same time to acknowledge that it must give to these entities the same status as observed data. Though they may be given, they are less obviously given than sense qualities. Hence a science which talks about hypothetical and theoretical entities, constructs, fictions, idealizations, and other suppositional entities must be *less* rather than *more* descriptive. This is all that need be admitted in order to establish the existence of sciences which are predominantly descriptive. A descriptive science can therefore be defined as one which minimizes rather than maximizes the importance of such entities. Descriptive science frankly admits that it has no concern for considerations of this kind, since they arise only when questions of the rationality or intelligibility of nature are raised—questions which are foreign to the program of empirical science. The task of descriptive science is the complete description, classification, and organization of objects as they appear to the observer. A perfect science is the most inclusive and the most orderly formulation of the laws connecting appearances. The desire to *explain* the interconnections of nature has not yet arisen, hence there is no urge to get beneath or behind phenomena. Descriptive physics would contain no symbols for molecules, atoms, or electrons; instead it would describe as exactly as possible the way in which the macroscopic, i.e., the ordinary, objects of everyday experience influence and are influenced by one another. Descriptive biology would make no mention of genes and vital forces; instead it would endeavor to express as accurately as possible the variations in the behavior of organisms under different stimuli. Descriptive sociology would not refer to vague social forces, minds, and wills, but would rest content with the statistical description of the behavior of individuals in society. Descriptive psychology would reduce to descriptive biology, except that the behavior reactions would be more complex.

(2) The essential method of descriptive science is classification. This is involved in the very notion of the linguistic

symbol, which is the essential tool for handling data on any level. Every linguistic symbol (except proper names and certain applicational symbols such as "the," "a," "some," etc.) is either itself universal or contains elemental symbols which are universal. Hence such symbols determine classes of referents. In the very act of representing an event by a symbol there has been performed an operation of classification. If this is true, classification cannot be considered as the peculiar feature of descriptive science; on the contrary any science must, in so far as it employs linguistic symbols, use classificatory techniques. What distinguishes descriptive science from other science, therefore, must be the way in which classification is employed. The essential feature seems to be that emphasis is laid on specific and narrow rather than highly abstract and inclusive classes. If one is to remain as close as possible to the clearly given, he must avoid wide and sweeping generalizations. High abstractions are vague and indefinite, and classes defined by them involve an extensive anticipation of experience. For this reason descriptive science, in its attitude of caution, classifies events only in terms of those obvious features which are called to one's attention by the fact that he observes other objects which do not possess them. Color, sound, smell, taste, shape, size, weight, hardness, and so on constitute the apparent properties of events. "The usual mode in which an investigator proceeds to form a classification of a new group of objects seems to consist in tentatively arranging them according to their most obvious similarities. Any two objects which present a close resemblance to each other will be joined and formed into the rudiment of a class, the definition of which will at first include all the apparent points of resemblance. Other objects as they come to our notice will be gradually assigned to those groups with which they present the greatest number of points of resemblance, and the definition of a class will often have to be altered in order to admit them. The early chemists could hardly avoid classing together the common metals, gold, silver, copper, lead and iron, which present such conspicuous points of similarity as regards density, metallic lustre, malleability, etc."[1]

It seems to be essential to the notion of classification as employed in empirical science that its results are considered merely as tentative. A property which appeared to be obvious may prove to be somewhat obscure on more careful examination; superficial resemblances may hide deeper differences, and superficial differences may hide deeper resemblances. Jevons goes on to point out that the early classification of metals is soon seen to exhibit difficulties. "Antimony, bismuth, and arsenic are distinctly metallic as regards lustre, density, and some chemical properties, but are wanting in malleability. . . . In this way it comes to pass that almost every classification which is proposed in the early stages of a science will be found to break down as the deeper similarities of the objects come to be detected."[2]

The classificatory schemes found in such a science may be called "diagnostic" or "indexical." They are essentially practical in character and serve merely as techniques for locating events conveniently. Their purpose is not to reveal remote similarities or abstruse connections, but merely to afford simple devices by which objects may be referred to their proper classes, and other properties inferred. Often the classificatory scheme is nothing more than an index or catalogue by means of which one establishes a correlation between a place where an object may be found and a certain tag which is attached to the object, as in the case of a telephone directory. Negatively, one may say that classificatory schemes at this level of science do not exhibit hidden resemblances. For example, one would not expect to find circles classified with rectilinear figures, though abstractly they may be considered as rectilinear figures of an infinite number of sides; nor would one expect chlorine which is a gas, bromine which is a liquid, and iodine which is a solid to be classified together in spite of their similarity in

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[1] W. S. Jevons, *Principles of Science*, p. 690.

[2] *Ibid.*, p. 691.

chemical properties; nor would one expect to find light and electrical phenomena to be grouped together, though modern physics has shown that they are identical. Resemblances such as these are revealed only in the more advanced stages of the science, and through the application of highly complicated techniques. For a purely descriptive science they are non-existent.

(3) Similar remarks apply to the serial properties of events. It seems safe to say that recognition of order requires greater abstraction than recognition of mere similarities. Hence it is likely that a purely descriptive science will contain only a minimum of ordered groups. Such as are found will be based on obvious properties, like size, shape, weight, motion, color, and sound. Most of these sense qualities exhibit apparent variations in degree and thus may be arranged in series. Where there are no obvious differences in degree there are relations such as that of "betweenness" which permit the establishment of ordered groups. For example, green is quite obviously between blue and yellow, yellow between green and orange, orange between yellow and red, and so on; in this way the usual spectrum arrangement of the colors becomes possible. Similarly, a square is clearly between a triangle and a pentagon, and a pentagon between a square and a hexagon; in this way regular rectilinear figures may be arranged in order.

When the order of events is of a certain kind, measuring techniques become applicable. Descriptive science does not employ measuring operations unless it wishes to introduce greater precision than is possible through the crude judgments of sense perception. Man's unaided awareness enables him to make relatively satisfactory judgments as to which of two objects is larger, or heavier, or softer, or moving faster. The techniques for such decisions are simple. Object *A* is larger than object *B* if when the two coincide in certain points certain other points of *A* extend beyond the corresponding points of *B*; object *L* is heavier than object *M* if the former "pulls" more than the latter when they are unsupported; object *P* is softer than object *Q* if a given force makes a deeper impression on *P* than on *Q*; object *X* is moving faster than object *Y* if they are placed in a race and object *X* reaches a certain point before object *Y*. Measuring techniques are essentially the continuation of this process in the direction of greater refinement. Elaborate measurement techniques will presumably not be found in purely descriptive sciences, for these are often founded upon hidden correlations, as in the case of the measurement of heat by the expansion of a column of mercury, and in the case of the measurement of weight by the distortion of a coiled spring.

(4) The correlational features of descriptive science constitute its most distinctive properties. "Things are correlated (*con, relata*) when they are so related or bound to each other that *where the one is the other is, and where one is not the other is not*. . . . In geometry the occurrence of three equal angles in a triangle is correlated with the existence of three equal sides; in physics gravity is correlated with inertia; in botany exogenous growth is correlated with the possession of two cotyledons, or the production of flowers with that of spiral vessels." [1] But the correlations here used for illustrative purposes are not such as are found in empirical science. Descriptive science asserts not universal laws but merely enumerative generalizations. Often these correlations are merely statistical in character. The cautious attitude which is characteristic of science at this stage does not permit rash generalization. Laws are asserted only over the range of examined cases. Descriptive anthropology avoids extending to society as a whole customs of monogamous marriage found only in a limited group; biology refuses to apply the principles of sexual generation to the organic world as a whole on the basis of its apparent omnipresence in the higher living forms. Correlations in descriptive science are properly stated in the form of frequencies.

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[1] W. S. Jevons, *Principles of Science*, p. 681.

The result is that descriptive science cannot be said to exhibit the *necessary* features of the realm of events. It affirms merely that events are occasionally or frequently associated. That events are sometimes associated may be ascertained with a high degree of certainty, but that they are universally and inevitably associated cannot be established without elaborate assumptions and complicated operational techniques. A universal law extends beyond the actual cases examined, and therefore involves an anticipation as to the behavior of the still-unexplored. But the notion of anticipation is essentially opposed by descriptive science, which maintains that symbols should *follow* events, not *precede* them. This is in line with the spirit of extreme caution which is characteristic of descriptive science. If events cannot be known to be necessarily related, they should not be presumed to be so related.

INTENSIONAL FEATURES OF DESCRIPTIVE SCIENCE

Intensionally every descriptive science is (1) loosely integrated, and (2) non-explanatory. Considerations of the intensional character of science involve turning attention away from events and toward the interrelations of the symbols. Intension was defined in Chapter IV as the relation of a symbol to other symbols in the system. At this point examination will be made of the internal features of empirical science.

(1) By the loosely integrated character of descriptive science is meant the aggregational rather than the systematic character of the body of symbols. Loosely integrated complexes are commonly characterized as collections, conglomerates, or heaps, whereas highly integrated complexes are usually called systems, or organic and organized wholes. A pile of stones exemplifies an aggregate, while a machine or a human organism illustrates a system. Descriptive science resembles the former in that no concept has important relations to other concepts, and no law is intimately connected with any other law. Classificatory schemes, involving

the sub- and super-ordination of classes have not been established, and special laws have not been shown to be cases of more highly abstract laws. Any element of the scheme might be modified or taken from it without seriously influencing the remaining elements; a novel feature might be added without necessitating any changes within the body of symbols.

The best way to describe this feature is to suggest that the order of elements in an empirical science is that of *acquisition* rather than of *demonstration.* The propositions take on the order in which they have been discovered, rather than the order in which they could be deduced. The order of discovery, however, is more or less accidental, and might have been otherwise; but the order of logical deducibility is a fixed one: premise is always more basic than conclusion. Descriptive science is quite without deductive organization; the distinction between derived and underived propositions has not yet arisen. All propositions are on the same level logically; no proposition is the ground of another. Propositions in descriptive science merely *follow* one another; they do not *follow from* one another. No element of the totality, therefore, *determines* any other element; the elements are independent of one another, and the complex is an aggregate rather than a system.

An illustration of the difference between the order of the acquisition of propositions, which is descriptive of the structure of empirical science, and the order of the deduction of propositions, which is descriptive of the structure of rational science, is to be found in the distinction between a scientific *biography* and a scientific *textbook.* If a scientist were to write his autobiography he would describe his discoveries in the order in which they occurred to him. Chapter I would disclose the conclusions of his early period; Chapter II would indicate how his reflections were turned in a certain direction by these results, and how he was led to perform experiments of a certain kind; Chapter III might show how these conclusions led him to modify the formulations of his early life; Chapter IV might indicate how a chance observation led him to formulate still another theory; and so on. The total body of propositions representing his conclusions in the field would be contained in his biography. But their order would not be significant of their logical relations to one another; the propositions in Chapter II would not be *logically derivable* from those in Chapter I, though there might be a relation of *psychological suggestion* between them. Contrast this with the order of propositions in a textbook on science. Chapter I usually contains the most general principles, both of method and of subject matter. Chapter II, perhaps, indicates either their most immediate deductive consequences or their most apparent applications to phenomena of a certain kind. Succeeding chapters show progressively how more complicated theorems and corollaries can be deduced by the use of more complicated logical techniques. As in the case of a biography, the total body of propositions contained in the book indicates the conclusions of the scientist in the field. But their order is significant of their logical relations to one another. By learning where a proposition occurs in the book one would be able to conclude as to its position in the logical scheme. The structure of descriptive science, if it has any structure at all, is that of a biography, not that of a textbook. On the other hand, the structure of rational science, as will be shown later, is that of a textbook, not that of a biography.

An alternative formulation of the same feature of descriptive science is to say that there are no highly integrating propositions. If a system of symbols is organic, its relational aspects must be important. But the relational aspects of a symbolic system must themselves be expressed by symbols. A law, for example, expresses a relation between concepts, and a highly general law states a relation between more specific laws. A highly integrated symbolic scheme, therefore, would be one which contains many necessary and universal laws. But this is precisely the kind of symbol which is lacking in descriptive science. The correlations are merely occasional or frequent associations, and the only laws possible are statistical. Hence the concepts interrelated by such laws are not as determinately related to one another as they would be in the case of universal laws. To know that an event is occasionally or frequently associated with another does not tell one as much about the event as to know with what it is universally or inevitably associated. As a consequence, in descriptive science one can gain no important information about a symbol by exploring its intensional features, i.e., its relations to other symbols. One determines what a symbol means not by *definition* (intension) but by *illustration* (extension). In other words, one determines the properties of an event by pointing to the event, rather than through the indirect route of finding some other event of known properties with which the event in question is universally associated.

This is equivalent to asserting that in descriptive science extension is taken as regulative over intension. Every symbol has its meaning determined by that to which it refers empirically, not by other symbols to which it has relations. In the language of the traditional logic, all propositions of descriptive science are *synthetic* propositions, not *analytic.* A synthetic proposition is one whose predicate contains something not contained in the subject; an analytic proposition is one whose predicate can be obtained from the subject by mere analysis of its meaning. The proposition, "All bodies have weight," would presumably be synthetic, since its truth cannot be determined by analysis of the subject term, i.e., by "body" one does not mean "something possessing weight." On the other hand the proposition, "All triangles are three-sided figures," would be analytic, since its truth can be determined by a mere analysis of its subject term, i.e., by "triangle" one does mean "a figure which has three sides." For a descriptive science this distinction does not exist; all propositions are synthetic. The truth of every proposition must be determined by an examination of the cases to which it refers. No predicate is "contained" in a subject because there are no logical relations of the kind which would be described in this way. The only way to determine whether a predicate is contained in a subject is to examine the referent of the subject and the referent of the predicate and decide whether they have the required relation. Since no proposition of descriptive science is analytic, any proposition may prove to be false, since it may assert a state of affairs which is found not to be the case. Analytic propositions cannot be shown to be false, since they prescribe the way in which symbols are to be united, and no state of affairs can contradict such a prescription. But in descriptive science no such legislative action can be taken, for it presumes more empirical knowledge than can be justified. To legislate over events supposes that one is able to predict the behavior of events, and this is just what descriptive science is not entitled to do. The structure of events determines the interrelation of symbols, the interrelation of symbols does not prescribe the structure of events.

Still another formulation of the same feature is the assertion that a descriptive science is relatively insensitive to shocks from the outside. Any science must permit the possibility of its own falsity; new discoveries may at any time require the revision of an established science. Descriptive science is especially favored in this respect. Being loosely integrated, its symbolic scheme permits a change at any one point without necessitating a change at another. Hence the discovery that any proposition is false requires only the substitution of a true proposition for the false one, but no modification in other propositions. On the other hand, the discovery of a false proposition in a highly integrated science constitutes a crisis, for the proposition is both determined by and determinative of other propositions, and the substitution of an alternative proposition requires a revision of all related propositions. It is not to be expected today that any discovery, say, in biological or sociological science would constitute a crisis for these sciences; the sciences themselves are not sufficiently integrated to necessitate an important and extensive revision should such occur. In other words, these sciences are continually adapting themselves to important discoveries, but the changes required are of a minor character and hence are not critical events.

(2) Descriptive science is non-explanatory. Unless this statement is interpreted it may amount to a mere identity, i.e., a statement that a descriptive science is descriptive. Ordinarily the distinction between description and explanation is formulated in terms of the respective questions which they are designed to answer. Description answers the question "How?" and explanation answers the question "Why?" Descriptive science, then, symbolizes the way in which events exhibit themselves and behave; explanatory science symbolizes other events (or hidden features of the events themselves) in terms of which this behavior is to be understood. A simple formulation of the problem is to say that explanatory science is concerned with the premises from which the propositions of descriptive science may be deduced as theorems. Hence when one explains he searches for reasons, grounds, or causes, and the relation between any such entities and the events which they explain is that of logical implication. It follows that a descriptive science cannot explain, for its structure is not logical. As was pointed out, its propositions are independent of one another, and therefore in no sense derivable from one another. Hence descriptive science cannot answer the question "Why?" but must confine its activity to the statement of the mere "How?" The laws of gravitation will not state why bodies fall, but how they fall; the principles of natural selection will not state why organic forms develop, but how they develop.

It follows that facts for descriptive science are simply brute facts. One knows that they exist, but he does not know why. Nature might have chosen to be otherwise. Every fact in descriptive science suggests its own alternative.

Water boils at 100° Centigrade, but it might just as well have boiled at 200° or 50°; bodies attract each other according to the inverse square law, but the inverse cube law or some other law might have been found to hold; objects expand when heated, but there is no reason why they should not have contracted. Every fact is an isolated fact, and receives nothing from other facts. Since everything which occurs has an alternative, anything is possible in nature. What one knows about nature does not enable him to predict as to the character of the unexplored parts. Science in this form has no anticipatory function. This does not mean that nature may offer surprises; surprises are impossible unless one has expectations, and descriptive science justifies no expectations. It means rather that one must take nature as he finds it, and that he must never force laws or concepts on events. Symbols are determined by events, not events by symbols.

Such are the features of descriptive or empirical science. Many would deny that such disciplines are properly sciences at all. A science, they insist, must contain hypotheses, theories, and conjectures; it must make predictions; it must experiment; it must attempt to get behind or beneath phenomena to discover their deeper features and their essential connections. Certainly, if one means by descriptive science a *pure* descriptive science, i.e., one which makes no reference to hypotheses, predictions, experiments, and hidden aspects of events, there are probably no such sciences in existence. But this fact does not destroy the value of the characterization presented in the preceding pages. For there are sciences which approximate to this ideal character, and which develop under the impetus of techniques which are directed to the realization of precisely such an ideal character. Though there may be no scientists who deny the inevitability of hypotheses, predictions, and the like in science, there are many who feel that hypotheses should be avoided wherever it is possible to do so and limited to a very minimum where it is necessary to introduce them, who feel that anticipations of nature are justified only on very rare occasions and only in very limited fields, who feel that experiments involve artificial situations and hence mislead, and who feel that the search for the hidden features and connections of things is bound to be more or less futile and not to be undertaken until the possibilities of their more obvious features and connections have been completely exhausted. If there are scientists who profess some such beliefs as these, there are sciences which represent their approximate embodiment. It is only in this sense that descriptive science may be claimed to exist. The next chapter will show that there are investigators who at least claim that this narrow conception of science is the true one.

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CHAPTER VIII

THEORIES OF SCIENTIFIC CONCEPTS

There has been lurking in the background of the discussion thus far an important problem, whose consideration can no longer be postponed. It lies at the very foundation of the logic of science, and determines not merely one's general theory of science, but also, at times, one's conception of the actual techniques of procedure within science itself. This basic problem must be examined without further delay.

All who choose to express their ideas on the question of the logical structure of science recognize that the task of science, at least in a very general sense, is the construction of a system of symbols which is presumed to be representative of such portion of the realm of events as lies within the subject matter of the particular science concerned. It is not the purpose here to discuss the possible ambiguities in the notion of "representation." The term may be used at this point to include such extreme views as, on the one hand, that of Bergson, who insists that what one gets through science is always a system of static and discrete forms which is quite inadequate to reveal a world which is enduring and continuous, and, on the other, that of the pragmatists, who argue that scientific symbols are merely instruments of practical adjustment whose value is to be measured rather in terms of their effectiveness than in terms of any sort of correspondence with events.

But within this general agreement there are the widest sorts of differences of opinion as to the way in which scientific symbols are to be defined, and the rôles which they play in the systems of symbols of which they are parts. Do scientific symbols describe or do they explain? Are they defined by physical operations, by logical constructions, or by conventions? Are such things as atoms merely conceptual shorthand, or are they existent events? What is the difference between such things as molecules which presumably could exist, and perfect levers which presumably could not exist? Such questions as these demand answer if an adequate logic of science is to be constructed.

A satisfactory classification of the various answers to these questions is very difficult to make, for it itself presupposes a general theory of the logic of science. The classification which is here proposed is offered merely as a convenient method for presenting the theories which are to be discussed in this chapter. It makes no claim to general adequacy. The names listed in connection with each of the positions are those of the outstanding recent and contemporary representatives. The names in italics designate those individuals whose views will be considered as illustrative of the general positions.

A. Positivisms
    1. Strict Empiricism: Mill, Mach, *Pearson*
    2. Operationalism: *Bridgman*
B. Modified Positivisms (Fictionalisms)
    1. Constructionalism: Russell, *Hobson*
    2. Conventionalism: Vaihinger, *Poincaré*
    3. Logical Positivism: Schlick, Wittgenstein, *Carnap*
C. Realisms
    Scientific Realism: Planck, Meyerson, Whitehead, *Bavink*

The principles underlying this table are as follows: Granting that the task of science is the construction of a system of symbols having a certain representative value toward the realm of events, the important problem is to determine in just what way the system has been built up. Presumably the simplest formulation of this task is to say that the scientist confronts himself with the data, sets into operation certain activities usually called "knowing" or "thinking," and ends with the required system of symbols. This formulation, at any rate, indicates clearly that the symbols are the end-result, and that their meanings are a function of two factors—the data and the knowing activities. Using S to designate any scientific symbol such as a concept or law, D to designate the data, and O to designate the knowing operations, the dependence of the symbol upon these two factors may be represented,

$$S = f(D, O).$$

Now the first problem is to determine the character of this functional relation. *To what extent is the meaning of the symbol a function of the data, and to what extent is it a function of the knowing operations?* *Positivism* insists that it is primarily a function of the data, and only incidentally a function of the operations; hence the operations may be neglected or relegated to a position of unimportance. All *modified positivisms* and all *realisms* insist that the meaning of a symbol is at least as significantly a function of the operations as it is of the data; hence the operations may not be neglected. All *rationalisms* insist that the meaning of a symbol is primarily a function of the operations, and only incidentally a function of the data; hence the data may be neglected. It is interesting to note that theories of science rarely take rationalistic form; they are, almost without exception, either positivistic in the extreme or modified form, or else realistic. Hence there will be no further occasion to refer to rationalistic positions.

But there is another problem. Though the ascertainment of the importance of the operation serves to differentiate pure positivisms, on the one hand, from modified positivisms and realisms, on the other, an additional consideration is required to distinguish modified positivisms from realisms. Both of these positions insist on the importance of the operations. *But whereas modified positivisms insist that the operations are essentially inventive, and hence result in symbols for entities which are not presumed to exist, realisms insist that the operations are essentially exploratory, and hence result in symbols for entities which are presumed to exist.*

As a guide to the more detailed expositions which are to follow immediately, it will be well here to indicate the general positions of the representatives of these various views. Pearson and Bridgman are both positivists in their insistence upon the importance of data and the unimportance of knowing. For Pearson science is the mere classification and shorthand description of the data; when the more highly imaginative activities resulting in symbols for atoms and molecules intervene the importance of these operations is minimized, for the resulting symbols are themselves simply *devices for classification and simplified description.* Bridgman's insistence on the importance of operations does not remove him from the positivist group, for the operations with which he is concerned are mainly *physical*, i.e., they are concerned with *getting the data, not with knowing them;* hence Bridgman is essentially a positivist.

Hobson, Poincaré, and Carnap are all modified positivists in their insistence that there are at least some symbols in science whose meanings are determined in an important sense by knowing operations, and in their conviction that the operations are in some sense arbitrary, i.e., inventive. Hence they all call attention to the fact that there are some symbols in science which do not "apply" in any direct sense to nature. For Hobson these are *abstractions*, obtained through selective acts and therefore idealizations of the data. For Poincaré they are *conventions*, having their origin in data but transformed so as to be in accord with certain more or less arbitrarily set up definitions. For Carnap they are *formal propositions*, having both meaning and truth determined by the structure of the language which one employs to symbolize them—a language which is, in fact, simply the result of certain agreed-upon rules for the use of symbols.

Bavink is a realist. He insists both upon the importance of the knowing operations, and upon the necessity for supposing that atoms and molecules are existent entities, not different in kind from stones and trees. "Our knowledge of reality is not a conceptual structure, forever suspended over things, and based solely on conventions and definitions created for the purpose of thought economy; it offers us a picture of reality, that is of an objective body of facts already existing before all knowledge; a picture which is continually becoming more and more adequate." [1]

In the discussion which follows, no attempt will be made to criticize the various theories presented. The positions are examined merely for the purpose of illustrating some of the main views as to the status of scientific concepts. For the more detailed understanding of the theories the reader is advised to turn to the authors themselves, whose main works are listed at the end of the chapter.

STRICT EMPIRICISM

In the light of the discussion in the preceding chapter, positivism may be easily explained. It maintains that the complete task of science is description; hence, descriptive science represents science in its maturity. Description represents not the beginning stage of science, but the final stage; science never goes beyond description. The position indicated in the table as "strict empiricism" represents the most rigid adherence to the positivistic thesis, and it will be examined first.

The task of science, argues Pearson, "consists in the careful and often laborious classification of facts, in the comparison of their relationships and sequences, and finally in the discovery by the aid of the disciplined imagination of a brief statement or *formula*, which in a few words resumes a wide range of facts. Such a formula . . . is termed a *scientific law*. The object served by the discovery of such laws is the economy of thought." [2] "The man who classifies facts of any kind whatever, who sees their mutual relation and describes their sequences, is applying the scientific method and is a man of science. . . . When every fact, every present or past phenomenon of the universe, every

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[1] *The Natural Sciences*, p. 246.

[2] *Grammar of Science*, pp. 77–78.

phase of present or past life therein, has been examined, classified, and coördinated with the rest, then the mission of science will be completed." [1] The essential instrument in this task is the *concept*, by which thought classifies impressions, "analyses or simplifies their characteristics, and forms general notions of properties and modes." [2] For example, the concept of an object is simply a convenient shorthand device for associating present sense-impressions with stored or memory impressions. All of science, in fact, is inspired by this spirit of economy. It is searching a brief description, a mental *résumé* of the universe. Pearson quotes with approval Kirchhoff's definition of mechanics: "Mechanics is the science of motion; we define as its object the complete *description* in the *simplest* possible manner of such motions as occur in nature." [3]

But what can be said about such things as atoms, molecules, geometrical surfaces, particles, and absolute rigidity? Science contains symbols for such entities, and every logic of science must account for them. Pearson's answer is clear. In the process of constructing concepts to express in simplified form the relationships and sequences of phenomena "we often analyse the material of sense-impressions into elements which are not themselves capable of forming distinct sense-impressions; we reach conceptions which are not capable of direct verification by the senses; that is to say, we can never, or at least we cannot at present, assert that the elements have objective reality. Thus physicists reduce the groups of sense-impressions which we term material substances to the elements *molecule* and *atom*, and discuss the motion of these elements, which have never been, and perhaps never can become, direct sense-impressions. No physicist ever saw or felt an individual atom." [4] Hence there is no doubt that there are at least *symbols* of this kind in science.

But what is their function? Here, again, Pearson is quite clear. They do not explain, for in science there is no ex-

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[1] *Ibid.*, pp. 12–13. [2] *Ibid.*, p. 46. [3] *Ibid.*, p. 115. [4] *Ibid.*, p. 95.

planation. "The law of gravitation is a brief description of *how* every particle of matter in the universe is altering its motion with reference to every other particle. It does not tell us *why* particles thus move; it does not tell us *why* the earth describes a certain curve around the sun. . . . Such laws simply *describe*, they never *explain* the routine of our perceptions." [1] Even in causal and mechanistic explanations the necessity "is no categorical *must*, it is the descriptive *how* of the formula, the mere summary of what has been observed, the inexplicable routine." [2] "For science, cause, as originating or enforcing a particular sequence of perceptions is meaningless—we have no experience of anything which originates or enforces something else. Cause, however, used to mark a stage in a routine, is a clear and valuable conception, which throws the idea of cause entirely into the field of sense-impressions, into the sphere where we can reason and can reach knowledge." [3]

The function of such concepts in science is therefore not to explain but to describe. "Atom and molecule are intellectual conceptions by the aid of which physicists classify phenomena and formulate the relationships of their sequences." [4] "The fundamental conceptions of geometry are only ideal symbols which enable us to form an approximate, but in no sense absolute, analysis of our sense-impressions. They are the scientific shorthand by which we describe, classify, and formulate the characteristics of that mode of perception which we term perceptual space." [5] It seems safe to say that by such concepts as these we *mean* simply the sense-impressions which they serve to classify. Hence the imaginative operations which call such symbols into being prove to be nothing more than techniques of classification.

It follows that we must not ascribe to symbols which are merely simplifying devices any phenomenal existence other than that of the sense-impressions which they classify. "*To*

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[1] *Ibid.*, p. 99.

[2] *Ibid.*, pp. 126–127.

[3] *Ibid.*, pp. 128–129.

[4] *Ibid.*, p. 95.

[5] *Ibid.*, pp. 198–199.

no concept, however invaluable it may be as a means of describing the routine of perceptions, ought phenomenal existence to be ascribed until its perceptual equivalent has been actually disclosed."[1] "Science takes the universe of perceptions as it finds it, and endeavors briefly to describe it. It asserts no perceptual reality for its own shorthand."[2]

Such a position is definitely positivistic. It defines clearly the realm about which the scientist is permitted to talk—the realm of sense-impressions. These alone can be said to be given clearly. The task of science with reference to these entities is that of mere classification, ordering, and correlating. Any symbols which presume to refer to entities not directly observable must be recognized for what they are, viz., merely devices for classification, ordering, and correlating. Science does not explain, but describes.

OPERATIONALISM

A somewhat more recent form of positivism is that of P. W. Bridgman. This physicist's starting-point is the recognition of two facts: the presence of discontinuities in the realm of experience, and the necessity for a pure empiricism. "The first lesson of our recent experience with relativity is merely an intensification and emphasis of the lesson which all past experience has also taught, namely, that when experiment is pushed into new domains, we must be prepared for new facts, of an entirely different character from those of our former experience."[3] We must not be surprised, therefore, if we find that the concepts defined in terms of the "middle-sized" objects of ordinary experience fail of application, or become meaningless when extended to the range of cosmic phenomena on the one hand, or to microscopic phenomena on the other. "As we approach the experimentally attainable limit, concepts lose their individuality, fuse together, and become fewer in number."[4] Heat, for example, does not apply to molecules, nor do space

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[1] *Ibid.*, p. 277 (italics are the author's).

[2] *Ibid.*, p. 208.

[3] *Logic of Modern Physics* (New York: Macmillan, 1927), p. 2.

[4] *Ibid.*, p. 24.

and time and the laws of gravitation apply to small bodies; electrical laws and the principles for the ascertainment of length through the application of meter sticks do not fit when the phenomena are cosmical. But since progress in physics consists in the gradual expansion of the known into the realm of border-line events, we must expect that our conceptual schemes will be subjected to shocks of varying degrees of intensity, to the extent to which we have not modified them in favor of the probable novelties which they will be called upon to explain.

The second fact to be recognized is the necessity for an empirical attitude in physics. The physicist "recognizes no *a priori* principles which determine or limit the possibilities of new experience. Experience is determined only by experience. This practically means that we must give up the demand that all nature be embraced in any formula, either simple or complicated."[1] And it also means that concepts, which are the sole instrument for exploring experience, should themselves be empirically defined. "Concepts can be defined only in the range of actual experiment, and are undefined and meaningless in regions as yet untouched by experiment. It follows that strictly speaking we cannot make statements at all about regions as yet untouched, and that when we do make such statements, as we inevitably shall, we are making a conventionalized extrapolation, of the looseness of which we must be fully conscious, and the justification of which is in the experiment of the future."[2]

The safest procedure, in the face of such a situation, is to adopt an operational theory for the definition of concepts. Concepts must be defined in terms of the processes by which the objects in question are found. "We may illustrate by considering the concept of length: what do we mean by the length of an object? We evidently know what we mean by length if we can tell what the length of any and every object is, and for the physicist nothing more is

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[1] *Ibid.*, p. 3.

[2] *Ibid.*, p. 7.

required. To find the length of an object, we have to perform certain physical operations. The concept of length is therefore fixed when the operations by which length is measured are fixed: that is, the concept of length involves as much as and nothing more than the set of operations by which length is determined. In general, we mean by any concept nothing more than a set of operations; *the concept is synonymous with the corresponding set of operations*." [1] Bridgman illustrates further by pointing out that the word "length" means something different according as it measures a concrete object such as a house, a moving street car, a very large object (for which a theodolite is required), the distance from the earth to the moon, and the distance between the planes of atoms in a certain crystal. Nothing but confusion can result if we suppose that "length" in each of these cases means the same thing, for the operations employed in each case are different, and the operations employed in one realm are in general meaningless if applied in another.

The result is that "if we deal with phenomena outside the domain in which we originally defined our concepts, we may find physical hindrances to performing the operations of the original definition, so that the original operations have to be replaced by others; . . . but we must recognize in principle that in changing the operations we have really changed the concept, and that to use the same name for these different concepts over the entire range is dictated only by considerations of convenience, which may sometimes prove to have been purchased at too high a price in terms of unambiguity." [2] Our procedure, when we find ourselves confronted with a new range of phenomena, should be somewhat as follows: We should "start by applying the older concept until it got us into trouble; after it had got us into trouble often enough we would begin to find some sort of regularity in the way in which the trouble occurred, just exactly like the rat trying to get out of a maze and running

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[1] *Ibid.*, p. 5 (italics are the author's).

[2] *Ibid.*, p. 23.

his nose into the end of a blank passage, and we would then formulate a set of verbal rules, instructing us at a certain stage of operations to switch our procedure and begin operating in a new way which we had found appropriate by the method of trial and error."[1]

The position here defined is clearly positivistic. It insists that concepts be defined in terms of data which are themselves operationally determined, and that the mind proceed very cautiously when it attempts to explore the realm of less clearly given objects. "Since our concepts are constructed of operations, all our knowledge must inescapably be relative to the operations selected."[2] Hence we cannot impose a concept defined in one realm upon another; this expresses the attitude of caution which is characteristic of positivism. Furthermore, a large number of our so-called concepts prove to be pseudo-concepts because they are, strictly speaking, meaningless. "If a specific question has meaning, it must be possible to find operations by which an answer may be given to it. It will be found in many cases that the operations cannot exist, and the question therefore had no meaning. For instance, it means nothing to ask whether a star is at rest or not."[3]

Both Pearson and Bridgman, therefore, insist upon the basic importance of data. For Pearson the data are merely given; for Bridgman they must often be produced operationally, and, having been so obtained, take on their operational origin as part of their content. For both writers, symbols are simply classificatory and descriptive. Pearson insists upon this fact even in connection with such apparently remote notions as atoms; Bridgman argues that concepts must be kept empirical by defining them in terms of their proper range of application, and by re-defining them in case they are to be applied to novel spheres. It seems safe to characterize both positions as positivisms, though one must admit that Bridgman's variety is somewhat less nar-

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[1] *Nature of Physical Theory* (Princeton: Princeton University, 1936), p. 28.

[2] *Logic of Modern Physics*, p. 25.

[3] *Ibid.*, p. 28.

rowly conceived than Pearson's. For this reason Bridgman's position offers a convenient transition to the consideration of the modified positivisms or fictionalisms.

CONSTRUCTIONALISM

The term "constructionalism" has been employed in the table to characterize, perhaps not very aptly, a large number of theories which are not sharply differentiated from positivism of the type exhibited by Pearson. The common feature of such positions is the more or less conscious recognition that "knowing makes a difference." They are distinguished from the more "pure" positivisms in their insistence that the knower has something more to do in science than to record in shorthand the succession of sense-impressions. They recognize that the knower is unavoidably active, and in his very attempt to record introduces in spite of himself changes of a significant character. They maintain, therefore, that representation usually involves construction—a term which may be here used to describe a variety of mental acts such as abstraction, idealization, serial extension, and association. The presumption is that these operations are all of a more significant character than mere classification and description, hence the task of science cannot be formulated merely in terms of these latter processes.

Though E. W. Hobson considers himself to have adopted a position similar to that of Pearson, there are outstanding differences. Most important of these is Hobson's insistence that the mental operations employed in building up a conceptual scheme involve more than classification. These operations are variously described as "constructive," "abstractive," "synthetic," "selective," and "idealizing." Laws are not discovered, but constructed. "The discovery, or rather construction, of a scientific law involves that synthetic activity of thought which manifests itself in a constructive process in which actual percepts are employed only as the raw material and starting point of the mental process."[1] Certain concepts of science have no direct perceptual counterparts; "they are formed by an effort of constructive imagination, for the purposes of the representative scheme . . . they must be regarded, at least provisionally, as purely ideal elements of the scheme, in fact as auxiliaries necessary for the purpose of the formation of a self-contained conceptual scheme which shall serve its purpose of providing a sufficient mode of representation in thought of the particular domain of physical events and objects."[2]

Hobson also differs from Pearson in his conception of the "representative" function of a conceptual scheme. He does not believe that science merely describes, in Pearson's limited use of that word. The possibility of setting up a conceptual scheme "has actual physical experience as its essential condition, but the constructive and generalizing work of thought is no less essential. The original function of such a scientific theory, or conceptual scheme, is to provide an ideal representation of some more or less restricted range of physical phenomena as actually observed, that is of certain sequences and regularities in percepts. But the functions of a conceptual scheme are much wider than those of merely describing symbolically what has actually been observed. The scheme is applied hypothetically to predict what will be observed in circumstances which differ in some degree, or in some characteristics, from those in which the experiments or observations which led up to the theory were made. The value and the range of validity of the particular conceptual scheme have to be estimated by its actual success in the fulfillment of this function of prediction."[3]

Yet Hobson is not to be considered a realist. He disagrees sharply with Whitehead in the latter's insistence that molecules and electrons are natural objects.[4] "Whatever else they are, molecules and electrons are concepts . . . they are concepts in scientific theories, and the only question

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[1] *Domain of Natural Science* (New York: Macmillan, 1923), p. 28.

[2] *Ibid.*, p. 32.

[3] *Ibid.*, p. 31.

[4] See above, Chapter VII, p. 133.

about a scientific theory is . . . whether it is logically coherent and how far it is adequate for the purpose of representation. . . . Natural Science postulates, as a working hypothesis, only that the perceptual complex is such that tracts of it are capable of conceptual description by scientific schemes. It does not require any postulate as to detailed systems of relations or of entities within that perceptual complex, or within any supposed reality behind that complex, which shall account for the fact that the working hypothesis has proved successful." [1] It is unnecessary for the purposes of Natural Science "to make the assumption that a single law has a precise correspondence with a single definite set of relations which actually subsist in nature." [2]

Hobson's position can be characterized, therefore, as a modified positivism, which insists that data are only the raw material of knowledge. These data must be worked over by an active mind and transformed into idealized symbols, which are then combined into symbolic schemes. Many elements of such schemes have lost all direct correspondence with the data from which they were derived, hence the system as a whole has no immediate representative value. It is therefore no longer descriptive, but is considered as "applicable" in a somewhat less direct sense. In addition to applicability it must have internal consistency, relative simplicity, and predictive value. It is the task of science to construct a system of symbols of this kind.

CONVENTIONALISM

Conventionalism may be described as a more extreme constructionalism. It is distinguished from the latter in its recognition of the increased importance of the knowing operations. If scientific concepts are merely constructed out of the raw material given in perception, they are still related to it in the sense that they have been derived by describable techniques; abstraction and idealization are operations of transformation, but one can, by retracing the

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[1] *Ibid.*, p. 59.

[2] *Ibid.*, p. 26.

routes, recover the events which were thus operated upon; hence there is some justification for saying that scientific concepts still have applicability to events. But in conventionalism this applicability has been more or less completely lost. One does not, strictly speaking, *derive* his concepts at all. He is tempted to say that he *creates* them. Yet this is not quite true; conventionalism does not reduce to nominalism. It insists that scientific symbols of this particular kind are suggested by the data, but that they are given meaning through elaborate mental operations. The extent and importance of these operations are demonstrated by the fact that the symbols thus devised can no longer be spoken of as true or false, but must be characterized as convenient or inconvenient.

Quotations from Henri Poincaré will illustrate the particular way in which he develops this position. While he admits that there are to be found in science not only empirical laws, i.e., descriptive generalizations, but also hypotheses in the ordinary sense, i.e., imaginative notions which can be verified and falsified, he nevertheless insists that the most important kinds of symbol are those which—mistakenly called "hypotheses"—are more properly characterized as "disguised definitions," or "conventions." The best examples of them are to be found in the principles of geometry and of mechanics. They are like laws in that they have their foundation in experience, but they are different from laws in that they are not dictated by data. "We see that experience plays an indispensable rôle in the genesis of geometry; but it would be an error thence to conclude that geometry is, even in part, an experimental science. . . . If it were experimental . . . geometry would be only the study of the movements of solids; but in reality it is not occupied with natural solids, but has for object certain ideal solids, absolutely rigid, which are only a simplified and very remote image of natural solids. . . . Experience guides us in this choice without forcing it upon us." [1] In the same

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[1] *Foundations of Science* (New York: Science Press, 1921), p. 79. This is his *Science and Hypothesis, Value of Science,* and *Science and Method* bound in a single volume.

way the principles of mechanics, though suggested by experimental data, describe not these data but idealized entities, such as perfectly isolated bodies.

But if the meaning and truth of these principles are not determined completely by data, how are they determined? Poincaré's answer is definite. They are determined by *convenience*, i.e., *the principles are defined in such a way as to make them most convenient.* "*The axioms of geometry . . . are conventions;* our choice among all possible conventions is *guided* by experimental facts; but it remains *free* and is limited only by the necessity of avoiding all contradiction. Thus it is that the postulates can remain *rigorously* true even though the experimental laws which have determined their adoption are only approximative. In other words, *the axioms of geometry . . . are merely disguised definitions.*" [1] The same description applies to the principles of mechanics. They have been drawn from experimental laws but "have, so to speak, been exalted into principles to which our mind attributes an absolute value." [2]

Unfortunately Poincaré has not told us precisely what is meant by a convention. Presumably a convenient symbol would be one which is useful, and he tells us that a useful science is one which enables us to foresee,[3] hence it would seem legitimate to conclude that the principles of geometry and mechanics are convenient because they enable us to make predictions which are later verified. If this is the proper interpretation of Poincaré, his position reduces in this respect to Hobson's. But he would disagree with Hobson in the claim that principles of this kind should be called true. "It is just as unreasonable to inquire whether they are true or false as to ask whether the metric system is true or false." [4] Definitions are not properly true or false, they are only convenient.

The exact nature of this transition from a law into a principle must be exhibited if one is to understand fully

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[1] *Ibid.*, p. 65 (italics are the author's).

[2] *Ibid.*, p. 125.

[3] *Ibid.*, p. 324.

[4] *Ibid.*, p. 124.

Poincaré's position. If we have two bodies A and B, we may formulate their relations in a law. But these relations will be extremely complicated, hence we are led to suppose two ideal bodies A' and B' whose ideal relations will be described by the principles of geometry. "The relation between A and B was a rough law, and was broken up; we now have two laws which express the relations of A and A', of B and B', and a principle which expresses that of A' and B'."[1] But the principle is absolutely true in the intensional sense, because it has been made so by definition, i.e., we define A' and B' in such a way as to make the principle necessarily true. But the principle cannot be true in the empirical or extensional sense, for it is not applicable to anything empirical. Hence we describe it as convenient.

The net result of Poincaré's view is somewhat as follows: There are to be found in science symbols of a peculiar sort called "conventions." These are not merely classificatory in Pearson's sense, nor are they simply abstractions in Hobson's sense. On the contrary, they are derived by elaborately creative processes which free them almost completely from the data. Thus, in contrast with strict positivism, the symbols have their meanings determined primarily by the knowing operations and only incidentally by the data. The creative operations, however, are performed with a definite end in view; the symbols thus produced must be convenient for science as a whole, i.e., they must be such as to permit valid predictions. Hence the symbols—which are at this stage essentially "empty" of meaning—are operationally defined in such a way as to make the predictions possible. The creative knowing processes are precisely these operational definitions. The symbols, once defined in this way, cease to be true or false in the empirical sense, and become only convenient. "*The scientific fact* [the convention] *is only the crude fact* [the data] *translated into a convenient language*."[2]

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[1] *Ibid.*, p. 336.

[2] *Ibid.*, p. 330.

LOGICAL POSITIVISM

The placing of logical positivism [1] in the context of conventionalism and constructionalism may strike one as somewhat unusual. Yet there is an important sense, it seems, in which this position has close affiliations with these modified positivisms. It certainly is positivistic in its high esteem for Mach, in its insistence that every statement of science "should be based on and reducible to statements of empirical observations," [2] and in its definite rejection of the *a priori*. But it cannot be a strict positivism for it recognizes the existence of certain types of statement which play a part in science and yet are not empirically verifiable. One thinks immediately of Hobson's idealizations in this connection, until he is told that these statements are incapable of truth or falsity in the ordinary sense of the term, and hence cannot be mere idealizations of the data. One then suspects that they are simply Poincaré's conventions; but this proves also to be an erroneous interpretation, for they are strictly without content or meaning—purely formal statements which say nothing and therefore could not be even convenient or inconvenient. Hence they are symbols which have so completely lost their reference to data that the latter do not function at all in the determination of the character of the symbols. Are the symbols, then, purely arbitrary? No, they are subject to the rules of language—rules according to which one constructs complex symbols out of simple symbols, and translates symbols into other symbols. But if language itself is merely a device which one employs when he knows—an operator, so to speak—then the purely formal symbols of the logical positivists are descriptive of this device. They are symbols which describe the operations of knowing rather than the data known. They are the values which S takes in the formula $S = f(D, O)$, when D becomes zero and O becomes the

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[1] See Chapter I, pp. 11–13.

[2] R. Carnap, *The Unity of Science* (London: Kegan Paul, 1934), p. 27.

complex body of operations associated with the invention and use of language.

The justification for this interpretation of logical positivism may be found in the writings of Carnap. Attention must be called, first, to the more positivistic aspects of his system. "Science is a system of statements based on direct experience, and controlled by experimental verification. . . . Verification is based upon 'protocol statements,' a term . . . understood to include statements belonging to the basic protocol or direct record of a scientist's (say a physicist's or a psychologist's) experience. Implied in this notion is a simplification of actual scientific procedure as if all experiences, perceptions, and feelings, thoughts, etc., in everyday life as well as in the laboratory, were first recorded in writing as 'protocol' to provide the raw material for a subsequent organization." [1] A protocol statement would therefore describe what Pearson called sense-impressions, and what we have called in the course of this discussion the most clearly given data. Statements in the scientific language which are not protocol statements, can, in general, be verified by the technique of deducing from them statements which are protocol. This is the indirect method of verification which, for Hobson, constitutes the test of the applicability of idealizations and abstractions.

But science, in the broadest sense of the word, contains more than this. "Scientific research may be concerned with the empirical *content* of theorems, by experiment, observation, by the classification and organization of empirical material; or again it may be concerned with establishing the *form* of scientific statements, either without regard for content (formal logic) or else with a view to establishing logical connections between certain specific concepts." [2] Every scientific statement, in other words, may be considered either in its "material" mode, i.e., as asserting about "objects," and "states of affairs," or in its "formal" mode, i.e., as referring only to linguistic forms. To logic and

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[1] *Ibid.*, pp. 42–43.

[2] *Ibid.*, p. 33.

mathematics are assigned the analysis of the "formal" mode of statements. Such statements "are tautologies, analytic propositions, certified on account of their form alone. They have no content, that is to say, assert nothing as to the occurrence or non-occurrence of some state of affairs. If to the statement: '*The* (*thing*) *A is black*' we add '*or A is blue*' the supplemented statement still conveys some information though less than at first. If, however, we replace the supplementary phrase previously chosen by '*or A is not black*' the compound statement no longer conveys any information at all. It is a tautology, i.e., is verified by *all* circumstances." [1]

But to consider a proposition formally is to consider it linguistically. "We will call 'formal' such considerations or assertions concerning a linguistic expression as are without any reference to sense or meaning." [2] By a language is meant the "*system of the rules of speaking.* Such a language-system consists of two kinds of rules, which we will call formation rules and transformation rules. The formation rules of a certain language-system *S* determine how *sentences* of the system *S* can be constructed out of the different kinds of symbols." [3] "The transformation rules . . . determine how given sentences may be transformed into others; in other words: how from given sentences we may *infer* others." [4] "Given any language-system, or set of formation rules and transformation rules, among the sentences of this language there will be true and false sentences. But we cannot define the terms 'true' and 'false' in syntax, because whether a given sentence is true or false will generally depend not only upon the syntactical form of the sentence, but also upon experience; that is to say, upon something extra-linguistic." [5] Formal propositions, however, are true or false simply by virtue of the rules of language.

These statements seem to justify an interpretation of Carnap's position which affiliates him rather closely with

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[1] *Ibid.*, pp. 33–34.

[2] R. Carnap, *Philosophy and Logical Syntax* (London: Kegan Paul, 1935), p. 39.

[3] *Ibid.*, p. 41. [4] *Ibid.*, p. 43. [5] *Ibid.*, p. 48.

the more traditional positivists such as Hobson and Pearson. Carnap's formal sentences are those types of symbols which are determined simply linguistically. But language itself is simply a technique of knowing, a set of rules formulated and agreed upon by which symbolic devices may be constructed and transformed. Hence one may say that formal symbols are such that their meaning and truth—in the only sense in which they can be said to have meaning and truth—are determined *rather by the methods of knowing*, i.e., *the operations*, *than by that which is known*, i.e., *the data*. They have no content—except linguistic content. They do not therefore make assertions—except about the linguistic techniques which are employed in all knowing. It seems safe to say, therefore, that Carnap's position represents an extreme in the class of what were characterized at the beginning of the chapter as "modified positivisms." In his case, however, the symbols which are not directly descriptive cease to be even indirectly descriptive; they are exhaustively determined by the knowing operations concerned with the formation of symbols, and not determined at all by data.

SCIENTIFIC REALISM [1]

Common to all positivisms, both of the strict type and of the modified type, is the attitude which Bavink calls "hypotheseophobia." For the purer forms of positivism this attitude expresses itself in a definite rejection of hypothetical and theoretical entities of all kinds except in so far as they serve merely as classificatory concepts. Mach expresses the hope that atoms will finally disappear from the theory of heat. Modified positivism admits such entities, but somewhat reluctantly; it insists that they have only a conceptual status and are therefore to be called constructs, conventions, and fictions. Characteristic among such beliefs is that of Vaihinger, who insists that the proper attitude of the scientist is one of "as if"; atoms do not exist, but nature behaves just "as if" they did.

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[1] This is not, of course, to be confused with "scientific realism" as a theory of perception, developed in Chapter V.

Scientific realism insists that this view is not plausible. The words of Bavink indicate clearly the attitude of the realist. "The truth which we state in opposition to this excessive criticism of hypotheses is this, that precisely those elements of the 'explanatory hypothesis' found by hypothetical and speculative means form the truest and most valuable contributions to knowledge made by science. Far from being mere aids to work, pictures, models or the like, they are in truth the most important matter, and contain exactly that around which the whole of investigation turns." [1] "*Atoms are just as real things as cannon balls or grains of sand, as waves on water or mountains.*" [2] Reasons can be given "which have forced present day physics and chemistry to recognize *the real existence of atoms and molecules* as an undoubted fact." [3] "The whole question of the physics of today turns about the question: What is the atom? No one asks any longer: in what respects do things behave 'as if' they consisted of atoms?" [4]

Bavink develops his position by pointing out two important kinds of theories. First there are those which may be called "elaborative." "Their characteristic is that they contain practically no hypothetical elements; the fundamental assumptions in them . . . are themselves data of experience. . . . If the word theory is understood as the logical arrangement of a large number of single laws to form a closed system of reasons and consequences, these examples from physics are without doubt patterns for such theories. Even philosophers who are not positivists will have no objection to make to this statement." [5] But there is another class of theories called "explanatory," and illustrated by the atomic theory. "The characteristic of this kind of theory is that the desired logical connection and unification of the facts is only reached on the basis of a speculative assumption, which is described as the hypothesis upon which the theory is based." [6] "*A physical hypothesis*

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[1] *The Natural Sciences*, p. 38.

[2] *Ibid.*, p. 29.

[3] *Ibid.*, p. 19.

[4] *Ibid.*, pp. 37–38.

[5] *Ibid.*, p. 34.

[6] *Ibid.*, p. 34.

is the presumption of the existence of a general state of affairs, lying at the back of certain phenomena which are matters of experience, and allowing the phenomena in the field of facts in question to be deduced qualitatively and quantitatively (mathematically) from the presence of the said state of affairs and its assumed laws." [1] This is "a supposition which was arrived at by purely speculative methods,"[2] but it requires the physicist to admit that "molecules and light waves, fields and their tensors, have just the same kind of reality as stones and trees, plant cells or fixed stars." [3]

Some of Bavink's statements lead one to believe that he would insist on this realistic attitude not only toward hypotheses but also toward all idealizations and even toward concepts themselves. In his criticism of conventionalism, for example, he argues that idealizations cannot be the result of arbitrary acts of mind, for "both the matter and the manner of neglect is completely dictated to us by the object; . . . *the world is so constructed, that it can be known, by means of rational concepts, in rational judgments (laws) of increasing approximation, and it prescribes the path of these approximations to our understanding.*" [4] "Even concepts such as plant and animal, or oak or beech, have not been put into nature by us or invented in order to make it more easy for us to grasp; they are forced upon us by an objective something that was there long before men with power of conceptual thought existed." [5]

Such quotations as these indicate the legitimacy of asserting—as was suggested early in the chapter—that for realists the knowing operations are operations of discovery or of exploration rather than of construction or invention. When we look at cells through a microscope "we do not see the cells directly in the literal sense. However, no one seriously doubts that the picture seen in the microscope is in general similar to reality, and even geometrically similar, if we leave distortion out of account, which itself can be exactly calculated. . . . In principle, Rutherford is doing

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[1] *Ibid.*, p. 42. [2] *Ibid.*, p. 35. [3] *Ibid.*, p. 243. [4] *Ibid.*, p. 233. [5] *Ibid.*, p. 234.

no more than this when he interposes between the flying atom and the eye a somewhat more complicated apparatus, consisting of an ionisation chamber together with a galvanometer; only in this case the way is somewhat longer. *All investigation of nature depends upon an enlargement of our senses of this description.*"[1] Theories, therefore, are, like microscopes, instruments for exploring nature; consequently, both the mental operation required in devising and using a theory, and the end-result of such an operation must be properly interpreted; the operation is not one of invention but simply one of exploration, and the result of the operation is not a creation but simply a datum.

The contemporary scene in the philosophy of science is characterized by more or less heated controversies between the exponents of the various theories discussed in this chapter. Of these conflicts the most sharply defined is that between the positivists and the realists. The antagonism between the extreme and the moderate positivists cannot become intense, for the differences between the two positions are essentially relative. The absence of rationalistic theories of scientific concepts, to which reference was made earlier in the chapter, is, perhaps, not surprising; science is so obviously empirical in spirit and method that a rationalistic explanation seems hardly plausible. Yet it is not altogether untrue to say that constructionalism, conventionalism, and logical positivism are the rationalisms of modern scientific theory. All of these positions deny the necessity for any conception of the *a priori;* yet it is significant that in these theories the knowing operations function essentially in the place of the *a priori*. The knowing operations—abstraction, idealization, definition, linguistic invention and manipulation—are not, according to the upholders of these points of view, themselves determined by the character of the data; they are imposed by the mind and become techniques by which the knowing activities are fostered and developed. They function, therefore, as something which must be given,

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[1] *Ibid.*, pp. 28–29.

not as part of the data, but along with the data, if knowledge as a whole is to be understood. It is not asserted that man is born with any concepts or ideas, hence the theories are not *a priori* in this sense of the phrase; but man *is* born with certain capacities for abstracting, idealizing, defining, and inventing and manipulating symbols. These capacities are not part of the data, but part of the knower, and they contribute to the final form of knowledge in much the way that the traditional *a priori* did. This is not, of course, a condemnation of the positions; it is merely an attempt to throw them in their proper light.

It was the task of this chapter to show, not only that a strict positivism is possible, but that there are alternative theories of science. All of these theories insist upon the necessity for going beyond obviously given data. Whether the movement is considered to be a matter of invention or a matter of discovery is of no importance here. What must be emphasized is that if science does go beyond data, it must do so by means of some sort of technique. The next chapter will attempt to determine what this technique is.

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CHAPTER IX

SCIENTIFIC DISCOVERY

At the very core of the logic of science lies the problem of scientific discovery. The status of the problem today is well indicated by its formulation as "the mystery of scientific discovery." That the scientist makes important discoveries and that these discoveries, in fact, constitute the essential motivating factor in the scientific method are truths which are well recognized. But the conditions of the occurrence of the act, the "rules" according to which it takes place, the factors in the scientific personality which are responsible for it—all of these are either recognized to be insoluble problems or else explained by reduction to something equally mysterious, viz., scientific genius.

Certain refinements in the formulation of the problem may help to place it in the proper perspective. In the first place, the *discovery* which is usually referred to in this connection is of *hypotheses* rather than of *data*. This is not to say that the scientist does not discover facts. But the discovery of facts occurs in science in two important ways—prior to the formulation of hypotheses, and after such formulation. In the former type, the discovery is primarily due to the data rather than to the scientist; the data impress themselves upon his attention because of their obviousness, or because of their intensity, or because of their unusual character, or because of some other more or less accidental factor. But in the latter type, the data are discovered through the instrumentality of the theory; because the scientist has, through freely creative activities of the imagination, devised a theory having certain definitely predictable consequences, he is able to anticipate what nature will reveal in certain definitely specified localities. He then turns his observation to these areas and discovers what occurs. But the data thus discovered might have remained unknown had it not been for the directed attention; hence their discovery is a result of the theoretical activity of the scientist. For this reason the real problem in this connection centers about the discovery of the hypothetical and theoretical notions which are to serve as the guiding factors in observation. The mystery in science is not how one discovers facts which are obvious, but how one discovers theories which in turn enable him to discover facts which are not obvious.

In the second place, for the descriptive view of science there is no problem of scientific discovery. Hence the introduction of this chapter into the discussion implies that science is not merely positivistic. The reason for this can be seen in the considerations of the preceding chapter. For strict positivism science does not anticipate nature through imaginative devices; instead it merely records and classifies what is directly given. For modified positivism science does anticipate nature through such techniques, though it looks upon them merely as working instruments. For realism science definitely posits entities "behind" the data in terms of which the latter are to be explained and through which they are to be supplemented. Now it is, perhaps, a more or less arbitrary question as to just where in this continuum *explanation* enters; the positivists deny that they explain, the realists insist that they explain, the modified positivists maintain that they explain only in a carefully specifiable sense. But it is clear that the admission of significant acts of mind—be they concerned with the formation of constructs, conventions, or language—involves more than mere classification and correlation of data. Hence a recognition of the presence of acts of mind, i.e., movements of scientific discovery, is equivalent to the admission that science is more than descriptive.

In the third place, the formulation of the problem in terms of *discovery* rather than *invention* should not be taken as prejudicial to the positivism-realism controversy. As was suggested in the preceding chapter, modified positivism insists that hypotheses are invented, while realism maintains that they are discovered. The essential difference seems to be that invented entities do not exist prior to the act of invention, and hence seem to exist only because of the act; but discovered entities do exist prior to the act of discovery, and hence do not seem to exist merely because of the act. However, though this may be a genuine distinction, it is not relevant here; so long as one confines his attention to the mental operations by which the symbols for such entities are called into consciousness, the question as to whether or not anything corresponds to the symbols is unimportant. That the invented or discovered entities exist first merely as concepts would be granted by both modified positivists and realists; differences arise only when the further question of the existence of the referents of the concepts is raised. Hence one may formulate the problem here, indifferently, as the problem of scientific discovery or as the problem of scientific invention.

ACT OF DISCOVERY

The generally creative character of the act, and its intimate association with the fact of imagination, have been well recognized. "The physicist is bound, by the very nature of the task in hand, to use his imaginative faculties at the very first step he takes. For the first stage of his work must be to take the results furnished by a series of experimental measurements and try to organize these under one law. That is to say, he must select according to a plan which will in the first instance be hypothetical and therefore a construction of the imagination. And when he finds that the given results will not fit into one plan he discards it and tries another. This means that his imaginative powers must always be speculating on the significance of the data which have been furnished through experimental measurements." [1] Faraday, whose researches are responsible for much of the

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[1] M. Planck, *Where Is Science Going?*, pp. 86–87.

modern theory of electricity, acknowledges that he "was a very lively, imaginative person, and could believe in the 'Arabian Nights' as easily as in the 'Encyclopedia.'"[1] Tyndall's "Essay on the Scientific Use of the Imagination"[2] has become the classic exposition of this aspect of scientific discovery.

Another feature of the act which has been called to attention is the unusual circumstances in which it seems to occur. The history of science abounds in examples, many of them doubtlessly spurious, illustrating the spontaneity of the flash of insight: Archimedes discovering the principle of specific gravity while bathing; Newton discovering the principle of gravitation on seeing an apple fall from a tree; Watt discovering the principle of the steam engine while watching the tea kettle on the stove; Poincaré discovering the fuchsian functions while boarding an omnibus; Gauss discovering the law of induction at 7 o'clock one morning before arising; and so on. Hamilton's formulation of his discovery of the Quaternions may well be quoted as descriptive of a typical act of this kind. He says of these important mathematical concepts, "They started into life, or light, full-grown, on the 16th of October, 1843, as I was walking with Lady Hamilton to Dublin, and came up to Brougham Bridge. That is to say, I then and there felt the galvanic circuit of thought *closed*, and the sparks which fell from it were the *fundamental equations between* I, J, K; *exactly such* as I have used them ever since. I pulled out, on the spot, a pocket-book, which still exists, and made an entry, on which, *at the very moment*, I felt that it might be worth my while to expend the labours of at least ten (or it might be fifteen) years to come. But then it is fair to say that this was because I felt a *problem* to have been at that moment *solved*, an intellectual *want relieved*, which had *haunted* me for at least *fifteen years before*."[3]

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[1] J. H. Gladstone, *Michael Faraday* (London: Macmillan, 1873), p. 89.

[2] J. Tyndall, *Fragments of Science* (London: Longmans, Green, 1871), Vol. II.

[3] *North British Review*, Vol. XIV, p. 57; quoted by G. Gore, *Art of Scientific Discovery* (London: Longmans, Green, 1871), p. 366.

Helmholtz has given us an enlightening description of his own methods of discovery. "Often enough it [the inspiration] steals quietly into one's thoughts and at first one does not appreciate its significance; it is only sometimes that another fortuitous circumstance helps one to recognize when, and under what conditions, it occurred to one; otherwise it is there, one knows not whence. In other cases it comes quite suddenly, without effort, like a flash of thought. So far as my experience goes it never comes to a wearied brain or at a writing-table. I must first have turned my problem over and over in all directions, till I can see its twists and windings in my mind's eye, and run through it freely, without writing it down; and it is never possible to get to this point without a long period of preliminary work. And then, when the consequent fatigue has been recovered from, there must be an hour of perfect bodily recuperation and peaceful comfort, before the kindly inspiration rewards one. Often it comes in the morning on waking up. . . . It came most readily, as I experienced at Heidelberg, when I went out to climb the wooded hills in sunny weather. The least trace of alcohol, however, sufficed to banish it. Such moments of fertile thought were truly gratifying." [1]

It is unfortunately true, however, that in spite of the recognized importance of such acts as this very little has been done toward explaining their nature or accounting for their occurrence. Whether this is due to the complexity of the acts and the elusiveness of the factors involved, or to an ultimate irrationality in what Carmichael calls the "highest movements of the mind" [2] need not be considered at this point. The fact is that originality, or genius, does not seem to yield to analysis. Certain conclusions of a rather general sort, however, have been drawn in recent years, and they do throw some light on the problem. For the purposes of this discussion these may be divided into the psychological and

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[1] L. Koenigsberger, *Herman von Helmholtz* (Oxford: Clarendon Press, 1906). pp. 209–210.

[2] R. D. Carmichael, *Logic of Discovery* (Chicago: Open Court, 1930), p. 2.

the logical analyses. The former exhibit the general setting of the act, the emotional factors presumably responsible for its occurrence, and the stages into which it seems to break up; the latter attempt to show the technique of discovery and the principles of thought upon which it is based.

PSYCHOLOGY OF DISCOVERY

Graham Wallas [1] analyzes the act of discovery into four stages which he calls Preparation, Incubation, Illumination, and Verification. The first stage is one of "hard, conscious, systematic, and fruitless analysis of the problem." [2] It includes the entire process of intellectual education—acquiring facts, collecting and arranging them, memorizing them, developing critical attitudes of mind, and so on. It is the stage of tiring labor without the rewarding consciousness of progress. The second stage is essentially negative in that it involves "voluntary abstention from conscious thought on any particular problem"; it may be spent "either in conscious mental work on other problems, or in a relaxation from all conscious mental work." [3] Many scientists, for example, obtain the proper diversion by immersion in detective stories; others by vigorous exercise, such as a brisk walk or a game of tennis. The third stage is less directly under control. Usually the idea occurs as a sudden flash, totally without antecedents in consciousness. There is some justification, however, for believing that it is the culmination of a successful train of association, which has itself been preceded by innumerable unsuccessful trains. The immediate antecedents of the flash of insight may often be discovered by careful introspective analysis. James [4] says that "every definite image in the mind is steeped and dyed in the free water that flows around it. With it goes the sense of its relations, near and remote, the dying echo of whence it came to us, the dawning sense of whither it is to lead." It is often preceded by an intimation of its forthcoming appearance.

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[1] *Art of Thought* (New York: Harcourt, Brace, 1926), Chap. IV.

[2] *Ibid.*, p. 81.

[3] *Ibid.*, p. 86.

[4] *Principles of Psychology* (New York: Holt, 1904), Vol. I, p. 255.

The final stage—which is not properly a part of the act of discovery in its most limited form—is one in which the idea is tested and made precise. Though there is almost always associated with the act of discovery an overwhelming conviction that the problem in question has been instantaneously solved, the actual justification for this feeling in the working out of a logical demonstration often requires great labor.

R. S. Woodworth [1] emphasizes the part which is played in the act by the factor of originality. The ordinary man, even the animal, in his knowing response, is not purely passive. "Ideas are not delivered to us ready-made by our teachers, but are modes of response which we have to develop for ourselves." [2] They are of the same general kind as the acquisition of learned reactions in the more ordinary affairs of life, such as in the conditioning of reflexes, and in the compounding of responses. To be sure, the novel reactions which eventuate in great acts of scientific discovery occur only under conditions in which the necessity for a new adjustment furnishes the proper drive. But there is nothing distinctive about the acts themselves; they are analogous to the simple techniques of problem-solving in the more commonplace situations of life. Hence experiments conducted with a view to ascertaining some of the factors involved in these humble cases can be expected to throw light on the more complicated problems. One cannot formulate rules which eventuate inevitably in acts of discovery, but he can offer guiding principles which contribute to effectiveness in solving problems. For example, one should keep his mind open to possibilities which have not yet suggested themselves, but one should also retain a certain rigidity in following out a suggestion; generalization from past experience, and keenness of observation, are also important; but in the final analysis "to have the 'detective instinct' that fixes on the right clue is the mark, in any given field, of the man who has a real gift for original thinking in that field." [3]

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[1] *Dynamic Psychology* (New York: Columbia University, 1918), Chap. VI.

[2] *Ibid.*, p. 136.

[3] *Ibid.*, p. 147.

J. M. Montmasson[1] suggests that the act of discovery may be considered from the point of view of three types of factor which enter into it. The first is the *phenomenon of feeling*. All discovery is motivated by desire. This may be a desire for knowledge, for fame or money, or for esthetic values such as order and symmetry. And all discovery culminates in intense joy. "This took the form of a mystical exaltation in the case of Keplar, Gutenberg, and Descartes; of an ecstasy produced by the unveiling of a new order in the case of Newton, Pasteur, and Ampère. In all these cases it was an enthusiasm evoked by an original vision of rational beauty."[2] The second factor is the *conscious intellectual phenomena*. Here Montmasson discloses three stages—conscious preparation, sudden intuition, and conscious verification—corresponding roughly to the last three stages of Wallas. The first of these stages exhibits a discontinuity between the work of research and the appearance of the new idea. In the second state there is often no element of surprise or suddenness; physics, biology, and technology are characterized by the slow progressive revelation of ideas through analogies, while mathematics and the moral sciences exhibit more often the sudden revelation. In the third stage the mind enters into a new period of conscious activity. "The mind, delighted to have glimpsed the end, cannot be satisfied until it has illuminated the road which it appears to have passed over like a bird in flight. This is the phase of experimental verification, of the justification which is necessary for the hypothesis."[3] The final factor is the *unconscious element*—the idea in its potential or hidden state prior to its entrance into consciousness. One cannot say that such an idea is a mere nothing. "It is a being which already exists, although incomplete; it awaits its perfection, its complements, its act."[4] The birth of an idea requires certain psychical antecedents; these are the potentiality of the resulting idea in the same

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[1] *Invention and the Unconscious* (New York: Harcourt, Brace, 1932).

[2] *Ibid.*, p. 189.

[3] *Ibid.*, p. 117.

[4] *Ibid.*, p. 194.

way that a block of marble is the potentiality of the resulting statue.

LOGIC OF DISCOVERY

The precise sense in which there can be said to be a logic of discovery should be made clear at the outset. It seems safe to say that there is no deductive logic of discovery; data do not contain their own explanatory hypotheses in the same way that premises contain their conclusions. Even in deductive logic there exists the important problem of how the conclusion of a deductive argument can be both novel and true. To the extent to which it seems to be new, it *cannot* be contained in the premises; hence it cannot be implied by them. But to the extent to which it seems to be true, it *must* be contained in the premises; hence it cannot exhibit novelty. The usual way of avoiding this difficulty is to deny that deductive conclusions have any genuine novelty. But hypotheses obtained by inductive discovery are obviously new, and one's only recourse, therefore, is to deny that they have necessary relations with the data. It is therefore impossible to look upon inductive discovery as a process analogous to the operation of a computing machine; one should hardly expect to pour data into one spout, turn a crank, and grind out an explanatory hypothesis from another.

Yet there is clearly *some* sense in which data may be said to "contain" their own explanatory hypothesis. Hypotheses are not drawn from the blue by purely creative acts of mind. On the contrary they are determined by data in at least three ways. They are determined in the sense that if there were no data there would be no hypothesis and if the data were different the hypothesis would be different. They are determined by a more inclusive range of facts in the sense that every hypothesis is probably based to some extent on an analogy. They are determined, finally, by the general nature of the explanatory relation, i.e., by the fact that the symbols which describe the hypothesis must be such as to imply the symbols which describe the data.

None of these relations, however, is sufficiently determinate to permit the ascertainment of one hypothesis to the exclusion of all alternates. One is obliged to say, therefore, not that any hypothesis is *proved* by being *derived* from the data, but that it is given a certain *probability* by virtue of the more or less definite *hints* offered by the data. Data do not imply an hypothesis, they merely suggest it. The function of hypotheses in the total scientific situation makes it extremely unplausible that an explanatory conception could be read out of the data in a necessary way. Hypotheses, as will be seen later, serve as instruments of anticipation; they are devices by which the accumulated knowledge of nature is combined with the imaginative envisagement of possibilities, in such a way as to predict the character of the still unexplored portions of nature. It is unlikely that the unexplored is exactly like the explored; hence nature as known cannot imply nature as unknown. Yet it is equally unlikely that the unexplored is totally different from the explored; hence nature as known must offer the only available hints as to the character of nature as unknown. These two facts suggest that prediction is possible only by what Tyndall calls a *prepared imagination*, i.e., an imagination which endeavors to include all relevant data as guiding factors in determining the creative insight.

A logic of discovery is possible, therefore, only in a limited sense. It offers no technique of prediction. But it does offer a technique by which the imagination may be prepared for the creative leap. Much that is ordinarily included in the act of discovery is of this preparatory character. Hence in recognizing the principles of a logic of discovery one is in a better position to see that the act is really of a twofold character—one part capable of a certain formalization, the other essentially mysterious and unpredictable. Though this approach does not solve the problem of scientific discovery as traditionally conceived, it does reduce it to two more sharply defined problems, each of which is capable or incapable of solution upon its own grounds.

The best approach to this analysis of the act of discovery is to be found in a distinction which has been made frequently in the philosophy of science. It is usually formulated as the distinction between the *abstractive* and the *hypothetical* methods. In a passage already quoted [1] Bavink expresses an equivalent opposition in terms of the "elaborative" and the "hypothetical" methods. Two other formulations of the distinction may be given. According to Cassirer we can proceed in two ways in science. "We can, by a pure 'abstractive' method, separate from a group of given things or phenomena that group of determinations which is common to all members of the class, and which belongs to them directly in their sensuous appearance; or we can go behind the phenomena to certain *hypotheses* for the explanation of the field of physical facts in question. Only the first procedure strictly corresponds to the demands of scientific and philosophic criticism. For only here are we sure that we do not falsify the observations by arbitrary interpretation; only here do we remain purely in the field of the facts themselves, for while we divide the facts into definite classes, we add no foreign feature to them." [2] Dingle's terminology is essentially the same. "Abstraction is the detection of a common quality in the characteristics of a number of diverse observations: it is the method supremely exemplified in the work of Newton and Einstein. Newton, for example, gave us 'laws of motion.' Now motion is not an experience; what we observe are moving bodies. Motion is an abstraction, a quality conceived to be possessed by all moving bodies, however much they may differ in size, shape, color, beauty, virtue, or anything else. The laws of motion express the characteristics of this common quality, and they are therefore a rational means of correlating a vast body of common experience. . . . A hypothesis serves the same purpose, but in a different way. It relates apparently diverse experiences, not by directly detecting a common quality in the experiences themselves,

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[1] Chapter VIII, pp. 168–169.

[2] E. Cassirer, *Substance and Function* (Chicago: Open Court, 1923), pp. 193–194.

but by inventing a fictitious substance or process or idea, in terms of which the experiences can be expressed. A hypothesis, in brief, correlates observations by adding something to them, while abstraction achieves the same end by subtracting something." [1]

There is no doubt but that the distinction here expressed is fundamental to the logic of science. But it requires two important refinements before it can be inserted in a general theory of scientific explanation. The first refinement appears to be merely a matter of terminology, though it is more than this. The method of abstraction should not be so called, since abstraction is only one of several processes which play essentially the same rôle in science. Abstraction is associated with *classification*. But, as has already been seen, *ordering* and *correlating* are two other important processes by which data are put into manageable form. If classification is the operation by which abstractions are obtained, one may say equally well that ordering is the process by which series are obtained, and correlating, or associating, is the operation by which complexes are obtained. What is required, therefore, is a term broad enough in its significance to include operations of classifying, ordering, and correlating. Presumably, "describing" would be precisely the word demanded, since these are all descriptive techniques. Certainly the positivist would be well satisfied with this choice. For him science consists of two methods—the descriptive, which is the essential method of science, and the hypothetical, which is dangerous and to be avoided at all costs. But for the modified positivist and for the realist this term would not be adequate; the essential feature of these methods is that they involve the creation of entities by definite acts of thought. Pure description never does this. In this connection Bavink's choice of the word "elaborative" to characterize these methods is particularly apt. Russell describes such techniques as methods of "logical construction"; they result in

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[1] H. Dingle, *Science and Human Experience* (London: Williams and Norgate, 1931), pp. 22–23.

"fictions" since they create entities such as classes, series, and complexes, not certainly known to exist. The question as to whether or not operations of construction eventuate in fictions is unimportant here. What is to be emphasized is merely the kinship of classifying, ordering, and correlating, and the fact that they are all, in a sense, more than mere descriptions. For this reason it will be well to speak of the *method of construction* in preference to the *method of abstraction*.

But in the second place, the two methods should not be looked upon as alternative, for neither can be employed without the other, and taken together they constitute only one method which is *the* method of science. This can be made clear by examining the distinction between them. It is the acknowledged aim of the method of construction to avoid any unjustified addition to the data; the method merely elaborates or clarifies by emphasizing features of the given. The method of hypothesis, on the other hand, consciously adds to the given "by inventing a fictitious substance or process or idea," as Dingle says. Hence it seems fair to say that the aim of the method of construction is to produce nothing new, while the aim of the method of hypothesis is to produce something new. Expressing the same idea in other words, one may say that the aim of the method of construction is to make as precise as possible the data which are relevant to the problem, while the aim of the method of hypothesis is to devise a symbol by which these data may be explained. This suggests that the method of construction is not alternative to the method of hypothesis but preparatory to it; it also suggests that the method of hypothesis is not alternative to the method of construction but its necessary outcome. Together, the methods make up the act of discovery; singly they constitute the stage in which the imagination is prepared, and the stage in which it makes its leap into the unknown.

This analysis permits a reinterpretation of the methods. If one speaks of both methods as operations for the creation of symbols he may then distinguish them in the following terms: *The method of construction permits one to attribute to the symbols such content as they must have by virtue of the data from which they have been derived*, and *the method of hypothesis permits one to attribute to the symbols such additional content as they may be presumed to have if they are to be successful instruments of prediction.* This indicates clearly that the former is essentially a method of description and the latter is essentially a method of explanation; in the former one "remains close to the data," in the latter he "leaps into the unknown"; in the former one merely elaborates, in the latter he creates; in the former one is simply making more clear what he has already observed, in the latter he is anticipating what he has not yet observed.

A very important consequence of this distinction of methods is that a pure construction can never explain. This may appear at first sight to be quite contradicted by the facts. It seems obvious that science is continually explaining in terms of abstractions, series, and complexes. But a moment's reflection will convince one that in so far as these entities do explain they cannot be pure constructions. For a pure construction must have only such content as one is entitled to give it from the data; but an explanatory entity—if it is to explain—must contain more content than the data which it is to explain. Explanation consists, as will be seen in the next chapter, in the transfer of properties and relations from the explanatory entity to that which is to be explained; in explanation one increases the content of the data by adding to them the features of the hypothesis. But if a construct has been obtained by direct derivation from the data, there is clearly no possibility of turning about and explaining the data in terms of the construct. The construct does not contain that increment of novelty without which explanation is impossible. A pure construct used as an explanatory entity produces an *ad hoc* or *verbal* explanation. One cannot use the soporific quality of opium to explain the fact that it produces sleep unless he knows more about the soporific quality than the fact of its producing sleep. In the same way, one cannot explain actual levers in terms of perfect levers, matter in terms of space-time, heat in terms of molecules, or life in terms of a vital force, if in each case the explanatory notion is a mere construction from that which it presumes to explain. It is likely that many explanations in science are of this pseudo-hypothetical character. Mention may be made of potentials, capacities, unconscious purposes, tendencies, instincts, affinities, powers, urges, and causal efficacies. There is reason to believe that most of these notions—unless they are definitely increased through the hypothetical method—are constructs rather than hypotheses, and hence have no explanatory function.

It follows that the problem of the logic of discovery reduces to two more specific problems: (a) What are the *techniques of construction* or *of elaboration?* (b) What are the *techniques of hypothesis-formation?* Unfortunately no very sharp line can be drawn between these problems. The task of the logic of discovery, presumably, is to formulate the techniques by which nature may be anticipated. But this problem is not likely to admit of solution. The best that one can do is to list the various techniques which may be employed as practical aids to the performance of the act of discovery. But an examination of these methods reveals the fact that they vary among themselves in determinateness. By determinateness is meant the degree to which the operation permits one to *infer* the content of the explanatory entity from the data. It has already been pointed out that the data never *imply* the hypothesis, yet they do seem to determine its content in certain limited ways. The purpose of a classification of the techniques of discovery is to exhibit the most common of these methods in such a way as to disclose their degrees of determinateness. Abstraction, for example, is relatively highly determinate; synthesis and analysis, on the other hand, are not so highly determinate since logic has not yet provided the principles according to which the properties of wholes may be inferred from the properties of parts, or vice versa.

The principle to be kept in mind in such a classification is that the techniques which are highly determinate are to be grouped under the techniques of construction, while those which are less highly determinate belong under the techniques of hypothesis-formation. The reason for this is the fact that the highly determinate operations permit one to extract the explanatory entity, so to speak, from the data; the inferential movement is simply an elaboration of what is already given; the suppositional entity is, in a sense, demanded by the data and hence may be read out of them. Its content may, therefore, be determined more or less completely by the data, and there is little room for the free play of the imagination. It is important to note, however, that constructs obtained in this way are often enlarged through the method of hypothesis. Abstractions, for example, are readily definable in terms of the data from which they have been derived; but entities thus defined are frequently augmented by acts of imagination in such a way as to permit their use as strict hypotheses. Hence to say that the method of abstraction is a method of construction is not to deny that it may also be a method of hypothesis-formation. In the same way, wholes and parts are not readily definable in terms of their respective parts and wholes and therefore afford a wide range for the free play of imagination; hence synthesis and analysis are properly to be classed as operations of hypothesis-formation. But there are clearly some ways in which parts and wholes are interrelated, hence there is a minimal logic of synthesis and analysis. It follows that the classification of these methods as operations of hypothesis-formation is not a denial of their possible consideration also as operations of construction. The best that one can do, therefore, is to construct a scale of discovery techniques, indicating that those at one end of the scale are essentially methods of construction and those at the other end are essentially methods of hypothesis-formation.

It is likely that all such techniques are based ultimately upon analogy. One can anticipate the unknown only if he has had previous samples of it. By a sample of the unknown is meant an idea, once conjectural and hypothetical in character, but now verified to such an extent as to have been incorporated into the known. Hence one's previous successful anticipations become the foundations for new anticipations. But the samples, by themselves, cannot afford such a basis, for one cannot presume that nature is everywhere alike. The principle of the uniformity of nature, so often considered to be the sole justification for inferences as to the future, asserts not that nature is everywhere the same but that there are pervasive and repeating *structures.* The realm of events is much more constant in its structural features than in its contentual features. Hence the principle of analogy entitles one only to infer that there will be the same type of connection between the present known and the present unknown that there was between the past known and the past unknown. It justifies only the assumption that one will continue to meet with success if he attempts to explain in terms of abstractions, serial operations, associations, causes and effects, microscopic elements, and so on—but it does not tell him specifically what content these explanatory notions will have. It is therefore very limited as a principle of anticipation. It enables one to predict the character of the unknown only upon the basis of these highly *general structural features* of the known, and does not enable him to anticipate anything as to its *specific contentual features.* These limitations justify the claim that there really is no logic of discovery in the strict sense of the term.

TECHNIQUES OF CONSTRUCTION

The techniques of construction are not distinguished in any significant way from the techniques of description, discussed in Chapter VI. That the former are "elaborative" rather than "descriptive" means only that they refer to the less obvious features of events. Hence they are such operations as are involved in classifying, ordering, and correlating events when the properties determining these operations are not so apparent as they are in the case of description. To speak of a piece of gold as a deformation in space-time is to say something more abstruse about it than to say that it is yellow and hard, though both are cases of classification; to characterize time as an instance of the mathematical continuum is to speak in more abstract terms than to say that it is a succession of events, though both are cases of ordering; to say that the space covered by a falling body is a function of the time of fall is to correlate events in nature, though in a less obvious way than to say that every falling body occupies space and endures through time.

Classification and ordering, when employed as techniques of elaboration, are usually characterized as *abstractive.* The foundation for this use lies in the fact that the properties specified, whether non-serial or serial, are highly general, and such as apply to a wide range of events. Hence any characterization of events by means of mathematical or logical properties constitutes an abstract representation of them. One of the best examples of abstractive techniques is to be found in measurement.[1] This is an operation which involves recognition of the quantitative features of events and the possibility of ordering them on the basis of certain types of relationships. The operation is called measurement, and the number so obtained is called the measured value; through the operation it becomes applied to the event as an abstract feature of it. The use of mathematics as a tool in science is based upon its abstract character. Similar applications of abstractive techniques may be seen in the "reduction" of consciousness to physiological processes, and of physiological processes to physico-chemical processes.

Correlation and associational techniques may be called, by contrast, *concretive.* They are concerned not with *simplification* through neglect of specific differences, but with *complication* through the association of events into more inclusive wholes. As was seen in Chapter VI, any association of events determines a more complex event of which the

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[1] See Chapter VI, pp. 112–115.

associated events become parts or aspects. The concretive method is employed whenever diverse events are considered to be explicable by the same complex entity, e.g., when expansion, surface tension, and solubility are presumed to be explicable in terms of the molecular constitution of matter. The definition of a substance as the totality of its properties or manifestations is also a case of the concretive method, i.e., it is an attempt to devise a construct in terms of which it will be possible to understand the association of these properties. Often the concretive method involves uniting apparent incompatibles, as in the case of the supposition that the ether is both a fluid and a solid, and in the hypothesis that light is both wavelike and corpuscular. Frequently the concretive method is concerned not with the supposition of a complex entity, but merely with the postulation of a more intimate correlation than that which is obviously given. Inferences from *some* to *all* may be considered as instances of the concretive method.

Both of these methods, however, have been considered in this connection merely as methods of construction, not as methods of hypothesis. The method of abstraction attempts to determine the properties of abstract features of events on the basis of the events themselves, and the method of concretion attempts to determine the properties of complex events on the basis of what is known of the individual events. But both abstractions and concretions, having been defined by the method of construction, may be increased by the method of hypothesis. Through this method the meanings of the abstractions and concretions are increased in such a way as to permit the deduction of further descriptive propositions. For example, measured values take on all of the features of the mathematical systems of which they are parts, and, as a result, the quantities which are measured by means of them take on additional properties. In the same way, the notion of molecules is enlarged through imaginative activities, and, as a result, new properties descriptive of the behavior of bodies are predictable. Such anticipations as these are possible only through the use of the hypothetical method.

TECHNIQUES OF HYPOTHESIS-FORMATION

Since the remaining techniques of discovery are less highly determinate in character, they may be conveniently discussed under the present heading. As was pointed out, the techniques of hypothesis-formation are simply those methods which permit only a very limited ascertainment of the content of the explanatory entity, hence leave room for the activity of the imagination. It should be remembered, therefore, that the following techniques are also methods of construction. Three types will be discussed: (a) serial operations, (b) cause and effect inferences, and (c) operations of analysis and synthesis.

(a) *Serial operations.* These are of two kinds, depending upon whether the operation is from a given segment of a series to an element beyond the segment, or between two supposedly adjacent members. The former may be called the method of serial extension, or of extrapolation, and has been aptly characterized by Eddington as the method of "just like this only more so." [1] It permits extension beyond any given range of data when they are capable of serial arrangement; the property of gradual increase or decrease exhibited by the known segment of the series is taken as a defining principle by which the character of the elements beyond the limits of observation may be ascertained. The latter is the method of interpolation, and involves the insertion of an element between two apparently contiguous members. Serial operations are employed in all of the sciences for the purpose of defining "ideal" entities, such as perfect levers, frictionless motion, ideal gases, utopias, and perfectly isolated individuals; they are the characteristic methods of mathematics and are used in the definition of infinity, irrationals, fractions, zero, and the negatives. More specific illustrations of their employment will be given in Part Two.

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[1] *Nature of the Physical World,* p. 247.

(b) *Cause and effect inferences.* Operations involving the discovery of causes and effects are commonly considered to be the sole concern of science. The more detailed analysis of this notion, which is to be the topic of Chapter XVI, will reveal the truth which there is in this statement. Here the problem is only to point out that the techniques for determining causes and effects are not highly determinate. They do not enable one to infer the characters of causes from effects, or vice versa. Instead, they are principles for telling one when and where to look for causes, given effects, and when and where to look for effects, given causes. They thus enable one to identify causes and effects by their temporal and spatial locations. Certain hoary principles, e.g., that cause resembles effect, supposed by many philosophers to be descriptive of the cause-effect connection in nature, do not seem to have any important application in science, and there is reason to doubt whether they have any clearly defined meaning. A precise logic of causal connections has not yet been formulated. As a consequence no operational construction of causes from effects, or of effects from causes, is possible; specific causes and effects cannot be anticipated with any high degree of certainty. Causes and effects are therefore hypothetical rather than constructual in character; they are given content through activities which are essentially imaginative, though the operations are often guided by analogies.

(c) *Operations of analysis and synthesis.* The demand for operations of this kind is based upon the fact that a whole is better understood when the properties of its parts are known, and the parts are better understood when the properties of the whole are known. The behavior of social groups is more intelligible when the modes of behavior of the individual members are known, and the modes of behavior of the individual members are more intelligible when the nature of social behavior is understood. In the one case the data constitute the complex, and the inferential route is analytic; in the other case the data are the elements, and the inferential route is synthetic. The justification for such an operational route is a logic of analysis and synthesis, which aims to show to what extent the properties of the whole determine the properties of the parts, and vice versa. As in the case of cause and effect connections, such a logic has not yet been written; philosophers have debated endlessly over the question of the "validity" of analysis. Consequently there is no definable technique for this mode of operation, and the discovery of parts from wholes and of wholes from parts is essentially imaginative in character, guided, as in the case of causal inferences, by loose analogies.

In transition to the next chapter reference should be made to the analogy between techniques of discovery and of verification, and descriptive techniques. The former determine an explanatory science in much the same way that the latter determine a descriptive science. In other words, when one sets about to control the objects of perception and the perceptual act, and when he endeavors to classify, order, and correlate the results, he is constructing the body of symbols which constitutes a descriptive science; similarly when one sets into operation the techniques of construction and of hypothesis-formation, and when he endeavors to predict and verify, he is constructing the body of symbols which constitutes an explanatory science. The latter type of science is one which attempts to integrate both the data which are obtained by observation, and the hypotheses and theories which are obtained through acts of discovery and verification. Its structure will be described in the next chapter.

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CHAPTER X

EXPLANATORY SCIENCE

If there are sciences which are empirical or descriptive in character, it is none the less true that there are sciences which are rational or explanatory in character. Mathematical physics, rational mechanics, and geometry are the most obvious examples of such sciences. It was stated at the close of the preceding chapter that explanatory sciences may be considered as the results of the techniques of discovery and of verification in the same way that descriptive sciences are the results of the techniques of classifying, ordering, and correlating; when one endeavors to describe nature he constructs an empirical science, and when he endeavors to explain nature he constructs a rational science. This places verificatory techniques on the same level as techniques of discovery, i.e., as methods through whose application explanatory science takes form. From this point of view it would seem preferable to discuss these techniques before an analysis of explanatory science itself is made. However, the fact that an understanding of verificatory techniques is impossible without an understanding of the general nature of explanation lessens the advantage of this manner of presentation. Consequently, the present chapter will be devoted to an analysis of the meaning of explanation and of the structure of explanatory science, leaving for the ensuing chapter the consideration of the techniques by which an explanatory science becomes corroborated and substantiated.

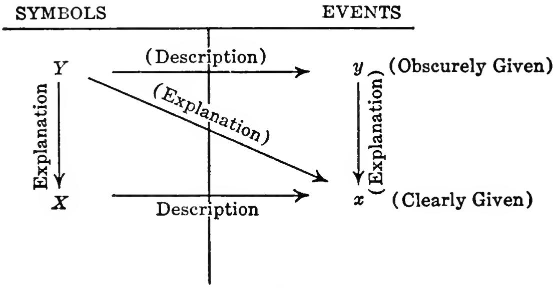
Since the key to the understanding of explanatory science lies in the meaning of explanation, this problem affords the logical starting-point for the discussion. What, essentially, does one do when he explains, and how is the activity of explanation to be differentiated from that of description? The answer to this question will throw into proper perspective the distinction between the two types of science.

NATURE OF EXPLANATION

Though description and explanation are usually sharply contrasted, there is reason to believe that the distinction between them cannot be made in any absolute way. If one takes the realistic attitude toward science and insists that all explanatory entities exist in the same way as that which they aim to explain, there can be no important difference between description and explanation; description calls attention to the more obvious properties and relations of events, and explanation indicates those which are less obvious. Hence one leaves descriptive techniques when he begins to penetrate beneath the surface, or to get behind apparent phenomena. But even if one takes the positivistic attitude and insists that the task of science is the invention of conceptual devices such as classes, series, and laws, which render nature intelligible through simplification and correlation, there is still no significant distinction between description and explanation; employment of the less elaborate symbolic tools is called description, and of the more elaborate tools, explanation. These two interpretations suggest that description and explanation are strictly continuous, and that the drawing of any line separating the one from the other is more or less arbitrary.

Yet there appears to be an important distinction between the two with reference to verification. Both description and explanation are, of course, in terms of symbols, and symbols are verified by their referents. But symbols which describe can be verified directly because their referents are given clearly, while symbols which explain cannot be so verified because the events to which they refer are "beneath" or "behind" the phenomena. Hence the verification of explanatory symbols must be indirect. For example, the description of copper in terms of its toughness, malleability, red color, and bright metallic luster can be quite readily verified by direct inspection and the performing of simple experiments; but the explanation of its properties in terms of atomic structure can be verified only by elaborate techniques which are designed to show what properties copper would exhibit if its structure were of a certain supposed character. Hence verification of symbols for explanatory entities is in terms of their logical consequents, not in terms of the entities themselves as in the case of descriptive symbols.

This affords a convenient foundation for the distinction between description and explanation. An event is described by those symbols which refer to it directly, and it is explained by those symbols which imply other symbols referring to the event directly. Description is thus the limit to which explanation approximates when the events to which it refers become more and more clearly given, and explanation is the limit to which description approximates when the properties to which it refers become more and more obscurely given. The situation may be diagrammed as follows:



In the strictest sense of the words "description" and "explanation," X may be said to describe x and Y may be said to explain X; thus description is a relation of symbols to events, and explanation is a relation between symbols. But according to a more generous use of the word "explanation" Y may be said to explain x, i.e., a symbol may be said to explain an event. Now if one adopts the realistic view in science and maintains that a symbol cannot explain an event unless it refers directly to some other event, then one may employ the word "description" in an equally generous sense and say that Y describes y even though y is a hidden event. By virtue of this use it becomes possible to say that x is explained by y, i.e., explanation is a relation between events. It seems best to employ the words in the strictest sense because this seems more precise and because it is a use which satisfies both the positivist and the realist, i.e., description is at least a relation of a symbol to a clearly given event, whatever else it may be, and explanation is at least a relation between symbols, whatever else it may be. According to this interpretation the essence of the explanatory relation is to be found in the implicative relation between the explanatory symbol and the descriptive symbol. Description is a matter of *extension*, while explanation is essentially a matter of *intension*. Descriptive or empirical science consequently emphasizes the relations of symbols to events, while explanatory or rational science emphasizes the relations of symbols to one another. Descriptive science is primarily concerned with reference outside of itself, explanatory science is primarily concerned with internal consistency. This is the foundation for the distinction, to which reference will be made later in the chapter, that empirical sciences are *real* or *existential* while rational sciences are *ideal* or *non-existential.*

The character of these relations may be made clear by an illustration. Suppose that X is the descriptive law, "Bodies expand when heated." A strictly descriptive law would never have this universal form, since *all* bodies could never be observed, but this fact may be neglected for the present. Then x symbolizes the actual fact of a large number of associations between cases of the application of heat and cases of ensuing expansion; the law may be said to describe this fact. Suppose, now, that Y is a group of propositions about molecules, viz., the group which would ordinarily be said to reveal the defining characteristic of molecules. Since this group of propositions implies the descriptive law, the latter may be said to be explained by it. Hence one may say that propositions about molecules explain propositions about heat, and propositions about heat describe facts in which heat occurs. A descriptive science is constituted by propositions of the kind X. An explanatory science would presumably be constituted by propositions of the kind Y; however, for reasons to be disclosed later, it seems better to characterize an explanatory science as including a descriptive science, and, hence, as any science constituted by propositions of the kind Y and the kind X in the relation Y implies X. Though the Y propositions would not ordinarily be called descriptive, they may easily become so by any techniques which permit y to appear in the more clearly given; for example, in the Brownian movement one may be said in effect to be observing molecules; hence through such activities the propositions about molecules may be considered as approximating to descriptive propositions.

The essence of the explanatory relation, then, lies in the relation of implication. Whenever there is a logical structure consisting of premises and conclusion, the premises may be said to *explain* the conclusion. The premises give the reasons for the conclusion, the conditions which—if satisfied—will determine the truth of the conclusion. Hence the relation of explanation holds between any group of underived propositions (postulates, axioms, definitions, principles) and the corresponding group of derived propositions (theorems, corollaries, laws). The underived propositions need not be ultimately underivable, but are simply, at the moment, considered as underived. The theorems and corollaries of a system of geometry may be said to be explained by the corresponding postulate system, and the laws of empirical science may be said to be explained by the proper set of principles and theories.

However, although the implicative relation seems to express the essence of explanation, it does not exhaust that relation. Implication is necessary for explanation, but is it sufficient? According to Campbell, "Explanation in general is the expression of an assertion in a more acceptable and satisfactory form."[1] He then goes on to show that a statement is more acceptable and satisfactory when it expresses ideas with which we are more familiar, i.e., "it invokes a definite response in our minds which we describe by saying that we understand the statement." Presumably one does not explain, therefore, unless he knows more about the underived propositions than he does about the derived propositions; one always explains the unknown in terms of the known.

There is a measure of truth in this contention, and a proper understanding of the logic of science demands that the notion be clarified. What, precisely, is meant by saying that in explanation one must know more about the explanatory entity than about that which is to be explained? Clearly there is a sense in which one knows more about that which is to be explained, since this is *given* while the explanatory entity is *hypothetical* and *conjectural*, i.e., the datum is clearly given while the hypothesis is obscurely given. Yet if the explanatory entity is to explain one must know more about it than he does about that which he is attempting to explain. This apparent paradox can be resolved if one recognizes the precise character of explanation. The datum is always more familiar in the sense that it is given *observationally*, but the hypothesis is always more familiar in the sense that it is determined *imaginatively*. Now the observational and the imaginative may differ both quantitatively and qualitatively. Quantitatively, the hypothesis contains less than the observationally given in one sense, and more in another sense. No hypothesis attempts to explain more than a *selection* from the total given, hence every hypothesis asserts *less* than is contained in the given; but every hypothesis attempts to make *predictions* about the given, hence asserts *more* than is contained in the given. For example, there are many thermal properties of objects (the occurrence of melting and boiling at one

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[1] *What Is Science?*, p. 77.

temperature rather than at another, the fact of expansion upon solidification in certain substances and contraction in others, and so on) which are not explicable by the theory of molecules; hence the explanatory conception accounts for only a part of the known facts. But the theory enables one to predict certain phenomena of diffusion, surface tension, and solution, which are later verified; hence the explanatory conception accounts for more than the known facts. Consequently one is obliged to say that the observational and the imaginative partly overlap, each extending beyond the other. Qualitatively, the observationally given would seem to have an essential advantage over the imaginatively determined; the facts of perception have a brute character which compels one's acceptance of them. Yet the imaginatively determined has a vividness which cannot be denied. The suggestion has already been made that the anticipations which one makes of nature are founded upon analogies drawn from the character of nature as known; hence the imaginative retains some of the forcefulness of the perceptual. But, what is more important, the imaginative is almost always vivified through the extensive use of iconic symbols; molecules are thought of as small billiard balls, the ether as a jelly, and forces as elastic tubes. This gives a clarity to these notions which would be lacking from a less pictorial symbolism. That the clarity may be of a pseudo-sort, and hence misleading, is not to be denied. But something of this kind seems to be the interpretation of the fact that the explanatory entity is ordinarily more clearly known than that which is to be explained.

The emphasis upon familiarity as an attribute of explanatory entities leads readily to the conclusion that explanation is merely verbal, or linguistic. To explain a proposition, according to this conception, is to express another proposition to which the first can be reduced by certain rules of transformation. Propositions are equivalent if they may be substituted for one another in all contexts in which they occur. Explanation is therefore essentially the same as translation from one language into another; for example, all propositions about heat may be translated into propositions about molecules, and if one happens to be more familiar with the latter language than with the former he succeeds by this act in explaining heat.

It is clear that this conception of explanation is in strict accord with the positivistic conception that entities should not be multiplied beyond necessity. Explanation is a linguistic matter—of this one can be sure. Whether it is more than a linguistic matter, i.e., whether there actually are entities referred to by the explanatory symbols—of this one cannot be sure. Hence the principle of caution demands that one refrain from supposing anything which is not certainly known to exist. It is also in strict accord with the positivistic contention that logical deduction is itself merely verbal; conclusion can be obtained from premises by the application of the rules for substitution. Hence premise and conclusion, underived proposition and derived proposition, statement about explanatory entities and statement about data—both propositions state the same fact, though in different language.

The realistic reply to this contention follows the lines laid out by Bavink in the quotations given in Chapter VIII, and need not be discussed here. Suffice it to say that for the realist explanation is not merely a matter of language, but an activity involving the actual penetration into the hidden recesses of nature. When one explains he is not exploring language, but nature; nature reveals not only heat but molecules as well, and the problem of science is to determine the precise character of the correlation between these two types of natural entity. This is sharply contrasted with the positivistic attitude which insists that there is only one thing, which may be spoken about either in the language of heat or in the language of molecules. (It is also sharply contrasted with an attitude of extreme realism which insists that when heat has been explained it has been explained away, hence *only* molecules exist.) The realist quite justly insists that there is as much of an assumption involved in making the distinction between the explanatory entity and the datum *merely* a linguistic one, as there is in making it a real or a natural one. The fact that it is a linguistic distinction affords no grounds for concluding either that it is or that it is not also a real one.

The general conception of explanation here developed permits of any number of more specific interpretations. It is sometimes contended, for example, that explanation is always in terms of causes, or that it is always in terms of generalities, or that it is always in terms of microscopic elements such as atoms and electrons. Nothing in the above conception of explanation would forbid any of these types of explanation. All that would be required in each case would be that the propositions describing the causes, or the general principles, or the minute elements, be such as to permit the logical deduction of certain other propositions descriptive of the data as actually known and as discoverable by further exploration. Whether or not such can be done is dependent upon the required logic. If there is a logic of cause-effect relations, then from propositions making assertions about causes one may infer propositions making assertions about effects, and perhaps vice versa. Similarly with reference to universal-particular relations, part-whole relations, and the like. For example, one of the vital problems of modern science is the legitimacy of inferring from the properties of the parts to the properties of the whole which is made up of these parts, and vice versa. Does determinism as exhibited by the mass behavior of particles afford a basis for concluding a corresponding determinism with reference to the individual elements? Are there "emergent" or "holistic" properties possessed by groups but not predictable from a knowledge of the properties of the elements? Do molecules in organic compounds possess properties different from those which the "same" molecules possess in inorganic compounds? Each of these problems is reducible to the general problem of the validity of analysis or of synthesis, and as such it is a special case of the problem of explanation. What is to be emphasized here is that explanation is not to be limited to one type. It is just as possible for the biological and social sciences to become explanatory—though they may require hypotheses of final causes and purposes—as it is for the physical and mathematical sciences through the application of notions of efficient cause, abstraction, and analysis.

Granting that explanation is approximately the sort of thing which has been discussed, what may be said as to the character of an explanatory science? Following the outline of Chapter VII explanatory science may be considered from the point of view (a) of extension, and (b) of intension.

EXTENSIONAL FEATURES OF EXPLANATORY SCIENCE

Extensionally explanatory science aims to represent both (1) the clearly given events, and (2) those less clearly given events which although conjectural as to status, are required both for the purpose of rendering the clearly given events "intelligible" and for the purpose of increasing the range of our knowledge of such events.

(1) The basic feature of explanatory science to be insisted upon here is that such a science, in becoming explanatory, does not cease to be descriptive. Every explanatory science consists of two parts: (a) the descriptive propositions which constitute precisely the empirical science upon which the explanatory science is founded, and (b) the hypothetical propositions which characterize all of the explanatory entities in terms of which the descriptive propositions are to be rendered intelligible. These constitute, respectively, the derived and the underived propositions, the theorems and the postulates, symbols of the kind X and of the kind Y, as given in the diagram on page 198. Every explanatory science, therefore, retains its descriptive reference; it contains a number of propositions which are capable of empirical verification. It is precisely these propositions out of which the explanatory science in its broader

sense arose, and in terms of which it is to be accounted for. Whatever else the explanatory propositions may do, they must at least explain the descriptive propositions. Otherwise they become purely arbitrary, subject to no criterion except that of internal consistency.

What is maintained here may be clarified by pointing out the thesis with which it is contrasted. It is usually asserted that sciences are capable of being divided into two mutually exclusive classes—the empirical, inductive, existential, real, or *a posteriori*, and the rational, deductive, non-existential, ideal, or *a priori*. The former, illustrated by experimental physics, biology, and sociology, are derived from the realm of events, and therefore refer to this realm; the latter, illustrated by logic, geometry, and rational mechanics, are not derived from the realm of events, and therefore do not refer to this realm. Hence the former may be called real sciences, while the latter must be called ideal. But since the latter exhibit an important logical structure they may be called deductive or necessary, while the former must be called inductive or probable. A proposition which belongs to one type of science cannot also at the same time belong to another. Hence no proposition can be both logically necessary and empirically true. Einstein has formulated this thesis clearly in a statement which is frequently quoted: "As far as the laws of mathematics refer to reality, they are not certain; and as far as they are certain, they do not refer to reality." [1]

The unfortunate consequence of this dichotomy is that it leaves inexplicable the problem of how the rational sciences contribute to the understanding of the empirical sciences. There is not only pure mathematics, but applied mathematics; not only rational mechanics, but empirical mechanics. If rational sciences have not been derived from and do not talk about events, how can they have any application to such events? The goal of science is neither propositions having a maximum of necessity and a minimum of truth,

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[1] A. Einstein, *Sidelights on Relativity* (London: Methuen, 1922), p. 28.

nor propositions having a maximum of truth and a minimum of necessity; on the contrary science is seeking propositions having a fair amount of both necessity and truth.

This goal can be attained if one recognizes that the rational sciences contain both empirical propositions and those other propositions which have been formulated for the purpose of explaining the descriptive propositions. Rational sciences are simply empirical sciences which have expanded into the realm of the theoretical. The theoretical propositions cannot be taken away from the total science in which they occur, for apart from this setting they become purely arbitrary, and responsible only to the imagination for both meaning and truth. In the context of their origin, however, they retain responsibility to the empirical propositions; they are both meaningful and true to the extent to which one may deduce from them propositions capable of empirical verification. All of the entities of the so-called rational sciences, therefore, must be considered merely as hypotheses in terms of which empirical propositions are to be explained, and from which further empirical propositions are to be derived. This seems to be the status of all propositions about perfect triangles and circles, frictionless motion and ideal gases, i.e., they refer to hypothetical events whose character is to be determined by the events which they are called upon to explain. It is unfortunate that when a descriptive science becomes explanatory the interest of the investigator shifts from the empirical propositions to the hypotheses. There is reason for this shift of interest since the empirical propositions can all be deduced from the hypothesis and hence need not be attended to explicitly. But what should be emphasized is that the empirical propositions cannot be deduced unless there is admitted into the postulate system a further proposition which states the relation of the ideal entities to the actual entities. Apart from this there can be no "applications" of the postulate system, and it becomes arbitrary.

(2) An explanatory science also claims to contain symbols for those less clearly given events which are required for

explanation. The contrast with descriptive science is here somewhat obliterated. As has been mentioned repeatedly, data are given with continuously varying degrees of clarity. Hence if one attempts to point out the most clearly given data, he may be said to be describing; but if he symbolizes the less clearly given data, he must be said to be explaining. Descriptive science, for example, insists that the task of science is the symbolism of the classificatory, serial, and correlational features of events. But it is noteworthy that descriptive science limits itself to the most obvious of these features, i.e., it does not describe in terms of highly abstract properties or series, nor does it refer to universal correlations. The presumption is that these features of events approach the obscure, and hence would lead into explanatory science. Reference to such entities would, in fact, constitute border-line cases, and one might say with equal justification that he is describing or that he is explaining. A mature explanatory science represents the continuation of this process of symbolizing more and more obscure entities. Hypotheses of the type mentioned earlier in the chapter (causes and effects, ends, elements and wholes, etc.) indicate sciences which have become definitely explanatory. Since it is not known that all events possess causes, effects, ends, elements, or wholes, one is plunging into the realm of the conjectural. He runs the risk of populating nature with entities of his own creation. Hence he has abandoned description and resorted to explanation.

INTENSIONAL FEATURES OF EXPLANATORY SCIENCE

Intensionally every explanatory science (1) exhibits a high degree of integration, whose essential feature is (2) the explanation of all descriptive symbols by means of hypotheses.

(1) An explanatory science is a system of symbols rather than a mere aggregate. An explanatory science is a descriptive science in which every symbol has found its proper place. A descriptive science is one in which no

symbol has important relations to any other symbol; any symbol might be removed from the scheme or modified without affecting the other symbols in any significant way. The integration of a descriptive science is achieved by introducing hypotheses which serve to relate apparently disconnected propositions by showing that they are jointly implied by the hypotheses. Hence an explanatory science which is precisely the expanded descriptive science, exhibits relations of super-, and sub-, and coördination of propositions. Every proposition is either an underived proposition (axiom, postulate, hypothesis) or a derived proposition (theorem, corollary, descriptive law); and every derived proposition takes its place subordinate to the hypothesis from which it can be derived, and coördinate with all other derived propositions deducible from the same hypothesis. The ideal of explanatory science is the construction of a single system of postulates from which all other propositions in the system may be derived. The special and general theories of relativity approach the ideal of such a postulate scheme in the field of physics.

The increased compactness of an explanatory science is accomplished by the introduction of highly integrating propositions. Such a science is distinguished from a descriptive science by the presence of universal laws in place of statements of occasional correlations, and by the presence of mathematical functional relations in the place of qualitative correlations. Since explanatory science is not tied down to what is clearly given it may assert universal laws, even though they may be hypothetical in character. Hence one may generalize in explanatory science from what happens in a few cases to what happens universally and without exception. This becomes an important instrument of integration, for every future case is derivable from such a law. Furthermore, mathematical techniques of measurement and correlation may be introduced. Relations of mere presence and absence are replaced by relations of degree of quantitative manifestation. For example, the colors become arrangeable on a scale through the numerical measures of their wave lengths; chemical substances become arrangeable in a table on the basis of atomic weights; circles, ellipses, parabolas, and hyperbolas become related through a general quadratic equation. As a result every symbol in the system takes on important relations to other symbols, and significant information about any symbol may be obtained merely by exploring its intensional features. *Definition* plays a predominant rôle, which replaces that of *illustration* in descriptive science. For as the system takes on integration the meaning of any symbol becomes increasingly a function of its associated symbols, and decreasingly a function of its referent.

In view of this fact there arises a peculiar duplicity in the meaning and truth of those descriptive propositions which have been explained by the postulates. A descriptive proposition has two very important relations, both of which may be considered as contributing to its meaning and truth: It has a relation to the fact which it describes, and it has a relation to the postulate group from which it can be deduced and by which it is explained. For example, the proposition $2 + 2 = 4$ is descriptively true, since it states a fact about stones, leaves, and other physical objects; but the proposition is also deductively true since it can be deduced from a series of postulates constituting the logical foundations of the number system. There is no difficulty here, since the proposition is true in both senses. But there are propositions which are descriptively false yet deducible from postulate schemes, and the question arises in such cases as to whether the propositions are true or false. The answer is to be found in considerations of the kind suggested in Chapter VII in the discussion of analytic and synthetic propositions.[1] Whether a proposition of the kind in question is to be considered as true or false depends upon whether the intensional relations are or are not to be taken as regulative over the extensional, i.e., whether the deducibility of a proposition from a postu-

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[1] Pages 142–143.

late set constitutes a ground for severing its connection with empirical data. So far as descriptive science is concerned, the answer here is definite; intension is never regulative over extension, hence a proposition is true or false according as it is descriptively true or false, regardless of its deducibility from a postulate system. But the tendency in rational science is to make intension regulative over extension, and hence to say that a proposition which is capable of being deduced from a postulate set is true no matter what its descriptive reference may be. This is essentially equivalent to considering the relation between the *Y* and the *X* proposition as analytic, and as depending therefore upon linguistic factors rather than upon data. If the postulates do imply the propositions in question, it is part of their meaning that they do so, hence nothing empirical could ever constitute a refutation of this fact; the postulates imply the propositions in exactly the same sense that the meaning of the word "triangle" contains the meaning of the phrase "three-sided figure." The only alternative is to consider the relation between the *Y* and the *X* propositions as synthetic, and as depending therefore upon empirical data rather than upon linguistic factors. If the postulates imply the propositions in question it is only an empirical fact that they do so, hence nothing in the character of language or logic could legislate this away; the postulates imply the propositions in exactly the same sense that the meaning of the word "body" is connected with the meaning of the phrase "possessing weight." According to this latter conception a descriptive proposition is always true or false in an empirical sense, and nothing in the character of postulate sets could make a descriptive proposition which is empirically true false, or one which is empirically false true.

This results in the development of a peculiar sensitivity in explanatory sciences. Being highly integrated, they suffer extreme shocks when new information necessitates a change. For a change in any one part of the system requires modifications in all other parts. Mathematics experienced such a shock last century with the formulation of non-Euclidean geometries, and physics passed through a similar crisis with the advent of the theory of relativity. Descriptive science is able to make readjustments by specific modifications in the region where the error is discovered. But in such explanatory science there are no isolated regions; every element has determinate relations to every other element. An explanatory science is organic in the same sense that the human body is; a purely localized disturbance may "make one sick all over."

(2) An explanatory science explains. Here, again, as in the case of descriptive science, this statement may be a pure tautology unless one interprets it. Explanatory science answers the question "Why?" and thus eliminates all brute facts. A brute fact is one which is unrelated, and might equally well have been otherwise. There are no such facts in a highly developed explanatory science. For every descriptive proposition is derivable from an hypothesis, and if it were otherwise the hypothesis would of necessity be otherwise; but if the hypothesis were otherwise all other descriptive propositions deducible from it would be different. Hence no descriptive proposition can any longer be said to assert a brute fact, for it has found its place in a general system of propositions which represents a general system of facts. Every descriptive proposition in the scheme has two definite relations—one extensional, to the events which it describes, and the other intensional, to the hypothesis from which it can be deduced. The extensional relation *verifies* the proposition, and the intensional relation *explains* the proposition. For example, the proposition "This metal bar expands when heated" is verified by a simple experiment which produces the event described in the proposition. The proposition in question is explained when it is shown that it can be deduced from a general theory stating that the bar is made up of molecules which are put into rapid motion when the bar is heated, and which tend by the violence of their motion to separate one from another and thus to increase the space which is occupied by their aggregate.

But a genuine hypothesis explains also in a more far-reaching sense. Every hypothesis is an instrument of discovery. As such it must permit the deduction of propositions not included in the list of known descriptive propositions, but such as to be later verified. Every hypothesis must be fruitful. It must direct the attention of the investigator to realms of nature not yet explored, and it must suggest novel experimental set-ups. This function of anticipation, as has already been seen, is a result of acts of creative imagination. The precise way in which it operates in science will be considered in the next chapter.

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CHAPTER XI

VERIFICATORY TECHNIQUES

The fact that explanatory science develops not only through the use of the techniques of discovery but through the application of the verificatory techniques as well, unites the present chapter with the preceding one in an intimate way. Properly speaking, no hypothesis is truly such until it has been verified. Consequently, a discussion of the techniques by which hypotheses are discovered or devised is insufficient. A genuine hypothesis must contain implications with reference to *novel* data, i.e., it must be fruitful in predictions. To assert this is to say, simply, that every hypothesis must be such that if it were true there would exist a certain specifiable state of affairs. The verificatory techniques are concerned with the ascertainment of what this state of affairs would be, and whether it actually exists as predicted. To the extent to which the anticipations are verified by the proper techniques the hypothesis takes its place as an integral part of the more inclusive explanatory science. Hence an established explanatory science must be presumed to contain a certain number of such hypotheses, once conjectural in character but now verified.

In order to prepare the way for this discussion of verificatory techniques, a brief résumé of the essential methods employed in the formation of a hypothesis may be given. This will duplicate, to a certain extent, material already presented in earlier chapters, but some features not yet considered will be introduced. The topic may be called the *development of a theory*.

DEVELOPMENT OF A THEORY

A theory may be said to develop through four stages. The first is the preparatory stage in which the theory is still embryonic. This is the stage in which the science from which the theory is to emerge is still at the descriptive level. The techniques employed up to this point have been directed mainly at *getting* the data through the introduction of physical, physiological, and psychological operators, and *describing* the data through the introduction of classification, ordering, and associating. Only the most obvious events are attended to. At this level there is no mention of hypotheses or theories, though the foundation upon which the theoretical structure will be built is being established.

The second stage is one in which there is recognition of the insufficiency of mere description. It is seen that the task of science is not merely the description of that which is obviously given, but the explanation of this in terms of something which is less obviously given. Hence it is admitted that the problem of science lies in the more or less imaginative extension of the given and the resulting introduction into science of conjectural, hypothetical, and theoretical entities. The essential technique which is employed at this stage is the method of construction. It recognizes the danger in the use of the uncontrolled imagination, and endeavors to keep the explanatory conceptions as close as possible to the range of data. It attempts, so far as this is feasible, to *derive* explanatory conceptions from the data. The methods of abstraction and of concretion are the two most important techniques at this stage; they aim to give to the conjectural notion only such content as is demanded by the data. Hence they are considered to be essentially elaborative, and not sharply differentiated from the strictly descriptive techniques.

The third and fourth stages constitute the most significant stages in the development of a theory. For only at these levels does the theory become truly explanatory. The theory explains because it implies propositions descriptive of the data. Hence the data are explained if there can be found the required propositions making assertions about hypothetical and conjectural entities, and of such a character that all of the propositions descriptive of the data can be deduced from them. But since this body of descriptive propositions includes not only those which are known to apply to the data but also those which constitute anticipations of data not yet disclosed to perception, it is readily seen that, if this is to be possible, the explanatory entity as obtained through the method of construction must be increased through activities of the imagination. If the construct is merely derived from the data it cannot be turned about and used to explain the data, nor can it be used to predict data not yet observed. Hence these two stages represent the theory as it undergoes progressive expansion through the tentative attribution to it of the desired content.

The distinction between the third and fourth stages is relative. In the third stage one attempts to increase the content of the explanatory entity in such a way as to permit the deduction of the propositions already known to be verified, i.e., the propositions descriptive of the original data of the problem. In this activity one finds it hard to avoid the feeling that he is cheating. He is deliberately giving the explanatory entity such content as will permit him to deduce propositions which he already knows; he is making the hypothesis do precisely what he wants it to do. He can be sure that the explanatory entity will explain because he has developed it in such a way as to make it do exactly that thing. But in the fourth stage, into which the third merges imperceptibly, the investigator is not proceeding in such an obvious way. Here he attempts to increase the content of the explanatory entity in such a way as to permit the deduction of propositions not yet known to be true, i.e., propositions descriptive of data still to be discovered. This stage constitutes the essential moment in the development of the theory. Only in this stage has it become a genuine theory, fulfilling its predictive function. Only in this stage has it become really novel, enabling one to anticipate nature. Here one no longer feels that he is playing the game unfairly. On the contrary he recognizes that the activity of imaginative expansion rests upon insight rather than rules, upon penetration rather than superficial awareness, and upon genius rather than techniques. Whether this activity is considered to be "creative" involving the "invention" of explanatory entities, or "penetrative" involving the "discovery" of obscure features of nature, depends upon one's general theory of science, and need not be decided—if it can be decided at all—at this point. It may be that scientific genius is like artistic genius, founded upon the activity of imagination which freely transforms and modifies the given; if this is the case the third stage would be differentiated from the fourth in the fact that in the latter the given has been definitely increased by contributions which are the product of human inventiveness. On the other hand, it may be that scientific genius is characterized only by a peculiar insight which enables it to explore the hidden recesses of nature, i.e., it may be that the mental act which is responsible for a theory is nothing more than the vague awareness of the existence of certain data, heretofore undiscovered. If this is the case the third stage would be differentiated from the fourth only in the fact that the data to be explained by the theory are clearly recognized in the former and only vaguely grasped in the latter. At any rate, the techniques of the third and fourth stages are not differentiated; so far as they can be formulated at all they are such indeterminate operations as serial extension and interpolation, cause and effect inferences, and analytic and synthetic operations. In all of these operations, however, the hypothetical expansion is a matter which is not susceptible of formalization; it reduces ultimately to a matter of insight or genius, which is to say that it is essentially a mystery.

A theory which has developed through the four stages is ready for verification. Whether the verificatory operations are considered as contributing to the development of the theory and hence as constituting a fifth stage in its growth, or whether they are considered to be techniques of testing or of corroboration, is a matter of no great concern. It is clear that a theory is not properly such until it has undergone at least a minimum of verification; inadequacies in the theory can be detected only by examining the predictions in the light of the facts, hence a false theory may be said to be one which has developed improperly or incompletely. This would seem to argue for the thesis that verification of a theory is the fifth stage in its development. But the movement of verification, on the other hand, is quite strictly the reverse of the movement of discovery. The movement of discovery is essentially a movement from data to symbols, from the clearly given to the obscurely given; while the movement of verification is essentially a movement from symbols to data, from the obscurely given to the clearly given. The former is a leap in the quagmires of conjecture, the latter is a return to the hard ground of fact. The former is inductive, receiving its motivation from insight and imagination, and carried on most readily by those types of mind excelling in spontaneity and suggestion; the latter is deductive, deriving its impulse from criticism, and pursued most actively by those types of mind excelling in logical rigor. Hence the movements seem to be different in kind and to offer support for the thesis that in the movement of verification the attitude and direction of the mind have been changed.

In the broadest sense of the term, verification may be considered as a double movement. It involves, first, the actual drawing of the implications from the hypothesis after it has been expanded through the hypothetical method. This may be called the *movement of prediction*. But it involves also the act by which the implied notions are compared either with existing data or with data which are capable of creation through experimental methods. This may be called the *movement of confirmation*. Each of these will be discussed briefly.

PREDICTION

The movement of prediction is one of deduction, and, as such, seems to offer no significant difficulty. There are recognized techniques of formal deduction. Hence the problem of prediction apparently lies simply in the derivation of the required propositions, by means of the rules of logic, from the body of propositions characterizing the theory. But the problem is not so simple. What is important about any hypothesis is not what it implies *formally* but what it implies *contentually*. The formal implications of a postulate system are those which can be drawn merely through the applications of the principles of logic; they are not based upon the content of the system, and are therefore independent of what the system is about specifically. They are expressive only of the highly abstract features of events—features which belong to all events indiscriminately, and hence of no importance where reference is made to a limited range of events. The contentual implications, on the other hand, are those which are based on empirical generalizations; they are expressive of the content of the system, and are therefore dependent upon what the system is about specifically. They are expressive of the more concrete ways in which particular kinds of events are associated in nature, and they are true only of events correlated in these ways.

As Carnap points out,[1] one must recognize the need in physical science for drawing consequences of two different kinds from a given proposition. On the one hand are the transformations which are based on logical rules, such as are found in *Principia Mathematica*. But one could add to such a system "transformation rules of an extralogical character, for instance some physical laws as primitive sentences, as, for example, Newton's principles of mechanics, Maxwell's equations of electro-magnetics, the two principles of thermodynamics, and such like." Carnap calls the former *L-rules* and the latter *P-rules*, and the respective consequences *L-consequences* and *P-consequences*, a distinction which may be generalized here in terms of *formal* rules and consequences and *contentual* rules and consequences. For example, the proposition, "Matter is made up of molecules, and molecules increase their rate of motion through the application of

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[1] *Philosophy and Logical Syntax*, pp. 50–54.

heat," formally implies a number of propositions. It implies, "Matter is made up of molecules," "Molecules increase their rate of motion through the application of heat," "Matter is made up of molecules or of atoms," "Molecules increase or decrease their rate of motion through the application of heat." But none of these propositions conveys any very important new information about molecules. They are all possible by virtue of the fact that the proposition in question has the form "*p* and *q*," from which the propositions "*p*," "*q*," "*p* or *r*," and "*q* or *s*" follow without regard to content. The contentual implications of the proposition in question, however, are somewhat more informative. If the application of heat to bodies increases the speed with which the molecules are moving, it follows that bodies will expand when heated. If matter is made up of molecules in motion, it follows that gases placed in the same container should diffuse, and that certain solids immersed in fluids should dissolve. Only implications of this kind have any value as instruments of verification; they follow from the particular assumption about the molecular character of matter, and would not, presumably, follow from any alternative theory. Hence they are implications which permit decision to be made as to the adequacy or inadequacy of a theory. Formal implications permit no such decisions to be made.

But whereas formal implications are of no great use in verification, they are the only techniques for which there are rules. There are no rules for the drawing of contentual implications. No formal analysis of the notion of molecules would enable one to predict what empirical phenomena would exist if there were such things as molecules. The only justification for a contentual implication is an empirical generalization. "Man" implies "mortality" because all men are mortal; "metal" implies "conductor of electricity" because all metals are conductors of electricity; "increase in pressure of a gas" implies "decrease in volume" because all increases of pressure are accompanied by decreases in volume. One proposition contentually implies another because the facts which they describe, respectively, are related in nature in a specifiable way.

But this does not meet the difficulty which is involved in making predictions from hypotheses. Only the contentual implications of the hypothesis count. But empirical generalizations are of no avail in making predictions from hypotheses. For every hypothesis designates an entity which is not certainly known to exist. Hence, in the very nature of the case there can be no *descriptive* generalizations expressive of the behavior of hypothetical entities. Every hypothesis, considered as a postulate for the drawing of implications, represents a contrary-to-fact condition. It has been debated by philosophers whether any consequence follows from a contrary-to-fact condition. Certainly no strictly contentual implication can be drawn. It may be insisted that an hypothesis designates not a contrary-to-fact condition but merely a different-from-fact condition. But in either case the empirical foundation for the generalization has disappeared, for the hypothesis refers to a state of affairs which has never been observed and which therefore has never been described in generalizations.

It follows that there is no technique for prediction. Here, again, resort must be had to more or less imperfect analogies. If molecules have never been observed, one simply cannot know how they will behave. But billiard balls in a bushel basket *can* be observed and crude analogies can be drawn from such behavior. Furthermore, operations of serial extension upon such objects, involving consideration of them as getting smaller and smaller, and more and more elastic, can be performed. To be sure, in this process of refinement the objects will almost certainly lose many of their empirical properties; but they will probably retain some of them, and the nature of the operation of refinement should tell the investigator roughly what these are. But every prediction from the resultant entity can be no better than probable. Necessary prediction from hypotheses seems impossible. All that can be said is that predictions will be more or less probable according as the realm of the clearly given exhibits events more or less like the hypothesis in question. If there are events of a closely similar character, the empirical generalizations descriptive of the behavior of such events may be taken as the foundation for predictions from the hypothesis, and such predictions will have a certain plausibility; but if there are no events of a closely similar character, the only foundation for the prediction is some less closely related empirical generalization which is then imaginatively modified through such activities as abstraction and idealization. Predictions in such cases will have a correspondingly lower degree of plausibility.

Reference should be made to a further feature of the predictive act. In order that the verificatory movement may be carried out it is not sufficient simply that implications be drawn from the hypothesis; the consequences must be such as can be either confirmed or infirmed [1] empirically. By this is meant not that the predictions must be such as can be corroborated by data, though such must be the character of the predictions if the theory is to be established; what is meant is, rather, that the predictions must be of the general kind which have relevance to empirical data, i.e., they must be of the kind which can be unequivocally confirmed or infirmed by experience. Hence the drawing of an implication which demands the performance of an impossible physical experiment, or the assuming of an impossible observational point of view, would be illegitimate. Furthermore, the implications must be such as to refer to the range of the more clearly given data; no verification of hypotheses, which by their very nature refer to the obscurely given events, can be made if the implications drawn from the hypotheses do not take one out of the realm of obscurity. From this it follows that verifications of highly abstract hypotheses (e.g., the law of universal gravitation) cannot be

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[1] The term "infirm" may be used in what follows to designate negative verification or negative confirmation. Thus the proposition "Today is hot" would be confirmed if the day is hot, but would be infirmed if the day is cold. This use is closely similar to that of Nicod, though not identical with it. See Chapter XVI, p. 361.

made in terms of further high abstractions but must be made through concretions (e.g., balls rolling down inclined planes); verifications of microscopic phenomena (e.g., molecules) cannot be made in terms of further microscopic phenomena but must be made through macroscopic objects (e.g., expanding bodies); verifications of idealizations (e.g., perfect levers) cannot be made in terms of other idealizations but must be made through actual objects (e.g., real levers). In every case the language of the hypothesis must be "translated" into the language of empirical data. Norman Campbell [1] suggests that every theory contains a "dictionary" by which ideas inside of the theory are related to other ideas whose truths are determined independently of the theory. It is by means of a dictionary that one passes from mathematical equations containing only numbers to the objects of which the numbers represent measured values. This is merely an alternative formulation of the necessity for providing a technique by means of which the essentially unverifiable entities constituting the hypothesis can be related to more empirical entities. It is an additional corroboration of the thesis, already insisted upon in Chapter X, that every rational science must retain its empirical reference; without the descriptive propositions from which it has been derived and in terms of which it is verified it becomes purely arbitrary; it has the same status as an hypothesis which has not been verified.

CONFIRMATION AND INFIRMATION

Confirmation and infirmation are the operations by which the predictions from hypotheses are established or disestablished. The scientific procedure as a whole is essentially a series of inductions and deductions, guesses and corroborations, flights and perchings. The confirmatory-infirmatory movement is that part of this total movement by which the imagination is restrained and kept relevant to the data. It is the act in which the scientist endeavors

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[1] *Physics, the Elements*, pp. 122 *et seq.*

to verify his imaginative efforts by returning to the realm of data from which he departed in search of explanatory conceptions.

It should be clear that the heart of this act lies in perception, which is, as was suggested in Chapter V, the essential method for getting events. All that was said in that connection may be repeated here, with a slight shift in emphasis. Whereas perception in the context of the movement of discovery is aimed at the derivation of symbols, perception in the context of the movement of verification is aimed at the verification of symbols. One perceives not merely to get ideas but to establish conjectures. The significant difference in the perceptual act in the two cases lies in the greater emphasis on control in the latter case. In the former, one observes anywhere and at any time for the purpose of noting anything that might occur; in the latter, one observes a specific spatio-temporal situation for the purpose of noting whether a specific event occurs. There is, consequently, in the latter case, a definite psychological framework within which the perceptual act takes place; attention becomes keener because of the functioning of the psychological operators. But there is a corresponding proneness to error, since the mind tends to read preconceived ideas into the physical situation. Bacon was so conscious of this fact that he placed undue emphasis on the movement of discovery, hoping by this to eliminate the verificatory act. It seems futile to argue whether facts precede hypotheses or hypotheses precede facts. Certainly at the stage in which science emerges upon the scene both facts and hypotheses are given. It is quite unlikely that a pure fact, without an increment of interpretative material, is ever perceived; and it is equally unlikely that a pure hypothesis, without connection with any observed fact, is ever conceived. Mind is not a *tabula rasa*, as Locke supposed, nor does it approach the world with a bundle of fixed forms into which facts are to be fitted, as Plato suggested. All that need be recognized for science is that investigation into nature is sometimes primarily a search for forms, and at other times primarily a verification of previously suggested forms. Both movements are essential to science.

A similar distinction may be made between experiments for discovery and experiments for verification. As was pointed out in Chapter VI, the more usual sorts of experiment belong in the latter class. The essential difference between the two is that experiments for discovery are *unguided*, while experiments for verification are *guided*. The guiding factor in the latter case is a preconception as to the nature of the outcome. In the inductive experiments there is no foreseen knowledge of the result; manipulations are made in a more or less random way, and the results of the changes are noted and recorded. But in verificatory experiments there is an anticipation of the result; it is predicted that a change of a specified character *should* bring about another change of a specified character. In the former case one is merely learning from nature; in the latter he is, in a sense, legislating for nature, since he is using his acquired knowledge of her behavior in other realms to predict her character in a given realm. He is saying to nature, in effect, "If you have not been deceiving me you must behave in such-and-such manner in the present situation." Verificatory experiments are therefore much more complex than inductive experiments, for one's previous experience with nature suggests myriads of ways in which changes may be introduced into it. In pure observation one merely listens to nature, in experiments for discovery he prods nature to make it speak, in verificatory experiments he asks nature questions—in fact, he asks questions which are definitely leading, and he is justly surprised when he receives negative answers. Consequently verificatory experiments are much more decisive than inductive experiments.

The distinction which is commonly made between observational and experimental verification is largely relative. For example, it may be maintained that the proposition

"Sodium is silver in color" can be verified observationally, but the proposition "Sodium burns with a yellow flame" must be verified experimentally. However, if all perception involves control the difference between these two situations cannot be great. Both propositions can be expressed in the following form: "If I place a piece of sodium in a certain physical situation (characterized merely by certain conditions of atmosphere and light in the one case, and by the presence of a flame in the other), and if I focus my eyes and attend to what is taking place, I shall perceive a certain event (a silvery luster in the one case, and a yellow flame in the other)." The only difference between the two situations is that the modification introduced into the first situation is not presumed to transform the end-object, while the modification introduced into the second is. If experimentation is defined as the introduction into nature of changes which would not have occurred without man's intervention, the distinction between observation and experimentation becomes quite obliterated. For man is himself a natural object, and whenever he observes in a controlled manner he introduces changes into nature. None but the most passive sort of observation, therefore, would be nonexperimental in character. As a working distinction between the two, one may say that experimentation involves control over both the observer and the observed, while observation involves control only over the observer.

The so-called experimental methods of John Stuart Mill[1] have application in this connection, though their use is somewhat more limited than the prominence given to them by logicians would lead one to believe. In their narrowest scope they have reference to the problem of the ascertainment of causes from effects, or of effects from causes. In their broadest scope they may be considered as techniques for the determination of laws, or correlations (of which causal correlations are a species). They are not, properly

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[1] *System of Logic* (7th ed., London: Longmans, Green, 1868), Book III, Chaps. VIII, IX.

speaking, methods for the *discovery* of conditions, as their name implies, but rather methods of *verification;* they are useless unless the investigator has some conception, however vague, as to what kind of event may be the condition which is being sought after. They are not, therefore, principles which enable one to infer conditions from consequents, or the reverse; they are rather principles of location. Based, as they are, upon the general kind of connection which condition and consequent bear in nature, they guide experimentation in the sense that they instruct one where and when to look for conditions, given consequents, and where and when to look for consequents, given conditions. This makes them important techniques, especially of infirmation. What should be insisted upon with reference to them is that their use is somewhat more limited than their name implies.

An important problem in confirmation is that which arises when the predicted consequences fail to receive corroboration, i.e., when they are infirmed rather than confirmed. The usual procedure is to reject the hypothesis from which the deductions were drawn. It is commonly recognized that many confirmations are required to establish an hypothesis, but one single infirmation is sufficient to disestablish it. This is based upon the fact that the hypothesis is the sufficient, but not the necessary, condition for the predictions. Hence the failure of the predictions permits one to deny the hypothesis, but the occurrence of the predictions does not permit one to affirm the hypothesis, for there may be alternative hypotheses each of which is sufficient.

But what has not been so commonly recognized is that there are alternatives to the complete rejection of the hypothesis in cases where infirmation occurs. In the first place, the hypothesis need not be rejected but may require only modification. Every hypothesis is merely an aggregate of propositions characterizing the hypothetical entity, and as such suffers no serious shock if some members of the aggregate are eliminated and replaced by others. The infirmed prediction may have followed from certain of the propositions, but not from others. All that is required, therefore, is to remove the undesirable elements, and replace them by propositions which will have other implications. It is a noteworthy fact that in the history of science no hypothesis seems ever to have been completely discarded; it may have gone through such extreme modifications as to be hardly recognizable, but there is usually a permanent core which persistently resists infirmation. This is probably that part which has been derived through the method of construction. Around this center there gathers through successive acts of imagination, a variety of fringes, each of which determines certain predictions, to be subjected to verification. Those predictions which receive confirmation become incorporated into the hypothesis as part of its essential core; those which receive infirmation are rejected and replaced by alternates. In this way the hypothesis expands and contracts, meanwhile growing progressively in such a way as to retain both past successes and past failures. It is still convenient, at times, to explain in terms of the geocentric theory of the heavens, the caloric theory of heat, the fluid theory of electricity, and the homuncular theory of the origin of life. All of these theories did actually explain at one time, in spite of misleading and erroneous implications which can be drawn from them. Progress in science consists not in the complete abandonment of infirmed theories, but in their modification in such a way as to retain their positive explanatory features.

In the second place, infirmation may require not modification or rejection of the hypothesis itself but merely a reexamination of the reasons for supposing that the hypothesis did imply the proposition which was infirmed. It has already been shown that there are no rules for the drawing of contentual implications. When the hypothetical entities are such as have never been observed, one is quite unable to assert with certainty anything as to their consequences. Hence an infirmed prediction may prove to have been erroneously drawn from the hypothesis. The amateur detective often finds to his dismay that the concept "murderer" does not strictly imply the many things which he supposed it to imply, and consequently that the one who according to his reckoning could not have committed the murder is the one who really did. Even empirical generalizations are subject to exception, and these generalizations which are founded on the imaginative anticipation of what would occur from a state of affairs nowhere realized cannot be expected to have a high degree of necessity. Hence it is always advisable to reconsider the implicative relation before rejecting the hypothesis.

In the third place, infirmation may be due to the failure to exert proper control either over the experimental or over the observational set-up. All verification, as was pointed out earlier in the chapter, involves noting what happens when certain conditions, partly in the observer, and partly in the physical medium, are established. That sodium burns with a yellow flame can be confirmed if the observer places his body and mind in a certain state of readiness, and if he places a piece of sodium in the physical situation which will produce the burning. But through inadvertence the observer may fail to realize one or more of these conditions; he may fail to look when the flame occurs, or he may not be attending to the event and thus he may fail to see it, or he may not have been sufficiently careful in setting up the physical situation. Under any of these conditions the predicted yellow flame may fail to occur for the awareness of the observer. Examination of these conditions should therefore be made before the hypothesis is rejected. Such examination involves, on the part of the observer, the ascertainment of whether normal conditions of observation prevail; the establishment of normal conditions in the case of the movement of verification is more difficult than in the case of the movement of discovery, for preconceived ideas are important sources of error; the observer is prone to read into the situation that which he expects to occur. But the examination also involves, so far as nature itself is concerned, the ascertainment of whether the conditions demanded by the experiment have actually been realized. This is a complicated problem requiring both knowledge of the situation and skill in manipulation of instruments. Only those who have experienced the chagrin of a carefully prepared experimental lecture-demonstration—which failed at the critical moment to function as predicted—can appreciate the complexity of conditions which are here relevant. What is to be emphasized at this point is merely that in a case of infirmation a careful examination of all of the observational and experimental aspects should be made before the hypothesis is rejected.

With this discussion of verificatory techniques the consideration of problems in the logic of science ends. The attempt has been made merely to outline the structure of scientific method and doctrine and to indicate the main types of problem which arise. Concrete application of many of the principles here disclosed will be made in Part Two.

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PART TWO

PROBLEMS IN THE ANALYSIS OF THE CONCEPTS OF SCIENCE

CHAPTER XII

PROBLEM OF THE ANALYSIS OF SCIENTIFIC CONCEPTS

The problems which are associated with the analysis of the basic scientific concepts are, perhaps, the most difficult in the philosophy of science. There are several reasons for this. In the first place, consideration of problems of such an abstract character requires a skill in the analysis and manipulation of highly general and highly technical notions which is comparatively rare. In the second place, the problems themselves demand for their solution a peculiar combination of logical acumen and metaphysical insight; questions of the nature of explanation are interfused with questions of the nature of the given; and there arises the problem of distinguishing that part of the meaning of any concept which is due to operations of the mind performed upon the data, and that part which is due to the data themselves. In the third place, problems of this type demand a greater familiarity with the content of science than is the case either with problems of method or with speculative problems; in order to analyze symbols as they are used in science one must know how they are used in science. In the fourth place, and most important, the problems themselves have not yet received any precise formulation; authorities seem to agree that there is a significant task to be performed here, yet they seem unable to come to any agreement as to the exact character of the task or how it is to be undertaken. As a result, one is obliged either to fumble along without any well-defined conception of the proper procedure, or to formulate the problem according to his own ideas even at the risk of being charged with an essential misconstruing of the task.

As a consequence, no universality can be claimed either for the formulation of the problem or for the illustrative solutions which are presented in the following pages. What is offered is merely a proposal. The attempt has been made to define the problem in a way which will make intelligible the many writings on the subject. A common conception of the task is, I believe, discernible in the mass of material, though the authors themselves are not always clearly conscious of its character; the general problem and the outlines of its solution seem to be implicitly rather than explicitly recognized. The proposed formulation may be examined immediately.

GENERAL CHARACTER OF PROBLEM

The terminology in which the problem is expressed is unfortunate. Properly speaking, the question is not merely one of *concepts* but one of *propositions* as well. Science is based not only upon unexamined ideas but on uncriticized beliefs. Hence the problem is to determine not merely what the concepts of science mean but what the justification is for the mass of assumptions about an external world, a uniform nature, the existence of causes, and so on. Concepts, of course, are neither true nor false; the only legitimate problem in connection with them is one of meaning. But propositions, or judgments, are both meaningful and either true or false; hence questions of both kinds may arise in connection with this type of symbol.

But from a more general point of view the differences between these two types of problem are not great. For both meaning and truth are determined by essentially the same techniques. A concept is meaningful and a proposition is both meaningful and true either extensionally, i.e., by reference to the realm of events, or intensionally, i.e., by reference to other symbols. Hence the problem is primarily one of establishing the relationships which any given symbol (concept or proposition) bears, on the one hand, to events, and, on the other, to symbols.

A recognition of this common feature seems to place the problem in its proper perspective. The problem of the analysis of the concepts of science is approximately as follows: *To clarify the meanings of vague concepts, and to establish the truths of doubtful propositions by showing the relations which they bear either to events, or to concepts whose meanings are more clear and propositions whose truths are more certain.* It may thus be considered as a problem of "reduction," for something which exhibits important deficiencies or inadequacies is replaced by something else upon which it in some sense depends and which lacks these deficiencies and inadequacies. It is obvious, for example, that meanings of words can be clarified by pointing to the objects which they designate; the meaning of the word "time" can be understood if one can discern from the background of events that general feature which is time. Meanings can also be clarified by the method of definition; the meaning of the word "motion" can be understood if one points out that it is equivalent to "change of space with respect to time." Truths can be ascertained by referring to facts; for example, the truth of the proposition "This substance is yellow" can be determined by examining the substance. But truths can also be ascertained from other truths; for example, the proposition "$2 + 2 = 4$" is true by virtue of the postulate system which defines the natural numbers and the operation of addition. Thus one clarifies meanings and justifies truths by disclosing relationships to other entities. Just why this activity should be called "analysis" is somewhat hard to see; it is probably not such in the vague sense in which a whole is analyzed into its parts. But if one considers that every symbol participates in a large complex of relationships, both to events and to other symbols, he realizes that certain of them must be more highly determinative of meaning and truth than others. Analysis is the act by which those relations which are significant in the ascertainment of the meaning and truth of a given symbol are selected. A man may have relations to his friends, business associates, wife, and children, but when he is spoken of as a "husband" the relation to his wife immediately becomes selected as essential. The problem of the analysis of the basic concepts of the sciences is concerned with a similar selection of relationships with regard to symbols in general.

However, if the problem is thus formulated it can be for the purposes of science greatly simplified. Science is definitely empirical in its ultimate reference. As a consequence the essential reference of any symbol is to the realm of events, and both truth and meaning must be determined by this route. The problem of truth and meaning cannot be solved finally by reducing symbols to other symbols, for the resultant symbols are themselves inadequate unless their empirical reference can be readily ascertained. Hypotheses are both defined and verified in terms of data, as has already been seen. No hypothesis can be clarified by defining it in terms of another hypothesis which is equally vague, and no proposition can be justified by deducing it from other propositions whose truths are equally doubtful. The only alternative to this principle is the admission of innate ideas and self-evident propositions—both of which are contrary to the empiricism of science. Hence extension predominates over intension in the ascertainment of truth and meaning, and the ultimate reference of any symbol is to the realm of events.

This is not, of course, to deny that the operations which are employed in the determinations of both meaning and truth may be very elaborate. In fact, it is the formulation of these operations which is the core of the problem. An important question to be asked with regard to every symbol is, "To what events does it refer?" But a still more important question is, "How does it refer to these events?" The meaning and truth of a symbol are a function not merely of the events to which it refers but also of the manner of referring to these events. Symbols are *descriptions*, *classifications*, *correlations*, *associations*, *generalizations*, *abstractions*, *measurements*, *idealizations*, *limits*, and *interpolations*. All of these are descriptions of the way in which symbols have been obtained from events, and, consequently, of the way in which they refer to events. Hence to tell what events a symbol refers to is to tell only a part of the story.

The task of the analysis of scientific concepts can be defined as the attempt to answer three questions with reference to each of the basic notions and each of the assumptions of science. (1) To what, presumably, does the concept or proposition in question ultimately refer in the realm of events? This may be called the problem of the *empirical foundation*, or of the descriptive reference of the symbol. (2) What does science itself consider to be the meaning and truth value of the symbol? This may be called the problem of the *scientific content*, or the scientific status of the symbol. (3) What is the character of the relation which the symbol, considered in its scientific meaning and truth value, bears to the realm of events? This may be called the problem of the *relation of the scientific content* of the symbol *to its empirical foundation*.

A quotation from C. D. Broad illustrates the problem as thus formulated. He is discussing the character of our knowledge of space as obtained through common sense. "We notice that all the information gained in this way is extremely crude, as compared with the concepts that we use in geometry and apply in physics. We see and feel finite surfaces and lumps of complicated shapes, not the unextended points and the lines without breadth of the geometers. And the spatial relations that we can immediately recognize between outstanding patches in our fields of view are equally crude. They are not relations between points and straight lines, but between rough surfaces and volumes. . . . These crude objects of sense-awareness do have properties that are evidently spatial, and . . . we can see in them the germs of the refined notions of points, straight lines, etc. The question is: 'How are the refined terms and their accurately definable relations, which we use in our mathematics and physics, but cannot perceive with our senses, connected with the crude lumps or surfaces, and their rough relations, which we actually do sense?' . . . What we want to do is to analyse finite figures and their fearfully complicated perceptible relations into sets of terms with simpler and more manageable relations. If we can do this successfully we shall have killed two birds with one stone. We shall have done full justice to the spatial properties of what we can perceive; for our analysis is supposed to be exhaustive. And, on the other hand, we shall be able to grasp these properties and to reason about them in a way that was impossible while they remained in the crude unanalysed state in which we meet them in sense-awareness." [1] Though the three problems are not specifically distinguished in this quotation, the threefold character of the pursuit is clearly evident. One is obliged, first, to describe the rough surfaces and volumes of immediate experience; secondly, one must define the refined points and lines of mathematics; thirdly, one should show how the latter are derived from or reducible to the former.

An essentially equivalent formulation of the problem is to be found in Russell. "The first thing that appears when we begin to analyse our common knowledge is that some of it is derivative, while some is primitive; that is to say, there is some that we only believe because of something else from which it has been inferred in some sense, though not necessarily in a strict logical sense, while other parts are believed on their own account, without the support of any outside evidence. It is obvious that the senses give knowledge of the latter kind: the immediate facts perceived by sight or touch or hearing do not need to be proved by argument, but are completely self-evident." [2] Data of this kind may be called "hard," or psychologically primitive, and are to be contrasted with "soft" data, which are psychologically derived. "Psychologically, a belief may be called derivative whenever it is caused by one or more other beliefs, or by some fact of sense which is not simply what

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[1] *Scientific Thought*, pp. 35–36.

[2] *Our Knowledge of the External World*, p. 68.

the belief asserts. Derivative beliefs in this sense constantly arise without any process of logical inference, merely by association of ideas or some equally extra-logical process." [1] Hence it is possible for a belief to be psychologically derived but logically primitive, since it is not the result of any logical deduction. "We naturally believe, for example, that tables and chairs, trees and mountains, are still there when we turn our backs upon them. . . . The belief that they persist is, in all men except a few philosophers, logically primitive, but it is not psychologically primitive; psychologically, it arises only through our having seen those tables and chairs, trees and mountains." [2] The basic problem then becomes: "Can the existence of anything other than our own hard data be inferred from the existence of those data," [3] i.e., can the existence of anything which is psychologically derived be *inferred* from the existence of something which is psychologically primitive? As applied to the field of physics the problem is that of "bridging the gulf between the world of physics and the world of sense." [4] The former reveals "a set of indestructible entities which may be called particles, moving relatively to each other in a single space and time" [5] while the latter reveals a mass of gross and changing objects, located in private spaces and private times. The problem is, then, "to show the kind of way in which, given a world with the kind of properties that psychologists find in the world of sense, it may be possible, by means of purely logical constructions, to make it amenable to mathematical treatment by defining series or classes of sense-data which can be called respectively particles, points, and instants. If such constructions are possible, then mathematical physics is applicable to the real world, in spite of the fact that its particles, points, and instants are not to be found among actually existing entities." [6] The similarities between this analysis and the threefold approach suggested above are obvious.

[1] *Ibid.*, p. 69.
[2] *Ibid.*, p. 70.
[3] *Ibid.*, p. 73.
[4] *Ibid.*, p. 101.
[5] *Ibid.*, p. 104.
[6] *Ibid.*, p. 122.

Since the discussion in the following pages will consist of illustrations considered from the point of view of the threefold analysis, a brief consideration of each of the subproblems will be of value at this point.

(1) The first problem is essentially metaphysical, or, at least, naturalistic. It is a problem of finding events of a certain kind. Presumably, when the scientist uses such words as "time," "space," "substance," "number," and "cause" and when he asserts that nature is uniform, he is referring to natural objects. The events to which he refers may be very elusive, and hence not open to direct inspection. In fact the existence of a *problem* in connection with such concepts and propositions suggests that their reference to events is not of an obvious kind. Certainly one must not be led to expect nature to reveal precisely the kind of thing that science means by these terms, and it may prove to be the case that nature reveals something which is only very remotely like the scientific notion. One must recall that the mind often operates upon empirically given entities and introduces elaborate modifications and transformations. But it seems likely that science does not begin with just nothing. Perfect points, lines, and levers, ideal gases, frictionless motion, isolated individuals, economic cities, utopias, and the like are not pure imaginative fancies but modifications of actualities. The mind is able to abstract, generalize, pass to limits, and form associations, with a freedom which is apparently very great, but it always seems to start with something which is empirically given and which exhibits a certain plasticity to modifications in these directions. Hence the problem of primary importance is to determine what this empirically given stuff is. The task is essentially descriptive, though it often involves the discovery and clarification of data which have been *implicitly* rather than *explicitly* recognized. Consequently it is a problem which, as will be seen immediately, offers serious difficulties.

(2) Though the first problem is metaphysical, the second is partly scientific and partly logical. It involves the exam-

ination of actual scientific doctrine and procedure with a view to determining the meaning of certain concepts such as "time," "space," "matter," "number," and "cause," and the truth value of certain propositions such as the assertion of the existence of a uniform and objective nature. Unfortunately, however, this problem is also complicated by the fact that the concepts and propositions in question function in science in an implicit as well as an explicit way. Hence it does not suffice merely to ask the scientist what he means by these symbols, for he may never have taken the trouble to think about them consciously. Consequently, he may not be able to tell what they mean, or what he says about them may be contradicted by the way in which he uses them, or his description of them may involve a circular process such as Eddington describes [1] as the cyclic method in physics, in which potential is defined in terms of interval, interval in terms of scale, scale in terms of matter, matter in terms of stress, and stress in terms of potential again. The point is that the problem of determining the meaning of such symbols is not merely a scientific problem but also a logical problem. It involves such considerations as the analysis of assumptions and preconceptions, the examination of the structure of logical systems, and ascertainment of the function of measurement in the sciences. It should reveal, for example, in combination with the problem of the empirical foundation of scientific concepts, the essential difference between the *time* which is assumed by the physicist and the *time* which is revealed in direct perception as the actual going-on of events, or the *space* which is implicit in geometry and the *space* observed as the relations of distance and direction between natural objects. The fact that the same word is used to describe the two notions in each case does not imply that the notions are identical.

(3) The third problem is almost entirely logical, though the border-line between the logical and the psychological aspects is not sharply drawn. It may be formulated either

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[1] *Nature of the Physical World*, pp. 260–265.

as the attempt to determine the operational *derivation* of symbols, or as the task of describing the techniques of the *verification* of symbols. Both of these formulations consider the symbols from the point of view of the routes by which they are connected with events, and attempt to relate the symbols as employed in science with their empirical foundations. The object of this correlation is to reveal any discrepancies between the two, and to prepare the way either for showing how the scientific notion could be obtained from its empirical foundation by the employment of certain describable operational techniques, or for showing how the scientific notion could be verified in terms of its empirical foundation by the use of certain techniques of verification. The differences between the derivational techniques which are psychological and those which are logical are not important. In general those which are unconscious, spontaneous, and varying from individual to individual are considered to be psychological, while those which are at least partly reflective, more or less deliberate, and relatively common to many minds are considered to be logical. The logical in this connection is only the psychological which has attained a certain standardization, deliberateness, and social approval. Logical techniques are psychological techniques which have achieved a certain formulation and thus are available as tools for the attainment of ends. Verification techniques may also, of course, be either psychological or logical, though the logical techniques are usually emphasized. In the discussion which follows, the derivational as over against the verificatory techniques will be emphasized. The justification for this approach to the problem lies in the fact that verification is never possible unless *some* meaning has already been given to the symbol in question. Hence the derivational problem is not only temporally prior but also, in a sense, logically prior to the problem of verification.

Though the proper formulation of the problem is an important step toward the determination of its solution, it is only a first step; the difficulties to be encountered in the various stages of the process are great. The remainder of the chapter will be devoted to a consideration of the most important of these difficulties.

DIFFICULTIES IN ASCERTAINMENT OF EMPIRICAL FOUNDATION

The first difficulty is one to which reference has already been made in connection with the difference between descriptive and explanatory sciences, viz., the fact that the empirically given is a variable, and fails to exhibit precise boundaries limiting the range of "hard" data. Russell himself admits that the distinction between "hard" and "soft" data is one of degree.[1] The given exhibits a strict continuity with reference to the clarity or obviousness of its elements. Some data are more clearly given than others, but may be less clearly given than still others. Hence one must admit something like levels in the empirically given. For example, it seems certain that the private perceptual space of the individual is more clearly given than the one universal geometrical space presupposed by science. But the application of analysis reveals the fact that the private perceptual space of the individual is itself a construction out of visual, tactile, and motor spaces; hence the private space which was presumed to be psychologically primitive turns out to be psychologically derivative. Similarly, the scientific concept of order seems to have its empirical foundation in the perceived relation of "betweenness"; yet this relation may itself be considered as a construction out of more primitive data constituting the merely directional features of events, e.g., right and left, above and below, etc. The consequence of these facts is that the description of the empirical foundation of a concept or proposition implies always a conscious recognition that the level which is being chosen as basic is only relatively so, and may prove upon further examination to be reducible to one still more primitive.

In the second place, one must be reconciled to the fact

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[1] *Our Knowledge of the External World*, p. 70.

that discourse about events on the empirical level is essentially vague. This is due partly to the fact that all definition must be in popular terminology, which is essentially obscure. One must talk about time, for example, as the "going-on," or the "passage," or the "flow" of events; one must describe space as the "spread-out-character" of events; one can explain a class only as a "heap," "aggregate," "collection," "pile," or "ensemble"; and one is obliged to characterize matter by describing it as "stuff," or "substance." But the vagueness is due to a much more important cause. Definition, in the strict sense, is not possible on the empirical level. The clearness with which events are given is a perceptual rather than a conceptual clarity, i.e., an extensional rather than an intensional clarity. Definition is possible only when symbols have been integrated into systems; but on the empirical level this has not yet taken place. Symbols must be clarified by the method of pointing, but not by the method of definition. The function of discourse at this stage is that of directing the attention to the place within the realm of nature where the events in question can be found, so that their characters can be determined by direct inspection. Proper names, gestures, and pictorial symbols are often more effective than abstract concepts. Discourse is possible only in the sense that one may "talk about" the symbols in question. One must proceed as did Poinsot, according to Claude Bernard:[1] "'If anyone asked me to define time, I should reply: "Do you know what it is that you speak of?" If he said, "Yes," I should say, "Very well, let us talk about it." If he should say, "No," I should say, "Very well, let us talk about something else."'"

But, in the third place, in addition to the ineffectiveness of the method of definition and the consequent resort to the method of pointing, one finds that the application even of this latter method is not without difficulties. Pointing is ambiguous. Events are highly complex entities and there

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[1] A. J. Lotka, *Elements of Physical Biology* (Baltimore: Williams and Wilkins), p. 19.

is no way of directing one's gestures in such a way as to indicate the phase or aspect of events to which he wishes to call attention. For example, one cannot by means of pointing distinguish the smoothness from the hardness of an object, or the shape from the size. One cannot, even by means of proper names and pictorial symbols, direct the attention of an individual unambiguously to the temporal feature of the world, for time is a phase of objects and one can never be assured that the listener has not confused the temporal phase with some other with which it is associated. Similar difficulties arise in the attempt to locate instances of number and order. For example, the clarification of the concept "twoness" may be attempted by pointing to a pair of objects. But a pair of objects is a highly complex event, of which not only the event as a whole but each of the partial events exhibits spatial and temporal features, shape, size, color, and so on; consequently there is no way of indicating that it is the duality of the event and not one or more of the associated aspects to which reference is being made. As a result, the reduction of scientific concepts to their empirical foundations and of assumptions to the empirical facts which they presume to justify cannot be carried out with any assurance of success. In case dispute arises as to what *is* given, almost no means of settlement are available. It seems likely that the disagreement between positivists and necessitarians on the question of the empirical foundation of causation is essentially of this kind; one group maintains that nature exhibits only *sequences*, the other insists that it discloses *intrinsic connections* as well. In a case of this kind, resort to the method of pointing is useless, yet similar difficulties arise in connection with the attempt to point out the empirical foundations of all of the basic concepts.

DIFFICULTIES IN ASCERTAINMENT OF SCIENTIFIC CONTENT

The second of the problems gives rise to further difficulties. Analogous to the variability in the given there is a variability in the levels of abstraction at which the scientific concepts may be analyzed. For example, time and space on the level of the Newtonian physics are not the same as the time and space of relativity; and number may be defined in such a way as to include only the positive cardinal numbers, or it may be generalized successively so as to include the negatives, the fractions, the irrationals, and the imaginaries. The situation is even more confusing than this, for by time and space may be meant simply the measured values obtained by reading clocks and scales, and by number may be meant simply the rules according to which certain arbitrarily chosen marks may be combined and substituted for one another. At these extreme levels it seems safe to say that the scientific concept has more or less completely lost its content; by attaining extreme generality of application it has lost the features which distinguish it from any other concept. The movement of science seems to be always toward greater and greater abstraction. For example, the attempt is made to identify the concepts of mathematics with purely logical notions, the concepts of mechanics with the concepts of mathematics, the concepts of physics with the concepts of mechanics, and so on. Hence one must recognize the importance of stating clearly at the outset what stage of abstraction is being considered in his analysis; conclusions which are applicable at one stage may not be applicable at another.

Since this is an important difficulty, a few remarks may be made relative to the position which will be taken in the following chapters. Though much is to be gained by considering the concepts at a high level of abstraction, much is also lost. Consider, for example, the use of measurement techniques in science. By this process a qualitative event, e.g., a force, a duration, or a motion, is replaced by a number corresponding to the reading on an instrument; the force is replaced, say, by the number 5 which represents so many dynes, the duration by the number 2 which represents so many seconds, and the motion by the number 3 which represents so many centimeters per second. The presumption of this view is that an entity can enter into physical science only if it can be measured; physics talks only about quantities, not about qualities. The outcome of the view is that the subject matter of physics is pointer readings, a position which has been made prominent in recent years by Eddington.[1] Time then becomes *identified* with clock-readings, space with readings on meter sticks, force with readings on spring balances, and so on. But what should be remembered is that this does not reduce time, space, and force to mere *numbers;* time is the number obtained from a clock, but a clock is different from a meter stick and a spring balance, hence the *nature of the instrument* must be taken into consideration in defining the measured value. This means that the actual techniques of *using the clock* in an experimental set-up, and even the techniques employed in *making the clock* in the first place, are part of the *definitions* of the measured values. This makes the reduction of physics to mathematics impossible. Physics is meaningless apart from clocks, meter sticks, and spring balances; and all of these instruments must be constructed and calibrated by perceptual differences in qualitative experiences. To lose these qualitative aspects in the abstractions of science is to make them essentially meaningless.

The problem here involved is very complex, and cannot be solved at this point.[2] What should be stated is merely the point of view which will be taken in the ensuing chapters. In what follows the concepts will not be taken at the high level of abstraction which reduces them to mere numbers. Recognition must always be made of the fact that the number is a *measured value of something*, and this something must be retained in a qualitative form. This is to deny not the legitimacy of measurement, but only the exhaustiveness of the reduction of quantity to quality. The distinctions

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[1] See below, Chapter XX.

[2] See A. C. Benjamin, *Logical Structure of Science* (London: Kegan Paul, 1936), Chap. XIV.

between time, space, and force remain in science even though they are all "reduced" to measured values. Similar considerations apply to that high abstraction of numbers which generalizes them into meaningless marks manipulated according to arbitrary rules.

In the second place, one must recognize the difficulties which arise in the attempt to clarify the meanings of the concepts and assumptions, even from the point of view of the way in which they function in the science. Attention has already been called to the fact that they are often implicitly rather than explicitly present, and hence may never have been consciously clarified even by the scientist himself. In addition there are difficulties due to the technical character of the terminology which is often employed in the definitions of these notions. But most important is the recognition of what is involved finally in the very method of definition itself. A definition is a statement of equivalence between two symbols, one of which is better understood than the other; hence definition involves the reduction of an obscure symbol to a clear one. "Now, obviously the process of definition can not be led back *ad infinitum.* It must terminate in the undefined, or indefinable. Any discussion must begin with certain *primitive* concepts from which it proceeds in an orderly way. Psychologically, it would always be best to take as undefined those concepts which are most clearly understood; but logically, such a procedure might be very limiting indeed, being confined by the circumference of the understanding to which we appeal. What is best taken as primitive (or undefined) may therefore be some extremely complex idea, so far as understanding is concerned. A glance at any mathematical system easily convinces us that this is the case." [1] Thus definition is not usually a reduction to the psychologically primitive but to the logically primitive, as Russell uses these terms in the quotation given on page 238. Broad points out that what is logically most primitive in nature is not what is

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[1] R. M. Eaton, *General Logic* (New York: Scribners, 1931), p. 298.

now most familiar to us.[1] Hence we may find that the techniques should be called *logically* rather than *psychologically* elucidating. From this point of view there is relevance in the following quotation from D'Alembert:[2] "The most abstract notions, such as the majority of mankind regard as the most inaccessible, are often those which carry with them the greatest elucidating power: our ideas seem to be blotted out by obscurity in proportion as, in any object, we examine into its sensible properties." Unless the distinction between logical and psychological elucidation is kept in mind, this quotation is in essential conflict with remarks made in connection with the problem of the empirical foundation. Abstract ideas are logically clear but empirically obscure; sense-data, on the other hand, are empirically clear but logically obscure.

Finally, the clarification of the basic concepts and assumptions of science cannot possibly be made through the method of pointing. Here the difficulty is not merely a matter of the ambiguity in the denotative gesture, but a matter of the inherent elusiveness of the entities in question. In fact many scientists, as has already been seen, give to all such entities a fictional status and refuse to find a place for them in nature. They are essentially mental creations, and have either no connection at all with the empirical realm or else are so remotely related that it is absurd to look for their empirical counterparts. One should not look for an infinitely extended time in nature any more than he should look for a perfect lever. Abstract space, number, order, and the like are not natural entities but conceptual schemes for the interpretation of nature, almost identical with the Kantian forms of perception and understanding. While not all scientists would go to this length in removing abstractions from the realm of the given, most would agree that if such entities do exist they must do so in a manner which is quite different from that of ordinary events. Consequently they

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[1] *Scientific Thought*, p. 26.

[2] Quoted by T. Ribot, *Evolution of General Ideas* (Chicago: Open Court, 1899), p. 201.

cannot be easily located in space and time, and they do not exhibit the ordinary sensible qualities. This is merely to say that they are inherently obscure as empirical entities.

DIFFICULTIES IN ASCERTAINMENT OF OPERATIONAL DERIVATION

The main difficulty in the ascertainment of the operational routes by which the scientific symbols are derived from their empirical foundations is the necessity for a previous knowledge of the main types of operation which may be performed. The techniques which have been employed in a specific case can be more readily recognized if one knows what the most common techniques are. But no satisfactory classification of the operations of symbolic derivation has ever been made. The list given in Chapter IX was merely illustrative and made no pretension to completeness. As will be recalled, the main types there mentioned were *abstraction* (including measurement), *concretion*, *serial operations*, *cause* and *effect inferences*, and *operations of analysis* and *synthesis*. It will be the task in the following chapters to illustrate the uses of some of these methods with reference to certain of the concepts. A more adequate classification would insure a more accurate analysis.

Another difficulty is the fact that alternative operations are often employed to derive concepts which are verbally the same. The best known example of this is the two attempts to derive the concept of the mathematical point. According to the one method it is defined as the limit of a series of gradually decreasing volumes; according to the other it is defined as the enclosure property which is exhibited by events through their capacity to extend over other events and thus to generate a series. The former route is that of serial extension, the latter is abstraction. Irrationals, such as $\sqrt{2}$, possess similar alternative routes of derivation; they may be defined either as the limits of certain series of increasing fractions, or as the class of such fractions. The concept of force may be derived either as the cause of motion or as the measure of motion; these suggest alternative routes of derivation. The verbal similarity is a source of error, for if the routes of derivation are different the concepts themselves would presumably be differently defined; at least the concepts should be assumed to be different until the routes of derivation have been demonstrated to be equivalent.

In the third place, a proper understanding of the operational derivation of any symbol demands that one ascertain whether the method has been that of construction or that of hypothesis. The distinction between these two methods has already been clarified.[1] But the decision in any given case is difficult to make. A symbol may be considered a construction if it contains only such content as may be derived from its empirical foundation through the use of the required operation; but a symbol may be considered an hypothesis if it contains such additional content as must have been obtained through the use of creative insight. The distinction is, of course, a fine one, and requires a precision in techniques which is not easy to attain. Furthermore, the difficulty is augmented by the fact that, although one may know in a general way what the operational derivation has been, scarcely anyone knows specifically what the rules for the employment of the operation are; hence he is not in a position to estimate the operation as having been correctly or incorrectly performed. The point is, simply, that one does not know in any very precise way what abstraction, association, serial extension, and the rest are; consequently he is not able to formulate any rules according to which symbols should be given meanings through the use of such techniques. As a further consequence, therefore, one is not able to say when a symbol has been increased through imaginative activities. This makes the discussion of the operational derivation of scientific symbols essentially unsatisfactory. The best that can be done is to throw out certain suggestions, indicate obvious inadequacies, and

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[1] Chapter IX, pp. 185–186.

point out further directions for research. This represents the condition of the problem today.

Since the following chapters are considered merely as illustrative, they will be confined to the problem of the meaning of the scientific concepts, and no reference will be made to the problem of the validity of assumptions. The procedures are essentially the same in the two cases. The neglect of this latter problem is merely a convenience of presentation, and should not be considered as a judgment as to its unimportance.

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CHAPTER XIII

ORDER, NUMBER, QUANTITY

The concepts which will be subjected to analysis in this chapter are those which are presumed to lie at the basis of mathematics. Whether they are properly mathematical concepts or not, however, need be of no concern. The point of view seems to be gaining ground in recent years that mathematics has no specific subject matter, but is constituted merely by its form. The famous definition of Benjamin Peirce, "mathematics is the science which draws necessary conclusions,"[1] and the views of Russell, Wittgenstein, Hilbert, and others are all in accord with this claim. Fortunately, the dispute need not be settled here. All that is required is the recognition that the concepts of *order*, *number*, and *quantity* are implicitly present in a large part of scientific investigation. They are thus basic in the sense that a recognition of their natures is important to an understanding of the sciences.

The order in which the concepts should be discussed is more or less arbitrary. If one's concern were with the establishment of a postulate scheme in terms of which the concepts could be defined, the decision as to order would be important. The more complex of the terms should be defined by means of the less complex, and the order of discussion would therefore coincide with the order of logical derivation. But since the present concern is also with the problem of empirical foundation, the issue is not so important. As has already been shown, the concepts which are empirically most obviously given are not necessarily those which are logically most basic. Hence it is of pedagogical convenience to examine the concepts roughly in the order of psychological accession rather than in the order of logical deducibility.

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[1] *American Journal of Mathematics*, Vol. iv, 1881, p. 97.

ORDER: EMPIRICAL FOUNDATION

It seems obvious at the outset that nature does exhibit situations which can be spoken of as *orders*, or *arrangements*, and the initial problem is to select events of this kind and examine their properties. Common sense is assured that complexes such as the following should be spoken of as series: the days of the week, the months of the year, points on a line, the floors of a building, the colors in the spectrum arranged in the usual way, individuals arranged according to height, automobiles arranged according to cost, and so on. Spatial and temporal relations are predominant in serial situations, though they are not usually supposed to *define* the orders in question. For example, the individuals in a group may be said to have serial properties by virtue of their differences in weight, yet individuals need not be arranged in an actual order with the heaviest at one end, the lightest at the other, and all intervening individuals placed properly on the line joining the two. In the same way, one may think of his pleasures as having an ordering of intensity, though no physical arrangement of them would be possible. Order, therefore, seems to be applicable to complexes whose elements exhibit certain characteristic relationships, and the problem is to determine the nature of these relationships.

Agreement can be reached, in the first place, on the fact that *order is applicable only to events exhibiting a certain complexity*. A single, unitary event may not be ordered; such an event may itself have *position* in a series, but it is then considered in its relation to other events, and the total group of events exhibits order. Serial arrangement is possible only with reference to the elements of a complex event. But how great must the complexity be; e.g., would a two-term complex suffice? The answer to this question depends upon the level at which the empirical analysis has been made. There is some justification for the contention that order exists wherever there is an asymmetrical relation;[1]

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[1] For the definition of this notion see p. 123.

for example, Monday and Tuesday have an actual order which would be different if Tuesday occurred before Monday; similarly *father* and *son* may be said to have a certain order by virtue of the fact that the father begets the son but the son does not beget the father. The question then arises as to whether the asymmetrical relation is psychologically more primitive than a three-termed relationship, such as Monday-Tuesday-Wednesday, or father-son-grandson, and whether the greater complexity of this latter type is essential to order. Since the question is largely a verbal matter at this point it may be answered arbitrarily by suggesting that order be denied empirical applicability to events unless they exhibit a complexity which is at least threefold.

What is required, therefore, in the second place, is to specify the character of the relational-structure which must be exhibited by a three-termed complex in order to permit the characterization of this complex as ordered. The required relational-structure may be described as one which possesses *betweenness*. *Betweenness* may be defined as a triadic relation in which there is a term such that (a) it has an asymmetrical relation to each of the two other terms, and (b) its relation to one of the terms is the converse of its relation to the other.[1] The element possessing this relation would then be said to be between the other two. For example, Tuesday is between Monday and Wednesday, since there is a triadic relation characterizing the complex, of such a kind that a certain element (Tuesday) is so situated that it has an asymmetrical relation to Monday (succeeds) and to Wednesday (precedes), and its relation to the one is the converse of its relation to the other. A similar relational-structure is exhibited by any three months in the year, any three points on a line, any three floors of a building, and so on.

The relational-structure may then be said to determine the serial arrangement. Usually the arrangement which is

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[1] This is essentially equivalent to Russell's definition. See his *Principles of Mathematics* (Cambridge: Cambridge University, 1903), p. 200.

adopted is one which is readily observable, i.e., one which employs the relational-structure of time or space. Since instants in time and points in space themselves exhibit order, they become convenient instruments for the portrayal of orders which are not directly temporal or spatial. For example, weights may be arranged by putting the least heavy at the top of a column and the others below in order of increasing weight; in this case the spatial relation of "lying beneath" is taken as the equivalent of the non-spatial relation of "being heavier than." Or pleasures may be arranged by mentioning first the most intense pleasure and then the less intense pleasures in order of decreasing intensity; in this case the temporal relation of "succeeds" is taken as the equivalent of the non-temporal relation of "being less intense than." These are illustrations of empirical series.

ORDER: OPERATIONAL DERIVATION

However, it is not the task of science merely to discern the obvious features of events. It is one of the functions of the developing intelligence to disclose hidden relations between events. But this requires greater precision in the conceptual tools which are to be employed in the search. Hence the scientist demands greater refinement in the concept of order. This refinement is accomplished, as is usually the case with the basic concepts, by an increase in *abstractness* and *generality*. The ideal is to eliminate from the empirical conception those features which are relatively specific. The goal is to retain the relevance of the concept to all of the empirical situations but to exclude all of those aspects which tie the concept down to one situation rather than another. The operation from empirical order to scientific order is the same as that from redness to color, or from color to quality in general. It is not an operation of neglect in the sense that the specific qualities are merely dropped out of consideration; rather the specific aspects are retained in a feature which is generic to them. One does not pass from redness to color or from color to quality in general by dropping out something, so that what one ends with contains less than what one started with; rather one retains *as a variable* those features which were present *as values.*

The generalization of the concept of order may be examined from several points of view, all of which are essentially equivalent. One is that of *serial extension.* It seems obvious that the relational-structure of an empirical series is not changed by the discovery of an additional element; hence a series does not become any less so by passing from a three-termed series to an n-termed series. All that is required is that the element designated as unique in the definition of "betweenness" should no longer be considered as such; if the series is n-termed, where n is greater than 3, any element except the first or the last may be the element which was formerly spoken of as unique. Hence the attempt is made to define the relation of betweenness in such a way as to make it applicable generally to the series. An equivalent result could be accomplished through an operation of *interpolation*, i.e., by recognizing that an empirical series is not changed by the discovery of a new element which would presumably lie between two elements supposed to be adjacent; the notion of adjacency is not essential to the concept of order. Through considerations of this kind many features of the empirical series are seen to be essentially irrelevant to the concept of order. It is seen that an order as such involves no specific reference to the number of terms, except that n must be at least 3 (or at least 2, if one wishes to include the directional features of nature under the concept of order); this is to say that a series does not need a first term nor does it need a last term in order to be a series. It is also seen that an order as such involves no specific reference to adjacency; this is to say that a series may have a term between any two and still remain a series.

ORDER: SCIENTIFIC CONTENT

The scientific concept of order, thus derived, is capable of precise formulation. One of the most satisfactory of the many descriptions of the concept is that given by E. V. Huntington:[1]

> If a class, K, and a relation, $<$ (called the relation of order), satisfy the conditions expressed in the postulates 0, 1–3, below, then the system $(K, <)$ is called a *simply ordered class*, or a *series*. The notation $a < b$ (or $b > a$, which means the same thing), may be read: "a precedes b" (or "b follows a"). The class K is said to be *arranged*, or *set in order*, by the relation $<$, and the relation $<$ is called a *serial relation* within the class K.
>
> *Postulate* 0. *The class K is not an empty class, nor a class containing merely a single element.*
>
> This postulate is intended to exclude obviously trivial cases. . . .
>
> *Postulate* 1. *If a and b are distinct elements of K, then either* $a < b$ *or* $b < a$.
>
> *Postulate* 2. *If* $a < b$, *then a and b are distinct.*
>
> *Postulate* 3. *If* $a < b$ *and* $b < c$, *then* $a < c$.

These three postulates may be called, respectively, the postulate of connexity, the postulate of irreflexiveness, and the postulate of transitivity.[2] The logical independence of these postulates, i.e., the fact that no one of them can be derived logically from any of the others, is shown in Huntington by illustrations of systems which satisfy each two of the postulates but not the third. As an example of a series satisfying postulates 2 and 3 but not postulate 1, he offers the class of all human beings throughout history, with $<$ defined as "ancestor of"; as an example satisfying 1 and 3 but not 2 he gives the class of all the natural numbers with $a < b$ signifying "a is less than or equal to b"; finally, as an example satisfying 1 and 2 but not 3, he suggests the class of natural numbers, with $<$ meaning "different from."

The generality of this postulate set is clearly seen in the fact that nothing is said with reference to the number of

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[1] *The Continuum* (Cambridge: Harvard University, 1917), Chap. II.

[2] The equivalence of this postulate set with the set given in Chapter VI, p. 122, may easily be shown. The principle of connexity was not there stated as a specific postulate, though it is equivalent to the assertion that an ordering relation must exist between every two members; the principles of transitivity in the two cases are identical; the principle of asymmetry which was given earlier as a specific postulate may be shown to follow from the postulates 2 and 3 as given by Huntington. See *ibid.*, pp. 10, 11.

terms, whether there is a first or last term, or whether the terms of the series are adjacent. This permits the application of the notion to infinite as well as to finite series, and to varying types of compactness. The variation in degrees of compactness is very important for science, consequently the postulates as given by Huntington for the definition of these specific types of order will also be given.

The postulates defining a *discrete series* are as follows:

Postulate N1. (Dedekind's postulate.) If K_1 and K_2 are any two non-empty parts of K, such that every element of K belongs either to K_1 or to K_2 and every element of K_1 precedes every element of K_2, then there is at least one element X in K such that:

(1) *any element that precedes X belongs to K_1, and*

(2) *any element that follows X belongs to K_2.*

Postulate N2. Every element of K, unless it be the last, has an immediate successor.

Postulate N3. Every element of K, unless it be the first, has an immediate predecessor.[1]

This is the type of order which, if it has a first and a last element, is that exhibited by the gross objects of perception; observed objects are discrete and lumpy and stand adjacent to one another, e.g., the houses on a street, the books on a shelf, and the days in a week. If the series is defined as having a first element but no last element, it is called a *progression*, and is exemplified by the natural numbers.

The postulates defining a *dense series* are as follows:

Postulate H1. If a and b are elements of the class K, and $a < b$, then there is at least one element x in K such that $a < x$ and $x < b$.

Postulate H2. The class K is denumerable; that is, the elements of K can be put into one-to-one correspondence with the elements of a progression.[2]

Postulate *H*1, called by Huntington the postulate of density, forbids one to give empirical examples of this type of series, unless one supposes that the numbers themselves are empirical objects. This postulate requires that there

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[1] *Ibid.*, p. 19.

[2] *Ibid.*, p. 34.

should always be at least one element and therefore an infinity of elements between any two. Empirical events could not constitute such a series, for at the limits of the perceptually discernible adjacency would be an observed fact. It is easy, however, at least with the techniques of mathematics, to construct series satisfying these postulates. One of the most useful is that of the class of proper fractions arranged in the usual order.[1]

An even more compact type of series is that called the *continuum*, which has important applications in the analysis of time and space. Its postulates are as follows:

Postulate C1. (Dedekind's postulate.)

Postulate C2. (Postulate of density.)

Postulate C3. (Postulate of linearity.) The class K contains a denumerable subclass R in such a way that between any two elements of the given class K there is an element of R.[2]

Some of the commonest examples of the continuum are the points on a line, the instants in a duration, the real numbers, and the non-terminating decimal fractions. It is clear that these are not empirical orders in the strict sense of the word; they are definable by means of operations having empirical application within a given range, but extended beyond that range through a principle justifying indefinite repetition.

NUMBER: EMPIRICAL FOUNDATION

That there are numbers, or at least numbered groups, seems obvious; yet the problem of the precise empirical foundation of the concept of number is one involving some difficulty. This is due at least partly to the fact that the concept of number in ordinary speech is ambiguous. It may refer, on the one hand, to a certain property of groups, or pluralities; in this sense it applies only to collections of events, and not to a single event. But it may refer, on the other hand, to the property of individuality, distinguishability, or discreteness, which is possessed by an event; in this

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[1] *Ibid.*, p. 14.

[2] *Ibid.*, p. 44.

sense it applies only to simple events, and not to complexes except as they themselves are distinguishable from other complexes. The two notions are obviously interrelated, yet they are not identical; for example, if a group has number the members of the group must be numerically distinguishable, and if an event is numerically distinguishable (from another event) there must be a group (of at least two) which possesses number; but it is clear that the property which is possessed by the individual members is not the same as the property which is possessed by the group. Before one can talk about empirical foundations, therefore, one must decide which of these two notions is the proper meaning to be given to the concept. Jevons, for example, defines number as "the empty form of difference";[1] but Russell defines it as a property of similar classes.[2]

But, in the second place, contingent upon this fact a new problem arises. Which of the two notions is psychologically more primitive? In other words, is all recognition of numbered groups a synthetic act involving a very rapid counting of individuals, or is all recognition of individuals an analytic act involving selection from totalities? Expressing the same idea crudely, does one build up the idea of "many" by combining "ones," or does one build up the idea of "one" by breaking up a "many"? It is not likely that this question can be answered with any finality. The conclusions of the recent *Gestalt* psychology suggest that there is such a thing as an immediate recognition of wholes and patterns; this would seem to argue for the fact that, in certain situations at least, plurality is psychologically more primitive than distinguishability. Yet the notion of number is so intimately associated in one's mind with the notion of counting, and the ascertainment of number in very large groups is so certainly a matter of counting, that plurality seems to occupy a secondary place in the order of experiential derivation.

Since the issue is not important one may adopt, as in the case of the concept of order, an arbitrary point of view.

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[1] *Principles of Science*, p. 158. [2] *Introduction to Mathematical Philosophy*, p. 18.

The notion of number as plurality seems to have a somewhat more direct empirical reference than the notion of number as distinguishability; hence it seems best to suppose that the immediate recognition of pluralities is possible without the employment of counting. At least there are simple empirical groups whose number seems to be discernible without a conscious act of counting, and these may be taken at least temporarily as on the primitive level of givenness. It may be that counting can be shown to be *logically* more basic than number, but this is an independent issue. The problem, then, is to determine what kinds of events have numbers, and what sort of thing number is when thus derived.

To say that number is a property of complexes or pluralities is merely to state a first approximation to an empirical definition of number; what is required further is a conception of the kind of property which number is. An understanding of the distinction between *collective* and *distributive* properties is helpful in this connection. For example, the word "all" is ambiguous in the English language; it may refer to an aggregate taken as a whole as in the proposition, "All the angles in a triangle are equal to two right angles," or it may refer to the individual members of an aggregate as in the proposition, "All the angles in a triangle are less than two right angles." This suggests that there are two kinds of properties—collective, or "holistic," properties which are applicable only to aggregates and not in general to the individual elements, and distributive, or elemental, properties which belong to the members and not in general to the totalities. Inference from a class to the members, as given in the ordinary syllogism, is based upon distributive properties; the absurdity of an inference constructed after the same pattern but with a collective property substituted for the distributive property is illustrated in the following: "Americans are numerous; since I am an American, I am numerous." The general fact to be pointed out, then, is that number is a collective property, applicable only to aggregates or complexes. In this respect it belongs in the same category as such words as "organization," "pattern," "structure," "complexity," "disorganization," and the like.

But it is important to determine the empirical foundation not merely for the general fact of number but also for specific numbers. Though all complexes possess number, they do not all possess the *same* number. When do two complexes possess the same number? Presumably, when they possess something in common. But this feature which they possess jointly is nothing very obvious. As Russell says, "it must have required many ages to discover that a brace of pheasants and a couple of days were both instances of the number 2: the degree of abstraction involved is far from easy." [1] Clearly, not just any sort of resemblance will suffice; a pair of shoes and a trio of crows may both be black, but they will not thereby possess the same number. The similarity must reside, rather, in the holistic properties. Yet how may this similarity be determined? The method which first suggests itself is to count the respective groups and determine the number of each. But this will not do, for counting itself presupposes that individual numbers have been recognized and arranged into a series. What is required is an activity by means of which the individual members may be selected from the respective groups and correlated with one another. This is called the activity of setting up a *one-to-one correlation*, and two aggregates which can be thus correlated are said to be *similar* to one another. Two classes may be said to be similar when for every member of the first class there is one and only one member of the second, and for every member of the second there is one and only one member of the first.[2] By means of this technique it is possible to ascertain that two groups have the same number without knowing *what* number either group possesses. For example, in a room in which every seat is occupied and there is no one standing, the number of individuals is the same as the number of seats; the relation "occupying" constitutes the correlating rela-

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[1] *Introduction to Mathematical Philosophy*, p. 3.

[2] If the occurrence of the word "one" in this definition is considered objectionable, it may be avoided by a different phrasing. See Russell, *op. cit.*, p. 15.

tion. In a country where neither polyandry nor polygamy is permitted, the number of husbands living at any moment is the same as the number of wives. By means of this technique all pairs could be correlated, all triads, and so on. The number 2 could then be empirically defined as the property possessed in common by all classes similar to some given class, say a pair of shoes; and the number 3 could be defined as the property possessed in common by all classes similar to another given class, say the sides of a triangle.[1]

This should suffice as an empirical definition of number. By means of such a criterion one should be able to distinguish the various types of complex from one another, group together all similar complexes, and devise the proper numerical symbol for the characterization of each type. But there is also involved in the empirical conception of number a feature which, I am convinced, must be considered as part and parcel of it. The different types of complex are recognized not only as being different in number from one another but as being derivable from one another by certain *mathematical operations.* It seems impossible that there should be an awareness of the difference between a 3-group and a 5-group without a simultaneous awareness of the fact that the 3-group would be similar to the 5-group *if it had two more members*, i.e., if it were combined with a 2-group. This seems to be the foundation for the notion of mathematical operation, which, in its general form, is a concept of great importance for science. Reference has already been made to the physical, biological, and psychological operations involved in the perceptual act.[2] In general an operation may be defined as an act performed upon something given to produce something else. Mathematically, the notion of operation is somewhat more specific; "an *operation* upon the elements

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[1] This is in essence Russell's definition. He defines number as follows: "The number of a class is the class of all those classes that are similar to it." The main difference is that Russell places more emphasis on extension; since there may not be any such property as number, while there obviously is such a thing as a class of classes, he considers that the safest procedure is to adopt the extensional definition. *Ibid.*, p. 18.

[2] Chapters V, VI.

a and *b* is defined, if, corresponding to the elements *a* and *b* and to a certain order of those elements, there exists a certain third thing *c*."[1] Thus the operation of addition upon a 3-group and a 2-group is defined, if, when the groups are taken in the given order there is determined a third group, viz., a 5-group; this operation is symbolized in the ordinary way, $3 + 2 = 5$. It seems important to recognize that certain elemental operations—addition, subtraction, and possibly also multiplication—are intimately tied up with the recognition of the simple numbers. As such they come definitely into the empirical picture, and are considered inseparable from the descriptive concept of number. Unless this is recognized a difficulty arises in the understanding of the scientific concept, for the latter makes important use of the concept of operation.

On the empirical level, then, numbers may be considered as collective properties of certain complex events which can be correlated with one another. What seems important to recognize is that the pluralities here considered have a complexity of not more than six or seven terms, and not less than two terms. At least groups of greater complexity can probably not be recognized by a simple act of attention, but must be built up by counting or by operations upon smaller groups. And groups of lesser complexity could not, according to the empirical conception, be groups. This suggests that the notion 1 is somewhat more abstract than the notions 2 . . . 6, and hence must be defined by logical extension from the latter group. The notion 0 is clearly one involving complicated acts, and arises only very late in the psychological development. The notions of the negative numbers, the fractions, and the irrationals arise by similar extension, hence are not to be examined at this level of analysis. Mention may be made further of the fact that the aggregate of complexes which constitutes the empirical reference of the concept of number has not yet been ordered;

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[1] J. W. Young, *Fundamental Concepts of Algebra and Geometry* (New York: Macmillan, 1927), p. 88.

in other words, one does not yet have the series 2, 3, 4, 5, 6, but merely the collection 2, 5, 4, 6, 3. One is, therefore, not yet able to count.

NUMBER: OPERATIONAL DERIVATION

The concept of number, like the concept of order, accomplishes its task in science through generalization of its empirical foundation. But whereas this was attained in the case of order through a direct generalization of the concept itself, it is brought about in the case of number through a generalization of the notion of operation.

It should be apparent that on the empirical level the notion of mathematical operation involves the conception of *illegitimate* operations. While addition may always be performed, since it produces a further complex which may then be presumed to exhibit number, subtraction often cannot be performed; for example, a 2-group cannot be subtracted from a 3-group, for what would remain would not be a group at all, hence could not be numbered; furthermore, a 3-group could not be subtracted from a 2-group for the act could not be performed. Similarly, though multiplication is always possible, division is generally impossible. Hence a proper formulation of the empirical conception would require a classification of operations on the basis of legitimacy and illegitimacy. It is this distinction which is abandoned at the scientific level. The generalization of the concept of number is accomplished primarily by the elimination of illegitimate operations. If mathematical operations can *always* be performed, the outcome of an operation will *always* be a number. As a result of this act of generalization place must be found for 1, 0 and the negative numbers, which arise through subtraction; for the fractions, which arise through division; and for the irrationals and the imaginaries, which arise through extraction of roots. This is accomplished, as will be seen immediately, through the introduction of the notion of *group*.

But a further transformation is sometimes introduced into the empirical concept of number through an operation of ordering. In fact the negatives, fractions, and irrationals may themselves be considered as serial extensions rather than as the results of operational generalization. As was pointed out, alternative routes of derivation are often possible. But what is important is that some notion of order is frequently considered essential to the definition of number, i.e., numbers are considered to be not merely a collection of entities but an ordered collection. This gives rise to the notion of ordinal numbers, and justifies the act of counting. It gives rise also to elaborate techniques by which order may be set up among fractions, for example, or among irrationals. The general techniques for the establishment of series have already been discussed; the problem in each case is the discovery of a relation which will satisfy the general postulates defining order. For example, it is possible to order the natural numbers on the basis of the relation "less than"; all of the general postulates defining order would be satisfied, and all of the special postulates defining the discrete series; hence the result would be the ordinary number system, 1, 2, 3, 4, 5, . . . , called earlier in the chapter the *progression*. It is by means of this that counting is carried on.

NUMBER: SCIENTIFIC CONTENT

The generality involved in the scientific concept of number suggests that it has become far removed from its empirical foundation. With the introduction of 1, 0, the negatives, the fractions, the irrationals, and the imaginaries, it seems that mathematics is no longer concerned with events. "In accepting these symbols as its numbers, arithmetic ceases to be occupied exclusively or even principally with the properties of numbers in the strict sense. It becomes an *algebra* whose immediate concern is with certain operations . . . defined formally only, without reference to the meaning of the symbols operated on." [1] Instead of defining number as the property of groups, mathematics considers it

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[1] H. B. Fine, *The Number-System of Algebra* (Boston: Heath, 1907), p. 26.

essentially as *the field of mathematical operations*, i.e., numbers are any entities which may be operated upon in specifiable ways to produce other entities of a similar kind. But if the operations are such as cannot be physically performed upon empirical groups, there is no reason to suppose that the resultant numbers will be empirically descriptive. Hence mathematics becomes by this act of generalization more or less removed from the empirically given. It is due to the important part which acts of this kind play in mathematics that it is commonly characterized as a strictly formal, or non-existential, science. With the breaking away from empirical groups the way is open for the development of meaning along intensional rather than extensional lines; hence the search is for formal postulate systems.

Clearly, the essence of the formal definition of number lies in the notion of operation. As a result there arise various levels of generality. One may define merely the positive integers; or one may define the entire class of whole numbers, positive and negative; or one may define the rational numbers; or one may generalize so as to include the irrational numbers as well, and thus define the real numbers; or one may reach a still higher stage and formulate postulates descriptive of numbers as including also the imaginary numbers. Each of these represents a higher stage of generalization, and each includes a wider range of different types of number. For the purposes of illustration the set chosen will be that which defines number in the highly general sense which includes the real and the imaginary numbers.

In this connection the notion of *group* is important. A group, in this technical sense, is a class of a certain kind, viz., a class whose members satisfy certain laws with reference to operations which may be performed upon them. Operations serve to correlate members of the group with one another. Hence if one specifies a certain class of elements whose natures are undefined except for the operations which may be performed on them and the laws according to which such operations must be performed, one has a group. One might leave the operations themselves in an undefined form, and thus define a highly abstract group. Or one might interpret them in a manner which suggests certain empirical operations performable on certain empirical objects, and thus define a less abstract group. The postulate system which is here offered as a definition of the field of numbers employs the notions of addition and multiplication as the basic operations. Then the number system may be defined by the following postulates:

1. *If* a *and* b *are numbers, then* $a \times b$ *and* $a + b$ *are numbers.*
2. (*Associative law.*) $a \times (b \times c) = (a \times b) \times c$, *and* $a + (b + c) = (a + b) + c$.
3. (*Commutative law.*) $a \times b = b \times a$, *and* $a + b = b + a$.
4. (*Definition of identity element.*) *There exists a number* 1 *such that* $a \times 1 = 1 \times a = a$, *and a number* 0 *such that* $a + 0 = 0 + a = a$, *for every number* a *of the class of numbers.*
5. (*Definition of inverse element.*) *There exists, corresponding to any number* a, *another* $-a$ *such that* $a + -a = 0$, *and another* $1/a$ *such that* $a \times 1/a = 1$, *except that no inverse of* 0 *is required.*
6. (*Distributive law.*) *If* a, b, c *are numbers,* $a \times (b + c) = a \times b + a \times c$, *and* $(b + c) \times a = b \times a + c \times a$.[1]

Nothing is said in this postulate scheme concerning the scope of the class of numbers; the only two elements whose existence is assured are 1 and 0. But if one supposes, say, that successive additions of 1 determine *distinct* elements, the class becomes infinite. The operations of subtraction and division may be defined, respectively, in terms of addition and multiplication. Through these operations the class becomes indefinitely extended to include the negatives, the fractions, the irrationals, and the imaginaries. The principles by which the class of numbers may be ordered are not given in this set, though they may be added as further postulates if they are required.

The important feature to be noted in connection with this abstract definition of number is the discrepancy be-

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[1] This set is a modification of that given by J. W. Young, *Fundamental Concepts of Algebra and Geometry*, p. 94. See also R. B. Lindsay and H. Margenau, *Foundations of Physics* (New York: Wiley, 1926), pp. 9–10.

tween the scientific content of the notion and the empirical content. There are two main differences—one pertaining to the order of the elements, and the other applying to the nature of the operations. The former is indicated by the fact that that which is psychologically primitive at the empirical level is the collection of numbered groups, 2, 5, 6, 4, 3, from which 1 and 0 are considered to be psychologically derivative; but that which is logically primitive at the scientific level is the class of elements, of which 1 and 0 are given, and from which 2, 5, 6, 4, 3, and the rest of the numbers follow by logical derivation. Hence the number 5, say, is psychologically primitive but logically derivative, while the number 0 is psychologically derivative but logically primitive. The distinctions in the meaning of the term "operation" are illustrated in the fact that at the empirical level an operation is a method for manipulating groups and collections, while at the scientific level it is a rule for manipulating symbols. In the present case this is not quite true, since the operations have not been taken at the highest possible level of abstraction; they retain a tinge of their empirical reference which permits one still to speak of the things which are operated upon as *numbers*. But the tendency to the sharp separation between the extensional and the intensional is clearly indicated. At a high level of generality emphasis shifts from extension to intension. Abstractions are elusive features of the given, and cannot be readily pointed to; hence symbols which describe them cannot be defined by the denotative method but require the connotative method instead. This leads to a formalization of the concepts in question, and to a gradual elimination of empirical content. It results in the conception that mathematics is not an existential science but is concerned solely with ideal objects. A recognition of the empirical content of the notion, and of the operational routes involved in the passage to the scientific content dulls the sharpness of this dualism, and results in the conception that mathematics is the same kind of study as empirical science, except that operations play a somewhat larger part in the determination of the meanings of its basic concepts.

QUANTITY: EMPIRICAL FOUNDATION

The greatest difficulty in connection with the concept of *quantity* is a terminological one. Authorities disagree essentially in the way in which "magnitude" and "quantity" are to be used, and in the way in which these two notions are to be connected with "number" and "measurement."[1] In the face of this confusion almost any use of terms is justifiable. In what follows, the term "magnitude" will be avoided, and the term "quantity" will be considered to be empirically definable as a certain feature of groups, and scientifically definable as the number which is attached through the technique of measurement to such empirical groups.

The fact that one speaks commonly of quantity of time, quantity of space, quantity of light, quantity of pleasure, and the like, suggests that there are events which exhibit certain properties which may be defined as quantitative. The problem is to locate such events and then endeavor to select that property in terms of which they are identified.

The most obvious feature of quantities is that they are always quantities of something—length, duration, motion, weight, sensitivity, pain, wealth, etc. This is simply to say that quantities are always qualities, i.e., events which exhibit quantitative differences or resemblances also exhibit qualitative similarities. Thus if three lines differ quantitatively they resemble one another qualitatively in the fact of one-dimensional extension, and if three pleasures differ quantitatively they resemble one another in being pleasures of the same kind—say, of taste, or of musical appreciation.

The second feature of quantity is its relativity; an event is called a quantity only by virtue of the existence of at least two other events which exhibit varying degrees of the

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[1] See Bertrand Russell, *Principles of Mathematics*, Part III; W. E. Johnson, *Logic* (Cambridge: Cambridge University, Vol. II, 1922), Chap. VII; A. Spaier, *La pensée et la quantité* (Paris: Alcan, 1927).

same quantity. This aspect of quantity is not apparent but may be made clear after a moment's consideration. Quantity is usually supposed to be relative in the sense that it always involves a relation of greater or less, i.e., an event is called a quantity only if it is greater or less in some respect than some other quantity. For example, a line is called long because of the existence of another line which is short, and a stone is called heavy because of the existence of another stone which is light, and so on. From this point of view no event could be called a quantity unless there existed at least *one* other event to which it bore a certain relation. It seems, however, that quantity is relative in a somewhat more complicated sense than this. Quantity, in fact, never arises unless there is something akin to the notion of an intermediate or "between" element, and this necessarily involves at least *three* events. Suppose, for example, that in a certain community every day were either hot, say 90°, or cold, say 20°, and there were no days of intervening temperature; it is not likely that in such a community the notion of heat as a quantity would arise. Heat would be considered as an absolute quality possessed by certain days and lacking from certain other days; hot days would be related to cold days as positive to negative, or the reverse. Similarly, if all light was either very bright or very dim, there would probably arise no conception of light intensity; and if all pleasures were of very great or very minute intensity, there would probably arise no conception of quantity of pleasure. Quantity arises only when there occur three events similar in kind but so connected that the relation of the first to the second is of essentially the same kind as the relation of the second to the third, or, what amounts to the same thing, when one event is between the others and has a relation to one of them which is the converse of its relation to the other. For example, so long as propositions are considered to be either true or false, the notion of degrees of truth cannot arise; but once the notion of probability is introduced it is seen that the relation of a somewhat probable proposition to a false one is of the same kind as the relation of a true proposition to one which is somewhat probable, i.e., a probable proposition lies in between a false one and a true one.

In view of the discussion of order these results may be summarized by stating that it is essential to the notion of quantities that they be orderable. Since this is still on the empirical level one may define order in terms of the "between" relation. One may then say that quantity is a property exhibited by any three-termed complex in which there is a term such that (a) it has an asymmetrical relation to each of the other two terms, and (b) its relation to one of the terms is the converse of its relation to the other. The relation "greater than," with its converse, "less than," would satisfy this criterion, and enable one to order any three such quantities.

But if this were all that could be said about quantity, there would be no means for distinguishing between quantity and order. For example, a father, son, and grandson could be ordered according to the relation "begets," yet they would not be called quantities; events could be arranged in a causal chain by the relation "causes," but the ordered events would not be considered quantities. What is the unique feature which distinguishes a quantitative series from a non-quantitative one?

This differentiating feature is to be found in the ordering relation. In a quantitative series the essential feature is that the between relation should be interpretable as "greater than" and "less than." This itself is an empirically given relationship which is probably irreducible to anything psychologically more primitive. Logically one sometimes defines a greater quantity as a lesser quantity to which something has been added. But this does not seem to constitute a psychological simplification, and it is, moreover, applicable only to extensive quantities and not to intensive quantities. Often a greater quantity cannot be obtained from a lesser quantity by any sort of addition, especially in those cases where the greater does not seem to extend beyond the lesser in any sense. For example, liquids may be arranged into a quantitative specific gravity series on the basis of the relation "floats in," but this does not involve the notion that a substance of high specific gravity extends beyond one of low specific gravity. One simply decides that the relation "floats in" determines a quantitative distinction. Essentially the same technique is employed in arranging substances according to hardness on the basis of the relation "scratches," and in the arrangement of masses according to weight on the basis of the relation "displaces on a balance." The relation "greater than" can be defined only as the abstract feature which each of these ordering relations exhibits.

It can be seen that at the empirical level the notion of quantity is not dependent in any way either upon the notion of number or upon the notion of measurement. Numbered groups are, of course, quantities, since they may be arranged in an order on the basis of the relation "greater than"; they are, in fact, one of the most basic quantitative notions since they afford the pattern for the understanding of the less clear quantitative notions. But it does not seem essential to a quantity that it be measurable, and it does not seem advisable to define the notion in this way. On the empirical level quantity may be roughly defined as the property exhibited by the elements of an event of at least threefold complexity when these elements are similar in some respect and capable of being ordered according to a relation which may be interpreted as "greater than."

QUANTITY: OPERATIONAL DERIVATION

Empirical quantity is incorporated into scientific conceptual schemes through the technique of measurement. Scientific quantity, as will be seen presently, is identified with measured value. Hence the operational transformation of empirical quantity is such as can be accomplished through the application of measurement. The detailed description of such techniques is far beyond the scope of this book. In addition to what has already been said [1] a few remarks may be made.

In the first place, measurement should be understood in its true light as an operation which is essentially abstractive. The attempt is made to consider only one property of an event, and to portray this only in its quantitative aspect. This is the foundation for the claim that quantitative methods in science lose the qualitative features of events and reduce them to abstract structures. It is well to note that although the measured values are numbers, and are manipulated according to the rules of mathematical operations, they are not purely such. A measured value is always a qualitative entity, for it is a value of something, e.g., a length, a mass, a duration, or a velocity. Thus quality is not completely lost in the measurement, for there must always be reference to the unit in terms of which the measurement is made. But it is true that the measuring techniques serve to preserve not the individual features of the quality but only its abstract quantitative aspects, such as its position in a series of similar quantities, and its reducibility or irreducibility to other quantities.

In the second place, brief recognition should be made of the essential measurement techniques. They may be roughly divided into three types. The first and most common sort is the direct measurement by scales and clocks of extensive quantities, such as lengths, areas, volumes, angular spreads, and durations. This may require the actual physical manipulation of certain devices such as tapelines and protractors. Or the operations may be incorporated into recording machines which require nothing more of the observer than the reading off of a number. The second kind of measurement is applicable to quantities which are not extensive but which have convenient extensive correlates; in cases of this kind the intensive variations of the quantity to be measured are associated through physical laws with

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[1] Chapter VI, pp. 112 *et seq.*

the extensive variations in certain other quantities, and the measured values of the latter become the measured values of the former. For example, heat intensities are measured by variations in the length of a column of mercury, weight intensities by the distortions in a coil spring, and electricity intensities by the spatial deflection of a coil in a magnetic field. Similarly, though much more crudely, the intelligence of an individual may be measured by counting the number of questions of a certain kind which he is able to answer correctly, and pleasures may be measured by the amount of money required to produce them. Finally, measurement of intensive quantities which have no extensive correlates involves an application of the principle of order; the quantities to be measured are arranged in a series by virtue of some ordering relation; one member of the series is then selected more or less arbitrarily as the standard element and given a number, such as 1 or 0; finally, the remaining members of the series are numbered, and values are assigned to them in terms of their distance in the series from the standard element. This is essentially the way in which hardness and specific gravity are measured, and it is commonly employed in the measurement of various commercial products such as the coarseness of sandpaper, the quality of coal, and the delicacy of canned fruits and meats.

QUANTITY: SCIENTIFIC CONTENT

Scientific quantity is measured value. Hence the field of scientific quantity is the field of numbers. But this statement requires clarification. Measuring and counting are both activities of attaching numbers to events, and they should not be confused. In the first place, number is properly an attribute only of pluralities, hence always applies to classes or aggregates; measured value, on the other hand, applies to a single event by virtue of its position in a series. Number, therefore, applies not to the simple events themselves but to the aggregate in which they occur; measured value, however, applies directly to the simple events themselves. In the second place, as was pointed out a moment ago, measured values are always numbers of something. Hence what is properly attached to events in measurement is not a number but a ratio, i.e., a fraction whose denominator is the standard used and whose numerator is the number of times that standard is contained in the quantity measured. A length is not 5 but 5 inches, a weight is not 10 but 10 pounds, and an angle is not 30 but 30 degrees.

With these reservations it becomes possible to say that the field of scientific quantity is the field of numbers. As a consequence, no specific set of postulates is required to define quantities. Quantities are simply numbers with which some qualitative notion is associated. It follows that the features of the abstract postulate set defining numbers have their correlative interpretations when applied to quantities. Operations upon quantities are subject to specific interpretations for different quantities. For example, the product of two lengths is an area, the quotient of two lengths is an angle, the quotient of space over time is a velocity, and the product of mass and acceleration is a force. In this way new quantities may be introduced by definition in terms of elemental quantities. The identity elements, 1 and 0, are given correlative interpretations, the former signifying usually the unit value of the quantity, and the latter indicating the complete absence of intensive or extensive manifestation. The negatives, fractions, and irrationals are sometimes subject to quantitative interpretation and sometimes not. As quantities, however, they are subject to more extensive empirical reference than as mere numbers. It is, in fact, their applications as devices of measurement which account for their importance in the natural sciences. Some difficulty is found in trying to grasp the empirical reference of fractions, negatives, irrationals, and even 1 and 0, so long as these are considered as numbers. But as quantities, fractions have an interpretation which is almost universal; negatives have certain noteworthy interpretations, as in the case of negative velocities, accelerations, spaces and times; and even irrationals are subject to empirical reference in the ascertainment of the length of the diagonal of a square. The techniques of measurement serve to establish these empirical connections.

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CHAPTER XIV

SPACE, TIME

The concepts of *space* and *time* are among the most obscure to be found in the philosophy of science. There are many reasons for this. Not the least of these is the long and venerable history which the concepts have had in philosophical literature; this has tended, unfortunately, rather to confuse than to clarify their natures. Of importance, also, is the fact that the concepts have been intimately associated with developing science, and have influenced and been influenced by theories of the continuity and discontinuity of matter and by views as to the nature of growth, decay, and change. Of greatest importance, however, is the complexity of the concepts themselves. Though they are high abstractions, they are not empty of content but are highly complex, exhibiting features of dimensionality, order, structure, and extent, and intricate relations to events which they "contain." For these reasons the discussion in this chapter will be hardly more than an outline of the features to which reference should be made in an examination of the concepts.

In order to avoid later difficulties, mention must be made at the outset concerning the meaning which is to be given to the concepts at the empirical and the scientific levels. The obvious fact, which confuses the entire problem, is that there are several empirical and several scientific levels. To consider space only, for example, there is not one space which can be called empirical; on the contrary there are visual, auditory, tactile, motor, and possibly olfactory, and gustatory spaces as well, each of which claims a certain psychological primacy. The problem then arises as to whether these *are* psychologically primitive, and, if so, whether one of them is more so than the others. Similar difficulties arise at the scientific level. Prior to the discovery of non-Euclidean geometries, about a century ago, no one had seriously questioned the accuracy of the Euclidean system as a description of the space of nature; scientific space *was* Euclidean space. But since the advent of the theory of relativity suspicions have begun to arise that an alternative space might serve better as the background for the explanation of certain phenomena. Yet Euclidean space still remains adequate upon a certain level. Hence one finds himself in the peculiar position of being obliged to examine two scientific concepts, each having a certain range of application. Some authors recognize this ambiguity, and insist even that there may be three spaces, viz., psychological or individual space, public or physical space, and geometrical space.[1] Similar difficulties arise in connection with time.

Since the discussion in this chapter aims at mere illustration rather than comprehension, an arbitrary point of view may be taken. Empirically, space and time may be considered on that level at which the synthesis of the spaces and times of the separate sense organs has already been made; hence there will be for each individual only one space and only one time. Scientifically, the concepts may be considered at the pre-relativity level, at least for the major part of the discussion; a brief consideration of the difference between the absolute space and time of Newtonian physics and the space-time of the special theory of relativity will conclude the chapter, and will, perhaps, compensate for the over-emphasis on the Newtonian conception. As a convenience of terminology, pre-relativity space and time may be called simply Euclidean space and Newtonian time; together they constitute the background of the classical physics.

For obvious reasons, a variation in the order of discussion of the three basic questions will be introduced in this chapter. Contrary to what was found to be true in the case of the concepts of mathematics, scientific space and time are,

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[1] R. B. Lindsay and H. Margenau, *Foundations of Physics*, Chap. II.

in a sense, closer to the individual than are empirical space and time; at least the average individual, if called upon to describe space and time, does so in the Euclidean-Newtonian terminology rather than in more empirical terms. Apparently he has become so habituated to the scientific conception, which has infiltrated into the common sense point of view, that he finds it difficult to remove himself to a level which is psychologically more primitive. For this reason a discussion of scientific space and time advisedly precedes a discussion of the more empirical notion; only when the former is contrasted with the latter is it seen to be hypothetical and to be itself derivable from the empirical conception by certain operational techniques. Hence in what follows, the order of discussion will be: scientific content, empirical foundation, operational derivation. Because of the obvious similarity between space and time the two concepts will be considered together throughout the chapter.

NEWTONIAN TIME AND EUCLIDEAN SPACE

The space and time of classical physics may be described as *objective*, *single*, *continuous*, *infinite*, *homogeneous*, and *isotropic*. A few remarks concerning each of these features will serve to characterize them briefly.

Objectivity. By the objectivity of space and time is meant their independence of events. They are considered to be containers which may or may not be full. Events, presumably, cannot exist without space and time, but space and time can exist without events. Events make no difference to space or time. The independent character of time is summed up in such adages as, "Time and tide wait for no man," and in the words from *Macbeth*,

> Come what come may,
> Time and the hour run through the roughest day.

Newton described time and space in the following words: "Absolute, true and mathematical time, of itself, and from its own nature, flows equably without regard to anything external. . . . Absolute space, in its own nature, without regard to anything external, remains always similar and immovable." [1] One of Euclid's basic assumptions was the legitimacy of moving figures from place to place; this presumed an independent character for space, and made its nature in no way an effect of its occupant. Hence space and time are considered from this point of view to be the background or stage-setting for events. Events may come and go but time goes on forever; similarly events may be here and there but space extends everywhere.

Singleness. The singleness of space and time follows, perhaps as a corollary, from their objectivity. If space and time are independent of events, they are in no way tied up with the peculiar content of *my* experience as over against *yours*. Hence I have no right to conclude that my space and time are different from yours. Though I always experience spatial events as relative to my own body, and though I always experience temporal events as relative to the stream of my own individual consciousness, space and time as such are subject to no such relativity. There is, in fact, only one space, which is the space of all experiences, and only one time, which is the time of all experiences. We live, therefore, in a common world whose structure is that of space and time. The time which is here (say, on the earth) is the same as the time which is there (say, on the moon); the "right" time is everywhere the same. Similarly, the space which is now (say, 1937) is the same as the space which will be at some later time (say, 2037): the "true" space is always the same. Negatively, no individual is isolated in his own spatial or his own temporal system. Communication is possible precisely because of the intersection of private systems in a common space and common time.

Continuity. The notion of continuity as applied to space and time is somewhat ambiguous. There are at least three senses in which these aspects of nature may be said to be

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[1] *Newton's Principia*, tr. by Motte (New York: Adee, 1846), p. 77.

continuous. In the first place, they exhibit no natural divisions; time does not pass in pulsations or in rhythms, and space possesses no fences. Though space and time are both quantitative, i.e., they permit the comparison of increments according to the relation *greater than*, there is nothing in either space or time to suggest that they prefer one unit to another. Time does not proceed by jumps or ticks, but without rhythms; and space is unbroken by any natural lines of division. In this sense space and time may be said to be amorphous, since they offer no metrics; they exhibit no structure by which one part of space may be compared with another, or an earlier duration compared with a later one. Hence measurement in both space and time must take place by means of material objects and processes, more or less arbitrarily selected from a wide range of candidates. And their use implies measurement not of space and time as such, but merely of further material objects and processes located elsewhere in space and time. By the continuity of space and time in this sense is meant simply their unbrokenness; they are continuous in the same way that a London fog or a tropical sky is continuous. Any point represents a potentiality of division, but no such actual divisions are to be found.

In the second place, space and time may be said to be continuous in the technical sense of the linear continuum, defined in the preceding chapter. The order of instants in time and of points in space is that of the continuous rather than that of the discrete series. A discrete series is lumpy and atomic, with minimum elements between which strict adjacency holds; a continuum exhibits no such indivisibles, for between any two there is an infinity of others. A discrete series is empirically illustrated by a chain; but a continuum has no empirical illustration, for perceptual events exhibit only a finite divisibility. Hence one is obliged to resort to conceptual examples. The continuity of space and time are of the same kind as that exhibited by the series of real numbers. Accordingly, one may say that space is a three-dimensional continuum of points, and time is a one-dimensional continuum of instants, each of which series is capable of correlation with the series of real numbers. The applications of this correlation to measurement are obvious.

According to the third meaning of "continuity" space and time may be said to be continuous in the sense that they exhibit no gaps. There is no time when time does not pass, and there is no extent where there is no space. It is obvious that the notion of a gap in time would be inconceivable without the conception of another time stream by which the gap could be defined; for the notion of a gap in time implies a separation of two instants in time, and this would be possible only with reference to a background which would itself be a time stream. Another way of expressing this same feature is to say that at no point in the temporal series could one make a cut which would produce an element outside of time. Similar remarks apply with reference to the notion of a gap in space.

Infinity. By the infinity of space and time is meant the impossibility of designating any element as the end-element (either first, or last, or both). It is intimately tied up with the notion of order, and therefore requires reference to the dimensionality of space and time. Space is three-dimensional and time is one-dimensional. By this is meant simply that in order to locate an event in space three independent bits of information are required, while in order to locate an event in time only one is required. The notion of dimension is technically defined in terms of a cut.[1] Roughly, a series is n-dimensional when in order to cut it a series of $(n - 1)$ dimensions is required; for example, a plane must be cut by a line, and a line must be cut by a point. Space may then be considered as constituted by three intersecting linear series, while time is simply a single linear series. By the infinity of space and time is meant the absence of first and last points in each of the spatial series and the

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[1] H. Poincaré, *Foundations of Science*, pp. 240–241, 256–257.

absence of first and last points in the temporal series. Technically a series is infinite when, given any element, there is available a principle by which another element may be found. It is clear that the only argument for the infinity of space and time is of the rational type similar to that employed a moment ago when space and time were shown to be without gaps. It is obviously impossible to ask "When did time begin?" or "Where does space end?" since the very use of the words "when" and "where" implies a temporal and spatial background. Thus the question, "When did time begin?" means "At what instant in (another) time did time begin?" and the question, "Where does space end?" means "At what point in (another) space does space end?"

Homogeneity. To assert that space and time are, respectively, homogeneous is to say simply that they are everywhere alike. No point in space, and no instant in time bears any qualitative feature by virtue of which the general environment of space or of time from which it has been selected could be ascertained. Space and time do not exhibit any hummocks or irregularities, any compressions or expansions, any twistings or deformations. A mile selected from the line joining Chicago and New York would be indistinguishable from a mile selected from the line joining Paris and London; a ten-minute section drawn from the hours of the morning would be indistinguishable from a ten-minute section drawn from the afternoon. Neither the "here" nor the "now" is unique; no physical law describing the change in state of a system prescribes the time of day or the time of year when the law holds. The equations of the pendulum, for example, "*do not contain t explicitly.* The state of the system varies at any stage in a way dependent only on the instantaneous state itself, but not on the date of this state, not on the indication of the clock."[1] "Now" points and "here" points are determined by observers; but observers are of no importance so far as abstract time and space are

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[1] L. Silberstein, *Causality* (New York: Macmillan, 1933), p. 129.

concerned. As Newton said, "time flows equably," and it makes no difference whether the individual is busy or idle, amused or bored. Though the last mile may *seem* the longest, abstract distances are always the same.

Isotropy. Some difficulty arises in connection with the concept of isotropy, due to the ambiguity in the notion of "direction." By the isotropy of space is meant its sameness in all directions. Poincaré describes space as isotropic in that "all straights which pass through the same point are identical with one another." [1] This suggests that by isotropy is meant the similarity in all *dimensions.* If this is the correct interpretation no question arises with regard to time, for time is one-dimensional. But by "direction" is also meant the asymmetry of a relation; for example, Chicago is in a different direction from New York than New York is from Chicago, and the direction of time from 1936 to 1937 is different from the direction from 1937 to 1936. In this sense time may or may not be isotropic; but since there are two possible "directions" for time the question becomes at least relevant. One may call the former sense of isotropy its *dimensional* interpretation, and the latter its *directional* interpretation. Separate discussions of space and time are then necessary.

According to the dimensional interpretation Euclidean space is isotropic. By this is meant that the up-down, right-left, and here-there dimensions of space are identical. Since one's movements in the first of these dimensions are somewhat restricted, he tends to assign to that dimension a peculiar place in space; but this individual character must be seen to be merely relative to the observer, and hence not a feature of space as such. So far as space itself is concerned, rotation of objects makes no difference, i.e., an object is not compelled to suffer a deformation merely by virtue of rotation through ninety degrees. According to the directional interpretation Euclidean space is also isotropic. By this is meant that the up-down direction is the

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[1] *Foundations of Science,* p. 67.

same as the down-up direction, the right-left is the same as the left-right, and the here-there is the same as the there-here. Spatial lines have no arrows. Directional passage in space is not affected by the character of space. Isotropy in both of these senses is required for measurement. Though it is properly the material object, i.e., the scale, which is presumed to be unchanged by any movement in space, corresponding properties are attributed to space itself. "For example, it is assumed that the interval associated with any two points on a rigid rod (used as a scale) is independent of any motion of the rod; that is, no matter how the rod is moved about as a measuring instrument, the interval remains the same. This is a natural assumption; it is difficult to see how measurement could be carried out at all simply without it. Yet we must recognize its postulational nature. As soon as we have transferred it from its status as a postulate about operations with physical objects to a postulate about 'space,' it becomes equivalent to the assumption of isotropy and homogeneity (or free mobility, as we shall later call it) which are then considered characteristics of physical space."[1]

In the directional sense time may or may not be considered isotropic, depending upon the level of abstraction at which it is examined. From the empirical point of view, as will be seen later, time has an obvious arrow; events which are now future *become* present and then past; time "flows" from the future to the past and not from the past to the future. From the scientific point of view, however, the story may be different. At least if one asks what the symbol t means, as it occurs in physical equations, he may receive a different answer. If one limits himself to the equations of mechanics, "the equations are just as valid for negative values of t as they are for positive values. If the

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[1] Reprinted by permission from *Foundations of Physics* by R. B. Lindsay and H. Margenau, published by John Wiley and Sons, Inc. Professor Carl Eckart has pointed out to the author that this "assumption" of Lindsay and Margenau should be taken either as a definition of "rigidity" or as a definition of "interval." To make it an assumption involves a needless multiplication of undefined terms.

equations predict future events, they predict past ones as well. Time in mechanics is then completely reversible or two-way time."[1] Eddington expresses the same fact by saying that the laws of nature are indifferent as to the doing or the undoing of an event, hence "they must be indifferent as to a direction of time from past to future. That is their common feature, and it is seen at once when (as usual) the laws are formulated mathematically. There is no more distinction between past and future than between right and left. In algebraic symbolism, left is $-x$, right is $+x$; past is $-t$, future is $+t$."[2] The laws of mechanics enable us to compute the time-difference between one event and another, but they do not assign an absolute value to this difference and hence do not enable us to determine which event is earlier and which is later.[3]

The question hinges upon the meaning of *reversible processes.* "*All processes, the effects of which can be completely annulled, are called reversible.* Irreversible processes are such that, with the use of all possible physical means, a complete restoration of the initial state *everywhere* cannot be achieved."[4] The problem, then, is to determine the actual character of physical processes. Are all reversible, all irreversible, or some reversible and some not? In the first case the laws should contain a two-way time, in the second case a directional time, and in the third case distinct laws for the two types of process would be required. But when the problem is formulated in this way the solution is readily seen to be quite arbitrary. *Whether nature is considered to be reversible or irreversible depends upon the level of abstraction at which one looks at nature.* At the empirical level, as will be seen in the next section, time possesses an obvious directional feature, and any accurate description of physical processes must portray this fact. But if one neglects sufficient of the empirical features, e.g., friction and heat conduction, any physical process

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[1] R. B. Lindsay and H. Margenau, *Foundations of Physics*, p. 76.

[2] *Nature of the Physical World*, p. 66.

[3] *Ibid.*, p. 296.

[4] R. B. Lindsay and H. Margenau, *op. cit.*, p. 196.

can be described by laws which are invariant under the substitution of $+t$ for $-t$. To take a particular instance, the motions of the planets can be described by reversible laws if one chooses to ignore tidal friction, but such a law would be more abstract than one which includes reference to this important fact. Hence a classification of natural processes into reversible (movements of perfectly rigid bodies and of incompressible fluids, oscillations of a simple pendulum, movements of the planets) and irreversible (heat conduction, heat generation by friction, diffusion, explosions), such as that given by Lindsay and Margenau,[1] is based upon a difference of degree rather than one of kind. In the former the temporal aspects seem unimportant, and we feel justified in neglecting them, while in the latter the temporal aspects seem important, and we recognize the artificiality which results in our symbolic scheme if we fail to include them. The whole issue, in fact, is essentially one of convenience. "We can at any rate safely say that, in the actual use of the time concept in physics, the physicist will consult his convenience with respect to reversibility or irreversibility. The situation here is precisely that encountered in connection with other concepts. If the demands for clarity and simplicity cannot be met by the more intuitive notions, the physicist has no hesitation in modifying these notions and subliming them into more abstract concepts." [2]

EMPIRICAL SPACE AND TIME

Empirical space and time may be described as *relative*, *plural*, *discrete*, *finite*, *non-homogeneous*, and *anisotropic*. Corresponding remarks may be made with reference to each of these features.

Relativity. By the relativity of space and time is meant their inseparability from events. Space and time, from the empirical point of view, are simply features of *happenings in space and time*. At this level there is no empty space, and there is no unoccupied time.

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[1] *Ibid.*, p. 197.

[2] *Ibid.*, p. 76.

Consider space first. At the perceptual level space is a relational feature of events. Space reduces to position. But position is meaningless apart from relations of distance and direction, and these must always lie between *events.* An event has position because it can be reached from another event by a movement through a certain distance and in a certain direction. As an ultimate point of reference one always has his own body, and this tends to give to the "here," as will be seen later, a privileged position. Elimination of events from space, as in a perfectly dark room, results in a weakening of one's spatial sense; ascertainment of distance is conspicuously unreliable at sea; and the directional aspects of space easily become confused when one is riding an airplane through a fog. Space, thus, is for the individual a feature or aspect of events—essentially like color or weight, except that it is relational in character. Space must be occupied by events, otherwise it is not space.

Time exhibits in a more pronounced manner a dependence upon events. Time is, in essence, change, and change is always an aspect of events. For there to be change, *something* must change. Change, in fact, reduces to three aspects: It involves, first, "the coming in, rise or emergence into existence of something new, something which was not before. Every alteration or development is an illustration: a body moves into a new point; an organism passes into a stage which was hitherto not its own." But this requires a second aspect. "Plainly the new which comes to be does not rise into a void, but into a space already there, into a world already old. Here is the second moment of change—the persistence of a core of reality upon which the new is grafted. This abiding aspect of the changing thing is usually called 'the thing which' changes. . . . Third and last, change involves disappearance from existence, disintegration, loss. If we did not know this independently, we could deduce it from the moments of change already cited. For the new, by breaking in upon the old situation and giving to it a new element, necessarily destroys it."[1] Time also, therefore, is considered by the individual to be a feature or aspect of events—essentially like color or weight. Time must be occupied by events, otherwise it is not time.

Plurality. The plurality of space and time follows from their relativity. That event which is always given, and with which space and time become most intimately associated, is the observer himself. His body is always present to determine location in space, and his consciousness, if not nature itself, gives content to the temporal process. But just for this very reason space and time come to be built around him as a center. Space is for him the totality of relations of distance and direction connecting the events of *his* experience; time is for him the totality of relations of before and after, connecting the events in *his* experience. But no two individuals live the same lives, nor experience the same range of events. Hence, at the empirical level one may say that there are as many space systems and as many time systems as there are individuals. What I see at this moment is not identical with what you see, even though we are located at approximately the same point in space; if we are widely separated our experiences may be quite diverse. But my memories and expectations also differ from yours, even though we are contemporaries; if we are widely separated in time our total experiences may be quite diverse. The only means of linking our spatial and temporal systems together is through the medium of a *common* event, i.e., an event which is approximately "here-now" for both of us. Clearly, only by means of the discovery of such an event can the common space and the common time of science be derived. Hence arise the difficulties of the relativity theory, to which reference will be made later.

Discreteness. The characterization of empirical space and time as discontinuous is subject to the same ambiguity which appeared in the characterization of scientific space

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[1] D. H. Parker, *Self and Nature* (Cambridge: Harvard University, 1917), pp. 96–97.

and time as continuous. According to the first meaning of "continuity," viz., "unbrokenness," it seems safe to say that empirical space and time are continuous rather than discontinuous. In other words, in this aspect empirical space and time do not differ from scientific space and time. But whereas the unbrokenness at the scientific level was due to the absence of demarcations and pulsations, at the empirical level it is due to the multiplicity of divisions arising from the intimate association of space and time with events. Any event has such a variety of spatial relations to other events that no single one stands out as the characteristic metric; one does not find that space favors, say, the meter unit as over against the yard unit, since events are just as likely to be separated by the one distance as by the other. Similarly, time exhibits no subdivisions at all unless one associates it with clocks or other processes, and in such cases the processes selected are soon seen to be relative. For example, the rising and setting of the sun might seem to contribute a definite metric to time itself, indicating, so to speak, definite lumps in the temporal process. But these events are soon seen to be varying as measured according to certain other processes, such as the number of swings of a pendulum. Hence time seems to exhibit only such a metric as is obtained by a completely arbitrary selection of processes. This seems to indicate that empirical space and time are, like scientific space and time, simply unbroken potentialities of division.

But according to the second meaning of "continuity," viz., "linear continuity," the situation may be different. There seem to be definite limits in our ability to distinguish spatial extents and temporal durations in the direction of the very small. Perceptually one finds neither indefinitely smaller distances nor indefinitely shorter durations; points and instants, in the scientific sense, are not empirically given. Space and time are lumpy, consisting of atoms which represent the minimum discernibles. Nor does one find on the empirical level that space and time exhibit an indefinite number of elements between any two; in perceptual space and time the atoms are adjacent to one another. All of this may be summarized by saying that empirical space and time are describable rather as discrete series, in the technical sense defined in the preceding chapter, than as linear continua.

According to the third sense of "continuity," viz., "absence of gaps," empirical space and time may be said to be discontinuous, provided, of course, one recognizes the legitimacy of using scientific space and time as referential systems. From the strictly empirical point of view neither space nor time exhibits gaps, for an empty space would not be space and an empty time would not be time. When one spends the night in a deep dreamless sleep, the moment of going to sleep is strictly contiguous with the moment of waking in the morning; the time of the night's passage does not exist for the individual. In the same way, when one spends the night in a Pullman the view from the window on retiring is strictly contiguous with the view on arising; the space intervening does not exist for the individual. But through communication with others who remained awake all night the individual learns of the occurrence of intervening temporal and spatial events. He is then obliged to separate the contiguous events of his experience, and to insert empty time and empty space between them. This gives rise to gaps in his space and time. None of this occurs, of course, at the strictly empirical level, for at this stage space and time for the individual are simply *his* events, and an empty space and an empty time would be meaningless. It is only at the higher stage in which there is a growing recognition of a common space and common time that the notion of gaps has any significance.

Finitude. If space and time are intimately associated with events, they will exhibit definite limits. The last point in space is, for the individual, the most remote point which he has visited; the first moment in time is his earliest remembered experience. Strictly, of course, the notion of first and last elements implies a more extensive series taken as the referential system; hence there must be the vague recognition of scientific space and time in order that the finitude of empirical space and time may be rendered clear. But the individual easily recognizes that the events of this more extensive spatial-temporal system are not in *his* experience, hence do not constitute *his* space and *his* time. The question as to whether empirical time is infinite in both directions depends upon the attitude which one takes toward the existence of the future in his own time scheme. If the future is defined in terms of expectations just as the past is defined in terms of memories, then the most remote future instant will be determined by that event of which there are definite expectations, just as the most remote past instant is determined by that event of which there are definite memories. But if the future is considered to be non-existent, the empirical temporal series may be considered as ending at each present moment. Whether this would be considered a finite series would depend upon considerations too profound to be examined here.

Non-homogeneity. By virtue of the close association of empirical space and time with events, they take on the irregularities of the events. It seems unquestionable that the "here" in space and the "now" in time have privileged positions in the series. Since the "here" is the location of my body it becomes the ultimate standard of spatial reference, and qualitatively distinguishable from all other points. "Here" has immediacy, "there" has remoteness. Similarly, since the "now" is the moment of my consciousness, it becomes the ultimate standard of temporal reference, and qualitatively distinguishable from all other instants. In fact, some would say that only the "now" exists, and that all "thens" are non-existent; if so, the "now" occupies a distinctly privileged position in the series. By virtue of these features one should always be able to tell from what environment a given section of empirical space or time has been taken; it bears its qualitatively distinguishing features in itself. This is unquestionably due to the intimate association of both space and time with events, and applies also to extents of space and to durations of time. A mile which is "full," i.e., contains a great variety, seems shorter than a mile which is "empty"; and an hour which contains exciting events seems shorter than an hour which is filled with monotony. From the empirical point of view the inch on the end of the nose is actually longer than the inch on the yard-stick; and the one second interval before some impending catastrophe is really longer than a one second interval drawn from ordinary life. Events vary in importance and significance, and space and time take on corresponding heterogeneities. Empirical space and time exhibit hummocks and irregularities, compressions and expansions, twistings and deformations.

Anisotropy. According to the dimensional interpretation empirical space is probably not isotropic. The conclusion here depends partly upon whether one concerns himself with visual, tactual, auditory, or motor space. Due to the fact of eye convergence, the near-far dimension is probably qualitatively distinct from the up-down dimension or the right-left dimension. The omnipresence of gravitation gives to the up-down dimension a qualitative distinctness. The essential freedom in one's right-left and near-far movements, as compared with the limitations in his movements up and down, contribute further to the anisotropy. Much of the difficulty in one's visualization of the other side of the earth is due to the anisotropy of the up-down dimension. According to the directional interpretation, empirical space is not so clearly anisotropic. It is doubtful whether there is any fundamental distinction between the near-far and the far-near directions, and between the left-right and the right-left. The up-down direction is, I believe, sharply distinguished from the down-up—due, again, to the fact of gravitation; one moves up with great effort, but one moves down with ease. Probably the directional features of space are intimately associated with movement; to the extent to which movement is reversible space may be considered isotropic.

If time is anisotropic, it can be so only in the directional sense, for time is one-dimensional. In this sense there seems little reason to doubt the fundamental anisotropy of time. Time has a definite direction, or arrow; it passes from the future into the past, and not from the past into the future. The directional character of time is intimately connected with one's conception of causal influences, hence a reversal of time would be essentially unthinkable. A life in which one first dies, then is married, and finally is born, could hardly have any kinship with the present mode of being. To be sure, this is a series of *events*, not time itself. Yet from the empirical point of view time is inseparable from change of events, and it is because changes of one kind always occur, and changes of another kind never occur that a reversibility of time becomes unthinkable. It seems hard to doubt that an awareness, more or less vague, of the going-on of time is present at all levels of consciousness. Eddington identifies this basic recognition of time's arrow with one's feeling of "becoming."[1] The notion of time as something which "flows," or "passes" is essential to it at the empirical level; but "flow" and "passage" are both directional in character. Hence time must be described as asymmetrical, or irreversible.

SPACE AND TIME: OPERATIONAL DERIVATION

The analysis of space and time has thus far revealed an important fact. There are at least two conceptions of space and two conceptions of time, which are sharply to be differentiated from one another. In general the properties possessed by the one are contradicted by those possessed by the other. Immediately the question arises as to which is more adequate as a description of the space and time of nature, i.e., which is *real* space and which is *real* time. Although this formulation of the problem is essentially

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[1] *Nature of the Physical World*, p. 69.

misleading, since there is probably a sense in which each of the alternatives is real, it indicates the direction in which one must look for the answer. Obviously empirical space and time are in some sense closer to the facts, and hence less open to conjecture, than scientific space and time. Scientific space and time, on the other hand, are in a similar sense hypothetical or constructional. The problem, then, is to show how scientific space and time, as derivative, are obtainable from empirical space and time, as primitive. If scientific space and time are constructional in character, they can be given content only in terms of their operational derivations. Hence one has not properly understood what scientific space and time mean until he has described the techniques by which they are obtainable from more obvious data.

From relativity to objectivity. Empirical space and time are transformed from relativity to objectivity by an act of *abstraction*, or of neglect. What is given is always a complex consisting of an event in space and time. Through an act of isolation the spatial and temporal aspects of the complex are singled out for attention, since they seem permanent while events vary in content. From this it is but a short step to the realization that the essence of the complex is its spatial and temporal features, and that these are not in any way affected by the events. All of this is accomplished through an act of generalization, by which space and time are presumably given a wider range of application; empty space and empty time, which at the empirical level are impossible, become possibilities at the scientific level. Hence arises the notion of large containers, of a spatial and a temporal kind, in which events may be located. This is the objective space and time of science.

From plurality to singleness. The many spaces and times are united into a single space and time by an operation of *correlation.* The problem is to eliminate the privacy of *my* space and *my* time by finding an event which is in *my* space and time but also may be in *yours* as well. Correlation is therefore accomplished by finding *common* events, i.e., events which are "now" and "here" both for myself and for someone else. Practically, this can be easily done. My friend and I meet in a certain room. I then tap on a table and we agree that the event which is his hearing of the tap is to be considered as contemporaneous with the event which is my hearing of the tap. Thus by means of coexistent events our two time schemes become linked together. Events which are past in my friend's time system become correlated with events which are past in mine, and a common time begins to emerge. By extension the time systems of others are introduced. Finally, a moment of absolute time becomes defined as that property which is possessed in common by all coexistent events; this property is called *presence*, *pastness*, or *futurity*, depending upon its location in some one time scheme. Then, to say that an event happens at a certain moment t is simply an abbreviated way of saying that the event coexists with the mutually coexistent events $e_1, e_2, e_3, \ldots e_n$. Similarly, I can correlate my friend's space system with mine if we can agree on an event, say the table, which we shall both call "here." By means of this, events which are remote in his scheme become remote also in my scheme, and a common space begins to emerge. As more and more time schemes are included I begin to see the arbitrary character of the "here" point which has been selected, and willingly replace it by the equator or one of the fixed stars. Then a point of absolute space becomes defined as that property which is possessed in common by all events lying at a certain distance and direction from some agreed-upon event; this property is called *nearness*, or *remoteness*, depending upon its location in some one space scheme. Accordingly, to say that an event happens at a certain point p is simply an abbreviated way of saying that the event has the relation R to a certain event E. In this way arise the single space and the single time systems which are so fruitful for science. The important point to recognize, since reference will be made to it later, is that the correlation of the private spaces and private times is possible only through the discovery of common events. The theoretical difficulties arising in this discovery constitute one of the foundations for the theory of relativity.

From discreteness to continuity. Only one of the meanings of "continuity" will be considered here, viz., "linear continuity." The problem is to determine the operational route by which a finitely divisible space and time are made infinitely divisible. This problem is sometimes formulated in terms of the empirical derivation of points and instants. Two closely similar views as to the nature of this derivation may be presented. One is that of Russell and is called the method of *logical construction;* the other is that of Whitehead and is called the method of *extensive abstraction.*

Russell's illustration is taken from time, but a similar procedure is applicable to space. "Let us take a group of events of which any two overlap, so that there is some time, however short, when they all exist. If there is any other event which is simultaneous with all of these, let us add it to the group; let us go on until we have constructed a group such that no event outside the group is simultaneous with all of them, but all the events inside the group are simultaneous with each other. Let us define this whole group as an instant of time." [1] A somewhat less empirical point of view would be inclined to define "point" not as the class of such events but rather as the property possessed in common by all of the events.[2] This method of defining "point" seems preferable to the traditional one which considers it as the *limit* of a series of gradually decreasing volumes. Since the limit cannot be certainly known to exist, an operational route which defines the construct in terms of an abstract feature which is known to exist retains a closer empirical reference.

Not altogether unlike this is Whitehead's method of

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[1] *Our Knowledge of the External World,* p. 118.

[2] This empirical tendency has been noted before in connection with Russell's definition of number as the *class* of all classes rather than as the *property* of classes. See above, p. 264 n.

extensive abstraction. Consider it with reference to time. "Durations can have the two-termed relational property of extending one over the other. Thus the duration which is all nature during a certain minute extends over the duration which is all nature during the 30th second of that minute." [1] "Every event extends over other events, and every event is extended over by other events. Thus in the special case of durations which are now the only events directly under consideration, every duration is part of other durations; and every duration has other durations which are parts of it. Accordingly there are no maximum durations and no minimum durations." [2] "Such a set . . . has the properties of the Chinese toy which is a nest of boxes, one within the other, with the difference that the toy has a smallest box, while the abstractive class has neither a smallest event nor does it converge to a limiting event which is not a member of the set." [3] The abstractive set serves to "guide thought to the consideration of the progressive simplicity of natural relations as we progressively diminish the temporal extension of the duration considered. Now the whole point of the procedure is that the quantitative expressions of these natural properties do converge to limits though the abstractive set does not converge to any limiting duration. The laws relating these quantitative limits are the laws of nature 'at an instant,' although in truth there is no nature at an instant and there is only the abstractive set. Thus an abstractive set is effectively the entity meant when we consider an instant of time without temporal extension. It subserves all the necessary purposes of giving a definite meaning to the concept of the properties of nature at an instant." [4] As in the case of Russell, the notion of "point" is defined not as the *limit* of the series but as the *series itself*, or an abstract feature of it.

From finitude to infinity. The limits of empirical space and time are extended through *correlation*. The essential feature of an infinite series is the existence of a principle

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[1] *Concept of Nature*, p. 58. [2] *Ibid.*, p. 59. [3] *Ibid.*, p. 80. [4] *Ibid.*, p. 61.

by means of which, given an element of a series, an adjacent element may always be found. At the empirical level space and time are finite, for there is no such principle which is universally applicable; my space extends to the most remote point of my experience, and I have no principle for going beyond this; my time begins with my first remembered event and I have no principle for determining anterior events. But if I admit the legitimacy of correlating the events of my experience with those of another individual, I find myself provided with just the principle required. For example, my first remembered event may be a spanking administered by my mother for some misdeed, though I may not remember the misdeed; but by talking with my mother I may learn what the cause of the spanking was, since this is a remembered event in her experience. By multiplying correlations I extend my time system indefinitely into the past. Similarly, I correlate my spatial system with that of another by locating an event which is the boundary of my system with an event *in* the space of another who has traveled more widely than I; again by multiplying correlations I extend my space system indefinitely in three directions. The activities involved in both of these processes are those of serial extension; a principle presumed to hold only *in* the series is discovered to hold *beyond* its supposed limits.

From non-homogeneity to homogeneity. The irregularities in empirical space and time are eliminated by the *abstractive act* which rids space and time of events. Only because space and time are tied up with happenings do they seem to exhibit heterogeneity. The "here" is different from the "there" only so long as I think of my body as a unique point of reference; when by correlation I discover that my "here" is another's "there" and my "there" is another's "here" I realize the arbitrary character of this distinction; the "here" and "there" lose their qualitative distinctness. Similarly, when I realize that a space which for me is "full" may be correlated with a space which for another is "empty," I recognize the essential irrelevance of spatial occupants; space then begins to exhibit a permanent structure which is independent of events. Analogously, through a recognition of the fact that messages take time I realize that what is "now" for me may be "then" for another, and what is "then" for me may be "now" for another. Furthermore, when I find that an hour which for me was very exciting and therefore very short can be correlated with an hour which for another was very dull and therefore very long, I recognize the essential irrelevance of temporal occupants; time then begins to exhibit a permanent structure which is independent of all processes. In some such way the hummocks, twistings, and compressions of space and time are eliminated, and a strictly homogeneous space and time result.

From anisotropy to isotropy. Both the dimensional and the directional features of isotropy are introduced into empirical space and time through *abstraction*. The dimensional anisotropy of empirical space is lost with the abandonment of events. The uniqueness of the up-down dimension, for example, lies, as was seen, in such things as gravitation, our upright positions, the limitations of our movements in this direction, etc.; when reference to one's body and to other events is abandoned the uniqueness disappears. The peculiarity of the near-far dimension can be eliminated by the simple device of placing individuals in such a way that what is for one a near-far dimension is for the other a right-left dimension. The directional anisotropy can be eliminated in a similar manner. To show the arbitrary character of the distinction between right-left and left-right, all that is required is one individual facing another. By similar techniques all directional features of space can be shown to be dependent merely upon coördinate systems, and hence arbitrary. The directional anisotropy of time is eliminated when causal concerns are abandoned. Causation does not seem to be inherent in the time scheme, but merely a feature of events. The unthinkable character of a world in which death precedes birth is not a matter of time itself but of events; events in this world simply do not occur that way. An earlier instant does not cause a later instant; rather, an earlier event causes a later event. Hence with the elimination of events from time there is no longer any obstacle to the view that time is reversible. Time becomes a series, and a series has no unique directional features.

SPACE-TIME

In conclusion, brief reference may be made to the modifications which have been introduced into the scientific notion of space and time through the discoveries which eventuated in the theory of relativity. From the limited point of view of the treatment in this chapter the differences between the two views may be explained in terms of difficulties which arise in connection with the operational derivation from empirical space and time. It has already been suggested that Newtonian time and Euclidean space are constructs. By this is meant that they do not describe obvious features of nature but are derived from them by certain logical techniques. Immediately the question arises as to whether the techniques are legitimate. There are two ways of answering this question. One is to examine the techniques themselves, and ascertain the assumptions upon which they are based. This is the more difficult task, since the techniques are not certainly known, and most of the assumptions are implicit. The second way is to employ the ordinary techniques of verification; Newtonian time and Euclidean space evidently function not only as constructs but as hypotheses as well, hence their adequacy may be determined by drawing the implications from them and subjecting these to empirical corroboration. If the predicted consequences are not verified, presumably the techniques of derivation have not been correctly employed, and abandonment or revision of the hypotheses is necessary. This is the easier task, but it unfortunately does not tell us precisely in what respect the hypotheses are wrong, nor precisely where the error in operation has been committed.

Now it seems to be a fact in recent physics that the inadequacy of absolute space and absolute time has been disclosed through both methods. It has been shown that the existence of such entities is highly conjectural, on the one hand, because there is no technique by which they could be derived; by operations which are relative it is hardly possible to derive something which is absolute. But it has also been shown, on the other hand, that such entities must be conjectural because the deductions from them do not always coincide with facts; the outstanding illustration is, of course, the famous Michelson-Morley experiment. Hence through both routes attention has been called to the possible inadequacy of the classical space and time systems.

Reference will be made here only to the former of these routes, since the concern throughout the chapter has been with derivation rather than with verification. From this point of view it seems possible to say in general that a reëxamination of the operational techniques employed in the passage from empirical space and time to classical space and time discloses certain questionable assumptions. As a result, a modification of the classical point of view seems advisable. The outcome of this modification is the space-time of relativity, whose outstanding feature is a "closer" connection with empirical space and time. The justification of this assertion will be found in the following paragraphs, which will conclude the present chapter.

Relativity. It seems obvious that the space-time of relativity will exhibit relativity as its most characteristic feature. Yet a word of caution is necessary. To show that a thing is relative is to show that it possesses certain of its features by virtue of something else with which it is connected. For example, A may possess the property *p* not in itself but only in relation to B; in relation to C it may possess the property *q*. But this determines something absolute, viz., the law according to which the properties of A change from *p* to *q* as it is considered successively in its relations to B and C. There are two senses in which space-time is relative.

One, to be considered immediately, is the relativity of space-time to events. The other, to be considered under the discussion of the serial properties, is the relativity of space and time to one another. But in both of these cases there is something absolute. In the one case it is the law according to which measuring devices vary with changing systems of reference; in the other it is the abstract structure by which space and time unite into a four-dimensional whole. Hence there is a strict sense in which the theory of relativity involves the assumption of an absolute structure. But this absolute structure is different in important ways from the absolute structure of the classical physics, and reference will be made in what follows primarily to these differences.

The outstanding sense in which space-time is relative is in its dependence on events. But whereas at the empirical level the insistence is on events of any kind, at the level of the relativity theory the insistence is on *measuring devices.* Time and space are identified with measurement of time and space. This involves an inevitable reference to *clocks* and to *scales.* The result is that time and space both become relative to the techniques by which they are measured. Not only is time meaningless apart from the readings of clocks, but the setting up and reading of clocks is *all* that can be meant by time. Not only is length meaningless apart from the readings of scales, but the application and reading of scales is *all* that can be meant by length. This leads immediately to a consideration of the second aspect of space-time.

Plurality. If space and time are nothing apart from measurement techniques, they will vary in character according to the variation in the methods of applying and reading recording devices. The most important feature of measurement is that *someone* always measures; reference is therefore necessary in every case to an *observer.* However, this fact does not make space and time relative to the individual in the same sense that empirical space and time are; it does not make them private and relative to any *subjective* experience.

For the relativity theory, the important thing about the observer is that he is located in a certain frame of reference. Space and time are therefore plural in the sense that they are determined by systems of reference; "*length* and *duration* are not things inherent in the external world; they are relations of things in the external world to some specified observer." [1] "A length is . . . but the expression of a relationship between the observer and the observed, and the two partners of the relationship must be specified before the length can have any meaning. . . . Duration and time are mere relatives, mere expressions of relationships, and have no absolute significance *per se*." [2] Hence there are as many values assignable to any given length and to any given duration as there are possible systems of reference. In this sense space-time may be separated into space and time in an infinite number of ways. Yet in a more abstract sense space-time may still be said to be one, for there is a constant manner in which measuring devices are affected by changes in systems of reference. Though space and time are relative in their measured aspects, the law of the change of measuring devices is absolute. The old absolute of the Newtonian-Euclidean system of a space and a time has merely been transferred to the *relation* between space-time and measuring devices.

Continuity and infinity. Nothing in the operational derivation of classical space and time suggests that the assumptions of continuity and infinitude are illegitimate. Both of these have to do with the serial properties of space and time, and these are essentially irrelevant to considerations of relativity. Hence space-time of the relativity theory is continuous in the three senses discussed earlier in the chapter. It is amorphous, i.e., it exhibits no metric. It is describable in terms of the linear continuum, the most important feature of which is the absence of adjacency with

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[1] A. S. Eddington, *Space, Time, Gravitation* (Cambridge: Cambridge University, 1920), p. 34.

[2] A. d'Abro, *Evolution of Scientific Thought* (New York: Boni and Liveright, 1927), p. 151.

regard to its elements, i.e., space-time is capable of infinite subdivision. It is unbroken, i.e., there are no holes. Concerning the question of infinity, nothing in the operational derivation, so far as the special theory of relativity is involved, would lead one to call it into question. From the point of view of the general theory, however, certain considerations suggest that space-time should be considered as finite. The topic is too extensive to be discussed here.[1]

But reference should be made in this connection to the fact that the three-dimensional space and one-dimensional time of the classical physics have now become merged into a four-dimensional whole. The reason for this intimate union of what seem to be independent entities lies in the difficulties which arise in the attempt to correlate the various private space systems and private time systems. This is done, as was seen, through the medium of common spatial events and coexistent temporal events. Consider for illustration the process of determining the coexistence of two events which are located at points A and B, widely separated in space. They may be said to happen at the same time if they do so according to the readings of the respective clocks located at the events. But how can one know that the clocks have been properly set with reference to one another? One might carry the clock located at point A to point B and compare it with the clock at B. But the movement would almost certainly change its reading. Instead, one might send a light signal from A to B, and adjust the clocks accordingly. But this supposes that one knows what the velocity of light is. This difficulty might be avoided by a somewhat more complicated operation of sending a signal from A to B where it is immediately reflected back to A. Then if one records the time of leaving A (t_1) and the time of returning to A (t_2), the fraction $(t_2 - t_1)/2$ would represent the time of the light's journey from A to B. If one started the clock at B when the signal reached this point, and then set it back by an amount $(t_2 - t_1)/2$ the clocks

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[1] See d'Abro, *op. cit.*, Chap. XXXIV.

would then be parallel. But suppose that the whole system A-B is moving toward A or toward B; in either of these cases the time required to go from A to B would presumably not be one-half of the total, since in one case B comes to meet the signal and in the other case B must be overtaken by it. Further difficulties arise if the system is considered to be moving perpendicularly to the line from A to B. A situation of this kind determined the set-up for the Michelson-Morley experiment, whose results are too well known to require examination here. The point of this discussion is simply to show that the determination of coexistence is a highly complicated operation involving the use of *messages.* But the introduction of messages involves the introduction of *motion*, which itself brings *space* into the problem. The result is that measurement of time is seen to be impossible unless reference is made to space. Analogous considerations show that the measurement of space is impossible unless reference is made to time. Hence time and space are interwoven in a manner which defies separation.

This constitutes the second sense in which space-time may be said to be relative. Space measurements are relative to time measurements, and vice versa. But it is well to note here also that something absolute is implied in the conception. The abstract structure by which space and time are united into a four-dimensional scheme is not relative but absolute. "According to the Galilean system of reference[1] we may adopt, we divide space-time into space and time in one way or in another. Hence the resolution of the drawings of phenomena into space and time components is effected in various ways. We thus obtain variations in the spatio-temporal appearances of phenomena. Notwithstanding these variations, dependent on our relative motion, the absolute space-time drawings themselves, with their intersections, remain unchanged, and a direct study of the absolute world would consist in a study of these absolute draw-

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[1] A system of reference is Galilean if it is in uniform rather than accelerated or rotational motion.

ings without reference to their varying spatio-temporal appearances." [1] Hence space-time is an absolute four-dimensional scheme. This is simply to say that the location of an event requires four independent values, of which three are spatial and one is temporal.[2]

Homogeneity. Whether space-time is considered to be homogeneous or non-homogeneous depends upon whether it is examined in its abstract form, without regard to systems of reference, or in its more concrete form, relative to systems of reference. As has already been seen, the character of space and time is determined by the systems of reference; space may be shortened or lengthened and time may be slowed down or speeded up by properly chosen systems of reference. In this sense space and time may be said to be non-homogeneous, for the "here" and the "now" of the systems of reference determine the character of the space and time which is measured. But, again, if one recognizes that there is a constancy in the way in which space and time "appear" from given systems of reference he is obliged to conclude that the irregularities disappear if by space and time are meant not their "appearances" but the law according to which their "appearances" change for varying systems of reference. If space and time are defined not in terms of given manifestations but in terms of the potentiality of all possible manifestations, the hummocks, condensations, and other heterogeneities disappear. Abstract space-time exhibits a permanent structure of which all deformations are due to selected perspectives. Space-time is therefore homogeneous.

Isotropy. The dimensional isotropy of space-time is not perfect. So far as the spatial dimensions are concerned complete isotropy exists, i.e., there is no distinction between the up-down, the near-far, and the right-left dimensions, but

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[1] d'Abro, *op. cit.*, pp. 197–198.

[2] It is not to be denied, of course, that the pre-relativity system of Euclidean space and Newtonian time was also a four-dimensional system. Graphs of space against time were used long before Einstein. But the structure of the relativity system is different from that of the pre-relativity scheme. Roughly, the former involves a greater "interfusion" of space and time than the latter.

the now-then dimension refuses obstinately to become identified with any of the spatial dimensions. In a strictly Euclidean space the dimensions would be interchangeable. Furthermore, in a four-dimensional Euclidean system the additional dimension would be interchangeable with the other three. For example, the hypotenuse of a right triangle is represented by the formula,

$$s^2 = (x_2 - x_1)^2 + (y_2 - y_1)^2.$$

Analogously, in a four-dimensional world the equation would be

$$s^2 = (x_2 - x_1)^2 + (y_2 - y_1)^2 + (z_2 - z_1)^2 + (t_2 - t_1)^2.$$

But it turns out that space-time is not strictly Euclidean, though only a slight modification is required. If the t is changed from positive to negative, the required alteration is introduced. Hence the formula for distance in the theory of relativity becomes

$$s^2 = (x_2 - x_1)^2 + (y_2 - y_1)^2 + (z_2 - z_1)^2 - (t_2 - t_1)^2.$$

This expresses the lack of homogeneity in the space-time of relativity; the before-after dimension is always distinguishable from the spatial dimension.

From the directional point of view, space-time is isotropic as to its spatial dimensions but not as to its temporal dimension. "The common impression that relativity turns past and future altogether topsy-turvy is quite false."[1] Relativity, in fact, makes a distinction between past and future which is quite as fixed as it was for the classical physics. The basic notion is again that of causal influences, which is associated with the idea of messages. Since the maximum speed of messages is that of light, which is finite, causal relations are possible only between events so located that a message may be sent from one to the other. If an event B occurs at such a time that it may receive a message from another event A, then B may be the effect of A, and B must succeed A. Two such events lie absolutely in the past and

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[1] A. S. Eddington, *Nature of the Physical World*, p. 48.

future with reference to one another. But if the events are so situated that a message from one to the other would be required to travel faster than light, causal influences could not pass from one to the other. Two such events could have no absolute time order; A might be contemporary with B, A might precede B, or B might precede A, according to the system of reference of the observer. Hence relativity insists upon an absolute directional feature for time, but specifies in what sense it applies to events and in what sense it does not.

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CHAPTER XV

MOTION, FORCE, MATTER

As one approaches the concepts at this level, measurement techniques become increasingly important. The view that qualitative events are not properly a part of the subject matter of science unless they can be replaced by quantities, i.e., by measured values, has already been presented.[1] Such a view would entitle one to examine the present concepts at their highest level of abstraction and to say that by them one means simply numbers obtained by reading measuring devices; e.g., by force is meant simply the number obtained by reading a spring balance. Since numbers are properly the subject matter of mathematics, this view involves the reduction of physics without remainder to mathematics. The danger in this position has already been pointed out. If qualitatively different events in science are reduced exhaustively to numbers, all possibility of differentiation disappears for there is no mathematical difference between the number 2 which represents a *force* and the number 2 which represents a *motion*. Numbers which are measured values are always measured values of *something*, and they function in the equations of science not as mere numbers but as s, t, v, etc., i.e., as qualitatively distinct entities. The view of science which reduces quality to quantity neglects the fact that measuring instruments must be devised in the first place, must be calibrated by reference to judgments of perception, and must be manipulated in any given situation.

Hence the attempt will be made in the present chapter to avoid that extreme abstraction which reduces motion, force, and matter to pure numbers. This is not to deny that they are quantitative. But it is to recognize that the process of attaching numbers to them involves specific instruments

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[1] Chapter XII.

and specific techniques, and hence that the meanings of the concepts cannot be isolated from these physical events and processes. Scientific motion, force, and matter are definable as *measured* empirical motion, force, and matter but in that one word lies a significant distinction which it will be the task of this chapter to elucidate.

MOTION: EMPIRICAL FOUNDATION

At the empirical level, motion may be described as *a quantitatively variable change in the spatial relations holding between two events.* The terms in this statement which require clarification are "relation," "change," and "quantity."

Relativity. Empirical motion is relative in two very obvious senses: It is relative to *something which moves*, and it is relative to a *point of reference.* Consider the former of these two senses. Motion is a property of objects or events; trains, baseballs, clouds, raindrops, and planets move. Motion is capable of analysis into (*a*) the motion itself, and (*b*) the mover. What is given empirically, therefore, is not motion as such but rather moving objects. All that seems to be involved in this notion of objects is something which retains a qualitative identity through the change. The notion readily develops into the conception of an underlying "substance," existing beneath the observable qualities of the object. There seems to be nothing, however, in the empirical data that demands this hypothetical development. All that is involved is the possibility of identifying the object which moves throughout its history by comparing the object which began the motion with the object which completed the motion and noting enough similarity to permit them to be designated as the *same* object. The conception of empirical motion is closely allied to the conception of empirical space and time. Just as there can be no empty space or time, for space and time would then melt away, so there can be no "empty" motion. Motion is not mere change of space through a given duration, but change of something in space; from the empirical point of view mere change of space is meaningless, for it represents a change in something which has its form determined only by its content. *What* moves is indifferent; but that *something* moves seems essential.

But, in the second place, motion is meaningless apart from a point of reference. Motion is something which is descriptive of a relation between events, and not of a single event. As has already been seen, empirical space reduces to position, and position reduces to relations of distance and direction. Motion, therefore, which is change in position, reduces to change in distance and direction—relations which hold only between events or objects. On the empirical level it is arbitrary whether one says that A moves and B remains at rest, or B moves and A remains at rest. The air of paradox which one finds in a concrete illustration of this, e.g., the case of a train moving past a station, is dissipated when one recalls two facts: (*a*) The scientific theory of absolute motion has become so completely a part of the background of the individual's experience that he tends to think of motion in these terms rather than in terms of perceptual data. The situation here is precisely what it was in connection with space and time. (*b*) Empirical motion is in some way intimately associated with the feeling of effort which is required to move objects; small objects are easier to move than large objects, hence the former are more likely to move than the latter. In the case of the train, the station is unconsciously associated with all other objects with which it has no relative motion and which have no motions relatively to one another; the inherent difficulty involved in moving so extensive a system suggests that it is at rest while only the train is in motion. When the reference system is reduced in extent and becomes, for example, another train on a parallel track, the strict relativity of the motion again emerges; it is a noteworthy fact that if an observer in the latter case cannot see the ground and cannot feel the motion, he is quite unable to decide whether his train or the one on the parallel track is really moving.

Change. It seems clear, moreover, that motion is a *change* of some kind. The definition of the concept of *change* is a task of some difficulty, and can hardly be undertaken here. Reference may be made to the preceding chapter where a brief analysis of the notion was made in connection with the empirical foundation of time. It was there suggested that change always involves something coming into being, something passing out of being, and a permanent background within which these events occur. An alternative description is that of Russell, who defines change as "the difference in respect of truth or falsehood, between a proposition concerning an entity and a time T and a proposition concerning the same entity and another time T′, provided that the two propositions differ only by the fact that T occurs in the one where T′ occurs in the other."[1] For example, if of the two propositions, "x is white at time T," and "x is white at time T′," one is true and the other is false, then x may be said to have changed. This is to define "change" in terms of "truth" and "falsity"—a procedure which may involve one in further difficulties when the definitions of these latter concepts become necessary. The point is, however, that motion is change. But it is a *kind* of change. An object may be said to move when its spatial relation to another object passes out of being, another spatial relation comes into being, and there remains an identical feature which is the objects themselves. Or, in Russell's terms, motion arises when the common feature of the two propositions is a spatial relation, e.g., if "x is five miles northeast of y at time T" is true (or false), and "x is five miles northeast of y at time T′" is false (or true), then x and y may be said to have moved relatively to one another.

Of these two formulations of the concept of change, the former has an advantage from the empirical point of view, for it permits the description of motion *as taking place*, while the latter, without the addition of further propositions about the continuity of T and T′, describes only the fact of

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[1] *Principles of Mathematics*, p. 469.

motion *as having taken place.* Now it seems that motion on the empirical level is properly described rather as something taking place than as an inference to something which must have taken place by virtue of the evidence afforded by certain data. "If a body is moving at all fast, we *see* its motion just as we see its color. A *slow* motion, like that of the hour-hand of a watch, is only known . . . by observing a change of position after a lapse of time; but, when we observe the motion of the second-hand, we do not merely see first one position and then another—we see something as directly sensible as color." [1] Motion, therefore, is an empirical datum, and, moreover, one which resists analysis into anything which is psychologically more primitive. There are motions which are not reducible to a series of positions, but are perceived as single facts. It is these which give the concept of motion its empirical content.

Quantity. To say that motion is empirically a quantity is to characterize it in terms of a concept whose analysis has already been made. Quantities are events which may be ordered on the basis of the relation "greater than." Motions are, therefore, also velocities, and may be compared to one another according to degree. Perceptual discernment enables one to make crude judgments as to relative velocities, e.g., that an express train travels faster than a horse-drawn vehicle. In doubtful cases measurement is introduced. This involves correlating the two motions either with the respective changes in space over a given time, or with the respective times required to accomplish a given change in space. Furthermore, motion, in addition to exhibiting variations in *intensity*, manifests also variations in *direction*. Movement of A and B toward one another is not the same as motion of A and B away from one another, nor the same as motion of A obliquely to a line joining A and B. Rotational and "squirming" motions also exhibit features peculiar to themselves. These can be described as complicated functions of changes in distance and direction.

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[1] Bertrand Russell, *Our Knowledge of the External World*, p. 137.

MOTION: SCIENTIFIC CONTENT

There is not one concept of motion which functions in science, but two. Corresponding to the Newtonian-Euclidean absolute space and time there is an absolute motion, and corresponding to the space-time of relativity there is a relative motion. For purposes of illustration emphasis will be placed on the former, with concluding remarks showing the changes that must be introduced when the facts of relativity are taken into consideration.

Dependence on particles. The abstractive operation involved in the passage from empirical motion to scientific motion is not so great as that involved in the passage from empirical space and time to absolute space and time. Absolute motion cannot be empty; it is defined as "the occupation, by one entity, of a continuous series of places at a continuous series of times." [1] Abstractly, any correlation of the spatial continuum with the temporal continuum is possible; but only certain of these correlations are motions. The correlation desired is accomplished through the notion of the *particle.* This is merely a refinement of the notion of event, or object, which is demanded in the empirical conception of motion. For science, as for common sense, there is always *something* which moves. For the purposes of defining motion a particle may be considered to be any entity which can be at a point in space at an instant in time. The more precise characterization of this notion will be given later in the chapter. For the present, however, one may say that a particle is any entity which through the notions of "being at a point," and "being at an instant" brings about the correlation between space and time by which motion is to be defined. Absolute motion, therefore, is dependent on the concept of the particle.

Dependence on measurement. Motion as it functions in science is more closely tied up with activities of measurement than is empirical motion. Empirical motion is a given

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[1] Bertrand Russell, *Principles of Mathematics,* p. 469.

fact, exhibiting quantitative variations, but not reducible to readings of speedometers, clocks, and meter sticks. For science, however, motion is inseparable from the measurement of motion, since it is defined as the quotient of space divided by time, and these are in turn expressible only as numbers. Hence, scientific motion takes on its meaning only through a description of the activity of constructing, setting up, and reading clocks, meter bars, and other instruments of a more complicated sort. It is still empirical motion with which the scientist is concerned, except that he is able to measure unobservable motions, and to measure observable motions more accurately. The fact that motion exhibits not only features of magnitude, or intensity, but direction as well, places it in the class of *vector quantities*, and permits it to be handled according to the special laws determining the addition, multiplication, and other operations of this kind of quantity. The ordinary representation of a vector quantity as a straight line with an arrow attached, exhibits clearly the magnitude and directional features of motion.

Absolute character. The precise sense in which Newtonian motion is absolute must be made clear. If, for the moment, one calls the Newtonian theory the Absolute Theory, and the empirical theory the Relational Theory, the differences between the two points of view may be made clear by a quotation from Broad. "Absolute motion is the passing of a body from one point of Absolute Space to another. Absolute rest is the remaining of a body at a point of Absolute Space. Relative motion has the same meaning on both theories; it is just a change in the relative positions of two bodies. The difference about it is that the Relationists say that all motion simply is a change in the spatial relations of one body to others, whilst the Absolutists say that there is absolute as well as relative motion and that the two must be distinguished from each other. On the Absolute Theory all relative motion implies absolute motion, and is the appearance of it to us, but a knowledge of relative motion does not suffice to determine unambiguously the absolute motions involved. Thus, suppose that A and B are two bodies, and that *u* is the rate at which the distance between them is increasing. Then *u* is a relative velocity. The Absolutist says that it must be due to absolute motions in A or in B or in both, and that all that we can say about them is that their difference is equal to *u*."[1]

The difficulties inherent in the conception of the Absolute Theory are apparent. What is that ultimate frame of reference according to which all motions are, in the final analysis, absolute? Newton's formulation is not particularly helpful. "If a place is moved, whatever is placed therein, moves along with it. . . . Wherefore entire and absolute motions can be no otherwise determined than by immovable places; and for that reason I did before refer those absolute motions to immovable places, but relative ones to movable places. Now no other places are immovable but those that, from infinity to infinity, do all retain the same given positions one to another; and upon this account must ever remain unmoved; and do thereby constitute immovable space."[2] Absolute space seems thereby to be entirely postulational. Attempts have been made in the history of science to locate this absolute system of reference in terms of a material entity. C. Neumann, for example, assumed the existence somewhere in physical space of a completely immovable body called the "body Alpha,"[3] to which all motions are to be referred. Alternative choices for this ultimate point of reference are certain of the "fixed" stars, and (until fairly recent times) the ether. The arbitrary character of all such systems of reference is obvious.

Correlational character. Motion is commonly defined as the time rate of change of position, or as the first derivative of space with respect to time. The correlational character of this definition is clear; motion is incapable of definition without at least the notions of "space" and "change," the

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[1] *Scientific Thought,* pp. 97–98.

[2] *Newton's Principia,* p. 80.

[3] *Ueber die Principien der Galilei-Newtonschen Theorie* (Leipzig: Teubner, 1870), p. 15.

latter of which is essentially involved in the notion of "time." Hence, motion may be defined as the correlation of two continuous linear series, as these notions were defined in Chapter XIII, one of which is space and the other of which is time. But not every correlation of space and time is motion. By virtue of the notion of particle, by which the correlation is achieved, certain types are excluded. Abstractly, four general types would be possible: taking the two series in the order space-time, the correlation of the points of the former with the instants of the latter may be many-one, many-many, one-one, or one-many. But by virtue of the definition of "particle" the first two are impossible; a particle cannot be at two different points at the same instant. A many-one correlation of space and time defines *instantaneous extension*, and a many-many correlation defines *enduring extension*. The one-one and the one-many correlations then define, respectively, *motion* and *rest*. A particle is said to be in motion at an instant if it is possible to find an interval of time including that instant such that at every instant within the interval the particle is at a different point in space. A particle is said to be at rest at an instant if it is possible to find an interval of time including that instant such that at every instant within the interval the particle is at the same point in space.[1]

Two features of this definition may be called to attention. The first is the fact that a correlation of this kind gives the desired *continuity* to motion. "In a continuous motion . . . at any given instant the moving body occupies a certain position, and at other instants it occupies other positions; the interval between any two instants and between any two positions is always finite, but the continuity of the motion is shown in the fact that, however near together we take the two positions and the two instants, there are an infinite number of positions still nearer together, which are occupied

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[1] Compare Russell's definitions of these notions in his *Principles of Mathematics* (pp. 472–473). Difficulties with regard to end-points, i.e., points of change from motion to rest, or from rest to motion, can be handled by introducing the proper technicalities into the definition.

at instants that are also still nearer together. The moving body never jumps from one position to another, but always passes by a gradual transition through an infinite number of intermediaries."[1] The second feature of the definition is that both motion and rest require the notion of "interval" for definition. There is no such thing as motion or rest *at an instant;* in order to ascertain what the particle is doing at an instant one must know something as to what it was doing before that instant or what it will be doing after that instant. Motion and rest require *comparison* of the positions of a particle at two instants; if the particle is at the same position at the two instants, it is at rest; but if it is at different positions at the two instants, it is in motion. If one has given only *one* instant, no such comparison is possible. The importance of the notion of interval in the definition of motion and rest is obvious in the ease with which it enables one to avoid Zeno's paradoxes.

MOTION: OPERATIONAL DERIVATION

The operational passage from empirical motion to scientific motion is essentially that involved in the transition from empirical space and time to the space and time of the classical physics. Empirical motion is an aspect of empirical space and time in much the same way that scientific motion is an aspect of scientific space and time. However, the degree of abstraction is not quite so great. Whereas scientific space and time are empty, scientific motion retains the notion of particle; hence although for science space and time need not be occupied, motion demands occupants, i.e., motion and rest would be meaningless in an empty world. To be sure, in the scientific concept of motion there is an element of abstraction, for reference to specific spatial frames has been lost. Nevertheless, there is a general frame. The fact that even this general frame of reference tends to take material form in the "body Alpha," one of the fixed stars, or the ether, suggests the abstractness of the conception and the

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[1] Bertrand Russell, *Our Knowledge of the External World*, pp. 135–136.

reluctance of the human mind to leave the empirical realm. Similarly, the generalization of the scientific concept of motion to include velocities beyond the limits of observation may be described as an operation of abstraction, or, if one prefers, of serial extension. An instantaneous velocity could be defined either as the limit of velocities of continually decreasing durations, or as the series itself of such velocities, as in the case of Whitehead's definition of "point."

The distinction between the concept of motion as it functions in the classical physics, and the notion as it plays a part in the recent relativity theory is based upon a recognition of the elusiveness of absolute velocity through space. Newton himself, apparently, realized the hypothetical character of absolute motion, since he pointed out that the laws of mechanics have the same form in all reference systems moving with respect to each other with constant velocity. This was equivalent to admitting that by no mechanical experiment could absolute motion of this kind be determined. "No mechanical experiment that can be performed, for example, on a train moving at constant speed can divulge to us that the train is really moving, for the results of all such experiments *relative* to the train are precisely the same as those which would have been obtained by the performance of similar experiments at the station or on the roadside." [1] It was supposed, however, that such absolute velocity, i.e., velocity relative to some absolutely fixed point such as the ether, could be detected by other experiments, e.g., optical ones. The Michelson-Morley experiment was designed to verify precisely such a supposition. But its negative results, and the negative results of similar experiments performed upon electro-magnetic phenomena in general, have suggested that perhaps absolute velocity cannot be detected by *any* means; and the suspicion immediately arises that absolute velocity may be *meaningless*. At least if absolute motion cannot be established by any instrumental techniques, and if it cannot be logically derived

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[1] R. B. Lindsay and H. Margenau, *Foundations of Physics*, p. 331.

through any formal techniques, there is no longer any reason to suppose that it functions in science in any significant way. It must therefore be replaced by a more fruitful notion, viz., that of relative motion. This becomes the foundation for relativity mechanics just as the Newtonian conception became the foundation for the classical mechanics. The complications which are introduced when generalization is made to accelerated and rotational motions involve extension to the general theory of relativity and are too great to be discussed here.

Hence, in summary of the concept of motion, one may say that at the empirical level it designates a quantitatively variable change in the spatial relations holding between two events. But at the scientific level, it designates a certain correlation between points of space and instants of time, accomplished through the medium of a particle, and expressible in terms of measured values; for the absolute theory this correlation is between an absolute space and an absolute time, and hence determines an absolute motion; but for a relative theory the correlation is recognized as being relevant always to coördinate systems, and hence as in some sense arbitrary. Since motion is defined in terms of space and time, the operational derivation of scientific motion from its empirical foundation is such as is involved in the passage from empirical space and time to scientific space and time.

FORCE: EMPIRICAL FOUNDATION

The confusion which is to be found in recent literature with reference to the concept of *force* makes a satisfactory discussion of it impossible. The obscurity bears not only on the empirical notion, but on the scientific concept, and, as a result, on the operational derivation as well. That there is empirically revealed some such thing as force seems hard to deny, in spite of Russell's statement that "*force* is a mathematical fiction, not a physical entity."[1] Yet, on the

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[1] *Principles of Mathematics*, p. 482.

other hand, the term "force" is applied to so wide a range of experiences—weight, inertia, energy, mass, momentum, and work—that one wonders whether any definition of the term which would include such a variety of connotations would be possible. On the side of science itself, the confusion is of another kind. There seems to be little doubt but that Newton's laws of motion constitute the essential postulate system for the definition of "force" as it is used in science. But the task of determining which among these postulates is to be considered as definitive of the nature of force, and which merely as making assertions "about force," is not at all easy. Furthermore, dispute arises as to whether force is itself a basic notion for a system of mechanics, or must be defined in terms of a more fundamental notion, e.g., mass, or momentum. Hence one cannot be sure what is meant by force, even for contemporary science. As a result, the problem of logical derivation from its empirical foundation becomes essentially insoluble; logical techniques cannot be made precise if there is no clear-cut knowledge of that with which they start and that with which they end. At this level the problem is very much complicated by the ideal character of the laws of mechanics; they describe not actual but theoretical situations, hence their derivations from empirical situations must always be by involved routes. In the face of these difficulties the more or less systematic analysis of the preceding pages must be abandoned, and a somewhat random series of comments must take its place.

Attempting to ascertain the empirical foundation of the concept of force, one is compelled to recognize that forces have their loci in situations provoking in us feelings of effort. "Unquestionably the sensational basis of the scientific concept of force is the feelings of strain that we experience when we drag a heavy body along, or throw a stone, or bend a bow." [1] It seems hard to deny that there is a wide range of situations which give rise in us to feelings of a similar kind—stretching an elastic band, stopping a swiftly thrown ball,

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[1] C. D. Broad, *Scientific Thought*, p. 162.

carrying a heavy valise, lifting ourselves by our arms from a rigid bar, pressing upon a table-top, and so on. These are facts of experience, and anyone with a feeling for reality could hardly deny that they must be reckoned with in any comprehensive scientific theory. Just *how* they exist is not important. Some writers, for example, insist that forces are resident merely in our muscles, hence do not have a part in the physical world but only in the physiological, or perhaps the psychological, world. Others, Broad for example, insist that we must distinguish between our feeling of strain and the strains which we feel. "Force is not supposed to be our *feelings* of strain; it is simply supposed that the strains which we feel are forces, or are indications of forces." [1] Again, still others may dispute as to whether force is a purely relative aspect of events, depending for its existence upon the presence of the observer, or an objective feature, wholly independent of the observer; for example, is it correct to say that the sun attracts the earth through a force of gravitation, or is such a conception meaningless unless one supposes that the sun is in some respect like a human observer, and hence is able to *feel* the force? These are some of the most representative of the alternative views. But whatever status is given to forces, a recognition of their unanalyzable character seems basic. One cannot *say* what forces are, for to do so would imply the existence of something which is psychologically more basic than they are. To call them "efforts," "resistances," "strains," "tensions," etc., is not to define them but to attempt through alternative designations to point to them. Here, as in the case of all empirical concepts, one's only method of clarification is through denotation.

Furthermore, it seems undeniable that forces exhibit themselves always as attached to events. There are no disembodied forces; forces always manifest themselves as properties of things which push, pull, weigh, resist, impel, attract, and press. To say that force always certifies the

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[1] *Ibid.*, pp. 162–163.

presence of matter does not help, for this places an extra burden on the concept of matter. In fact, as will be seen later, there is reason to believe that force is one of the essential features of matter and hence to be used in its definition; but this would then forbid one to define force by means of matter. Accordingly it may be said, for the present, merely that forces are always attached to events which exhibit a certain qualitative identity. Even the wind which fans one's cheeks is hot or cold, fragrant or foul-smelling; and one promptly materializes as "unseen hands" any very subtle forces which seem to cause him to act without effort. Disembodied forces, like disembodied spirits, are not found so long as one limits himself to the most clearly given data.

This leads promptly to a consideration of a further aspect of force—its intimate association with motion. In this regard the concept of force is intimately tied up with the concept of causation, to which reference will be made in a later chapter. At the empirical level, force is commonly defined as the cause of change in motion. Whether the cause-effect relation is reducible to mere succession, or whether there is something of "production" involved, are questions which must be postponed. However, the feeling of force seems to be an important part of all situations in which one is himself active in bringing about a change in nature. But that the force is the *cause* of change of motion rather than the *effect* is not too obvious. When I instigate movement, e.g., push an automobile, I feel the force before I notice the motion, hence I conclude that the force has caused the motion. But when I stop movement, e.g., endeavor to hold a coasting automobile, I notice the motion before I feel the force, hence I conclude that the motion caused the force. It seems arbitrary, therefore, to say that force causes motion rather than that motion causes force. What can be said with a fair degree of certainty upon the empirical level seems to be merely that in all situations in which there is change of motion, i.e., initiation or acceleration, and stopping or slowing, force is present. In other words, nature reveals not causal connection but mere concomitance; force and change of motion coexist in events of certain kinds, but no invariable temporal relation and no relation of causal efficacy seem to be indisputably present. Furthermore, concomitance does not mean identity. Though force and motion are presumed to be coexistent features of certain events, they are distinguishable. In this respect they are analogous to certain other sense qualities, e.g., spatial extent and color; all extensions are colored and all colors are extended, yet one has no difficulty distinguishing these qualities from one another. Hence at the empirical level force is neither the cause nor the effect of change of motion, nor is it to be identified with change of motion; the two are simply concomitants.

Finally, as in the case of motion, force is quantitative. This is simply to say that forces may differ from one another in a way which permits the observer to say that one is greater in intensity than another. The force felt in bending a bow is greater than that felt in stretching a small rubber band, and the force imparted by a moving baseball is greater than that imparted by a tennis ball moving with the same velocity. Forces also exhibit differences in direction. A force up differs from a force down, and a force to the left from one to the right.

FORCE: SCIENTIFIC CONTENT

As a first approximation to a scientific definition of force, one may say that force *means* whatever is asserted about force in any propositions of science in which the word or symbol occurs. Whatever may be asserted as a true proposition about force *says something* about force, and the totality of such assertions constitutes the meaning of the concept. Furthermore, since the important statements about force in science are mathematical, force may be defined as the totality of mathematical equations in which f occurs.

What is required in science, however, is not primarily a knowledge of *everything* that may be said about force but only a knowledge of the *basic properties* of force. The presumption is, in other words, that certain statements about force may be taken as fundamental, and others taken as derivative, with the latter capable of logical deduction from the former. What is demanded is a set of postulates defining force, which will imply a set of theorems giving additional information about force. Then the postulate set may be considered as giving the essential meaning of the concept.

In spite of very general disagreement among writers on the philosophical foundations of the concepts of mechanics, there is apparently a common recognition of the necessity for defining force approximately in terms of Newton's three laws of motion. Whether these three laws are consistent and independent, and whether there is a more basic system from which they may all be deduced, are questions concerning which there seems to be some dispute. The reduction of these laws to their empirical foundations is a problem which, as will be seen immediately, involves some difficulty. But an examination of these basic laws will afford a satisfactory method for determining approximately what is to be understood by force as it functions in science.

"Every body preserves in its state of rest, or of uniform motion in a right line, unless it is compelled to change that state by forces impressed thereon.

"The alteration of motion is ever proportional to the motive force impressed; and is made in the direction of the right line in which that force is impressed.

"To every action there is always opposed an equal reaction: or the mutual actions of two bodies upon each other are always equal, and directed to contrary parts." [1]

The first refinement to be introduced into this conception, so far as modern science is concerned, is the replacement of the crude notion of "body" by the more precise notion of "particle." This concept was defined earlier in the chapter. The essential property of particles, for the purposes of this

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[1] *Newton's Principia*, p. 83.

discussion, is motion; since particles exhibit motion and rest they also exhibit changes of motion, and hence any other property intimately connected with this phenomenon.

The second refinement concerns the elimination of the notion of compulsion which plays so prominent a part in the first law. As has already been shown, there is some doubt whether even at the empirical level the notion of causation or necessity is an essential part of every force-experience. The whole question is tied up with the idea that force involves muscular action, and that changes in nature are analogous to those found in acts of will. That this notion played a prominent part in the thinking of primitive man seems unquestionable, but that it should be eliminated so far as possible from scientific thinking seems equally certain. "The idea . . . of enforcement, of some necessity in the order of sequence, remains deeply rooted in men's minds, as a fossil from the spiritualistic explanation which sees in will the cause of motion. This idea is unfortunately preserved in association with the scientific description of motion, and in the materialist's notion of force as that which *necessitates* certain changes or sequences of motion, we have a ghost of the old spiritualism. . . . The necessity in a law of nature has not the logical *must* of a geometrical theorem, nor the categorical *must* of a human law-giver; it is merely our experience of a routine, whose stages have neither logical nor volitional order."[1] Furthermore, "though there is no sense in saying that anthropomorphism is wrong," since "we can never get away from it in at least a diluted form," nevertheless this conception of force "is of doubtful value for mechanics."[2] Recognizing, therefore, the necessity for eliminating the notion of compulsion from Newton's first law, one may restate it in such a way as to avoid this feature, and to indicate more clearly that the law states merely a correlation between force and change of motion. "Every particle in a state of change of motion (i.e., neither at rest,

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[1] K. Pearson, *Grammar of Science*, pp. 119–120.

[2] R. B. Lindsay and H. Margenau, *Foundations of Physics*, p. 87.

nor in uniform motion in a straight line) exhibits force." The law then states merely that force may be defined with reference to something with which it is uniformly associated. In other words, it is part of the *meaning* of force that it should be associated with change of motion.

But although this law tells one essentially what is meant by force in terms of one of its important correlates, it does not afford a means for *measuring* force. If the important scientific statements about force are expressed in mathematical equations, the notion must function in science not as a quality but as a quantity. The quantitative character of force is described in the second and third laws. These state that force is capable of intensive variation, and that it exhibits a directional feature. Hence force, as motion, is a vector quantity. Forces then become numbers of certain kinds and can be operated upon according to the laws of vectoral calculation.

Specifically, the second law is presumed to afford a technique for measuring force intensity in terms of change of motion. Change of motion is capable of measurement through meter rods and clocks, and does not require the observer to be in contact with the moving object. Hence precision can be achieved, and anthropomorphism avoided. Change of motion is a somewhat more complicated function of space and time than mere motion. Just as the latter is the quotient of space over time, the former is the quotient of this quotient over time, i.e., the time-rate of change of velocity. This is called acceleration. The third law affords no technique for measuring force. It does state, however, an important fact about forces, viz., that they always occur in pairs, not singly. Though these two forces are equal in magnitude, they are opposite in direction; hence they are different vectoral quantities.

However, although the second law states a *proportionality* between force and acceleration, it does not state an *equivalence.* It affords a means for comparing two forces through the respective accelerations with which they are associated, but the comparison may be presumed to hold only in case there is no other factor of variation. However, there is just such a factor. To refer a moment to the empirical level, where there is contact with an observer and where forces may therefore be felt, acceleration is not an accurate measure of force. Greater force is exerted by a moving baseball than by a moving tennis ball, though they have the same velocity; and greater force is required to set an automobile in motion than a bicycle. This further property of particles is called *mass*, which Newton endeavored to clarify by calling it "quantity of matter." Force, then, may be defined as equivalent to the product of the mass by the acceleration, $f = m\,a$. This is the basic equation of mechanics.

In summary, then, force as it functions in science may be described as a property of particles; its measured value is a vector quantity, and it is universally correlated with (*a*) a similar property, equal in intensity but opposite in direction, and (*b*) two other properties known as *mass* and *acceleration* by means of which it is measured.

FORCE: OPERATIONAL DERIVATION

The search for a definition of force which will meet the needs of science has been the attempt to find a satisfactory criterion for *locating* and *measuring* forces. However realistic one may be in his attitude toward forces, he is compelled to admit that their empirical definition in terms of pushes, pulls, pressures, resistances, and the like—all of which imply contact between an observer and an object—is both too vague and too narrow to function adequately in science. Such a definition is especially vague because it offers no technique for measuring accurately the intensity and direction of forces. But it is vague also because it confuses a large group of "forces" all of which, to be sure, exhibit common features, but which show also important differences and require therefore to be distinguished. Furthermore, the definition is too narrow because it implies, indirectly at least, that forces can be known to be present only when the objects to which they are attached are *touched* by observers, and, consequently, that the study of force is a matter for physiology rather than for physics. The scientific definition of force must, therefore, satisfy two criteria: It must enable one to measure force and to differentiate between the various kinds of "forces"; but it must enable one to *locate* and to *measure* forces in situations not involving the immediate contact of the observer.

The essential operational act, therefore, by which the scientific notion of force is derived from its empirical foundation is *measurement.* This, as has been seen, is a type of abstraction. But it differs from other types of abstraction in that it attempts to devise a physical correlate for itself. Measuring instruments are physical events, and they are related to the events which they measure by physical connections. Hence the "mental" operation of generalizing a qualitative event so as to retain only its quantitative features has a parallel in the "physical" operation of inventing, setting-up, and reading recording instruments. Both processes begin with qualitative events and end with numbers. But whereas the former has no recognized principles or rules for its guidance (it is usually described simply as the "neglect" of individual features with the "retention" of common features), the latter is capable of accurate formulation in terms of certain recognized techniques determined by the character of the event to be measured, the nature of the recording device and its calibration, and its application in a specific situation. There are rules for measuring, and the meaning of any physical concept must not be separated from its techniques of measurement. "In general, we mean by any concept nothing more than a set of operations; *the concept is synonymous with the corresponding set of operations.* If the concept is physical, as of length, the operations are actual physical operations, namely, those by which length is measured; or if the concept is mental, as of mathematical continuity, the operations are mental operations, namely those by which we determine whether a given aggregate of magnitudes is continuous."[1] But where a recognized physical technique is available it is always to be preferred to a mental technique, even in those rare cases where the latter is to be found. Hence the attempt to define the scientific concept of force is the attempt to find a technique for the measurement of force.

The scientific notion of force presumes to include in its definition precisely such a technique; force is measured by the product of the mass and the acceleration. The necessity for this conception resides in the fact that there seems to be no way of measuring force *directly*. One cannot detach forces from objects and move them about as one can clocks and meter rods. One cannot even add and subtract forces in quite the same way that one can spaces and times. Force is an intensive quantity, like motion. Hence, like motion, it is measured not directly but through correlation with events of another kind which, in turn, are susceptible of more direct measurement. Motion, it was noted, is measured in terms of space and time, both of which are extensive quantities and readily measurable. Acceleration is measurable in terms of motion and time. But what may be said of mass? Is this also directly or indirectly measurable? If it is directly measurable, what is the qualitative experience of which it is the measure? If it is not directly measurable, with what qualitative experience is it correlated?

The answers to these questions are crucial. For if the meaning of the scientific concept of force is identified with its measured value, no analysis of this meaning is possible without a recognition of the techniques of measurement. What, then, is the empirical foundation of the concept of mass?

There are two main attempts to solve this problem. The first involves the search for a clearly given qualitative datum to which the term refers. The presumption is that mass is one of the psychological simples of experience, and if one can only locate it and measure it he will have the information

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[1] P. W. Bridgman, *Logic of Modern Physics*, p. 5.

through which the concept of force may be clarified. Unfortunately, this attempt seems to fail. To describe mass, as Newton did, by the phrase "quantity of matter" does not help one to locate it unless the meaning of this phrase is made clear. To describe it in terms of inertia is inadequate, as Lenzen points out, for "the status of inertia as a property is very dubious; it seems evident that we are not acquainted with inertia in the sense in which we are acquainted with redness, extension, etc." [1] Furthermore, even if there is something empirically given which can be called "inertia," this datum is one of the general kind called "force," and unless the features which distinguish it from all other "forces" can be made clear, one has made no progress. What is worse, unless there is some technique for measuring inertia, one has not advanced the problem. To suggest, as Newton did, that mass is measured by the product of the density by the volume is of no help, for density is measurable only as the mass per unit volume. Similar difficulties arise in the attempt to define mass through gravitation. For example, a recent writer [2] asserts that "the mass of a body cannot be more definitely described than to say that it is the quality to which are due both its inertia and its gravitational action." Apart from the difficulty associated with the notion of inertia and gravitation being *due* to mass, there is a further obscurity associated with the notion of gravitational action. The presumption is that this term is to be employed as a designation for the fact of *weight;* but if so the circular character of the designation becomes apparent, for weight, like inertia, is a datum of the general kind called "force," and unless the features which distinguish it from all other "forces" can be made clear one has made no progress. Hence the attempt to locate a qualitative datum to which "mass" directly refers, seems to lead up a blind alley. All such data appear to be merely force-experiences of certain kinds. Hence one finds that in the attempt to measure force by

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[1] *Physical Theory*, p. 111.

[2] F. H. Saunders, *Survey of Physics* (New York: Holt, 1930), p. 61.

means of mass, he can do so only by means of certain forces. The humiliating outcome is that one can measure force only by measuring force.

The second attempt involves, if not a complete abandonment of the empirical reference, at least the recognition that mass may be a somewhat obscure and complex property of events. From this point of view mass is considered not to refer descriptively to any clearly given type of force-experience. It is defined, rather, as a *construct* from the data, and can be said to be given only in the sense that it is required to make certain experimental situations intelligible. It is a property which "is ascribed to a body in order to represent the results of experiments on the body. We are acquainted with the phenomenon in the experiment, and know the measure which we assign to the body, but we do not appear to be acquainted with the property ascribed to the body."[1] If the extreme conventionalism of Poincaré's position can, for the moment, be neglected, the view which is here being maintained can be formulated in his words: "*Masses are coefficients it is convenient to introduce into calculations.*"[2] The fact of convenience prevents this from being a purely nominal definition. It can be restated so as to eliminate the tinge of arbitrariness by saying that masses are coefficients determined by certain complicated experiments on bodies, and by means of which other important properties may be demonstrated. The best example of this attempt to define mass is that first given by Mach,[3] and reproduced with some modifications by Lindsay and Margenau.[4] In this approach mass is defined in terms of acceleration, but in terms of a complicated function of the latter. Given two bodies, *A* and *B*, each exhibiting an acceleration toward the other, one may set up the function represented by the quotient of these accelerations; experiment shows this to be a constant, and it may therefore be presumed to be some sort of property possessed by the bodies; this property

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[1] V. F. Lenzen, *Physical Theory*, p. 111.

[2] *Foundations of Science*, p. 102.

[3] *Science of Mechanics* (Chicago: Open Court, 1893), pp. 216–222.

[4] *Foundations of Physics*, pp. 91–94.

may be called *mass*. For example, the mass of B with respect to A is defined as the acceleration of A with respect to B divided by the acceleration of B with respect to A, viz., $a_{AB}/ - a_{BA} = M_{BA}$. The mass of A with respect to B is the reciprocal of this. If the accelerations are equal the masses are equal, and if they are unequal the masses are unequal. By arbitrarily defining one of the bodies, say A, as a unit body, the mass of B becomes a fixed number. By this method numbers can be found which may be attached to all bodies. These numbers constitute the measured values of masses, and function accordingly in the equations of mechanics. All that should be emphasized here is that mass does not, according to this approach, refer descriptively to any immediately given force-experience. A quotient of accelerations is hardly one of the psychological simples of perception. Alternative definitions of mass, which are essentially constructive like the one given, are the attempts of Lenzen [1] and Broad [2] to define mass through certain constant properties of bodies as they behave under impact.

In conclusion, then, the most significant feature of the operational derivation of the concept of force is the invention of a technique for measuring force. This requires the construction (or the somewhat obscure discernment in the given) of a property of bodies called "mass," which is capable of measurement and affords the required technique. Force is then measured by the product of mass and acceleration, both of which are independently measurable. Many of the complications of this technique have, of course, been omitted. For example, the proportionality of mass to weight permits one to substitute the direct measurement of the latter by means of balances for the indirect measurement of the former. But this method could not be employed as a *definition* of mass, for then the difference between mass and weight would be lost. Complications of this kind require more detailed analysis than has been possible in this somewhat superficial treatment.

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[1] *Physical Theory*, pp. 107 *et seq.*

[2] *Scientific Thought*, pp. 169 *et seq.*

The result of this rather disorganized discussion of the concept of force is approximately as follows: Force at the empirical level is a vague name for a great variety of data, unquestionably present, yet incapable of precise characterization; these data seem to be always associated with events, often associated with motion, and capable of quantitative differentiation. Force at the scientific level is what is obtained when the attempt is made to determine precisely where forces are, and how intense their manifestations are, i.e., their loci and their measured values. But the attempt to measure force reveals the fact that there is nothing among the psychological simples of experience by which this can be done. However, there is revealed a somewhat obscure feature of nature, viz., a certain quotient of accelerations, which seems to afford the required empirical basis; this is called "mass," and is defined as proportionate to force. Then force becomes measurable in terms of mass. This makes mass psychologically derivative, but logically basic. The operational derivation of the concept of force is therefore simply the route by which its measured value is determined.

MATTER: EMPIRICAL FOUNDATION

It is likely that the concept of matter is both the most obscure of the basic physical concepts and the most ambiguous of philosophical notions. This is due partly to its long and controversial history. But it is to be explained also by the fact that the term "matter" has been employed to designate features of nature which are widely different from one another. An examination of the great variety of contexts in which the term has been used demonstrates this ambiguity quite clearly. For example, "matter" may be used as synonymous with "thing," "body," or "substance," to be contrasted with "property," "aspect," or "quality"; or it may be used as equivalent to "particular," as opposed to "universal"; or "content," as opposed to "form"; or "physical," as opposed to "mental" or "spiritual"; or "real," as opposed to "unreal" or "imaginary." It seems obvious that no progress can be made in the analysis of the concept unless there is an organized attempt to differentiate these meanings sharply from one another.

Fortunately, not all of these meanings are equally relevant for science and philosophy, and, as a consequence, a discussion of the scientific meaning of the concept may be prefaced by a definite rejection of certain of the philosophical meanings as having no importance. On the basis of this principle it is possible to limit one's consideration somewhat. Probably most scientists would agree that their task is not to explain matter either in the sense of the physical as opposed to the mental or spiritual, or in the sense of the real as opposed to the unreal or imaginary; both distinctions, they would insist, may be legitimate, and must be recognized by science provided they can be expressed in verifiable terms; but the problem of finding such distinguishing criteria is one belonging to philosophy and not to science. Similarly, the majority of scientists would be of the opinion that the distinction between content and form can be reduced to the distinction between substance and quality if form is considered in the sense of shape or figure, and to the distinction between particular and universal if form is considered in the sense of type, or abstract character. Finally, most scientists would presumably be in agreement that the distinction between particular and universal, while it must be recognized by the scientist, does not constitute a scientific problem as such but a logical and metaphysical problem. Without doubt everything that the scientist observes is a particular, whether it be a thing, a process, a happening, a duration, an extent, a relation, or even a number. The concept "particular" is therefore approximately synonymous with the concept "event," as this latter term was described in Chapter III and has been used throughout the discussion. But it hardly seems the job of the scientist to determine the nature of this basic scientific entity. Every scientific entity is an event but certain events are material and certain events are not, and it is the task of the scientist to determine the character of these more concrete types of entity. The scientific problem of matter is the problem of the ultimate stuff of nature, not in the sense of the most abstract natural entity, but in the sense of that type of event to which all other events can be related as attributes, relations, processes, happenings, or changes. Material events are fundamental only in the sense that all other events are referential toward them.

The scientific problem of matter, therefore, reduces to the first of those given above. Matter is that which is most adequately described by such vague common sense words as "thing," "body," "object," and "substance." The further problem, then, is to determine as precisely as possible what these terms reduce to at the empirical level, what content the scientist gives to them when he employs them as tools in his investigation, and by what operational techniques the latter may be derived from the former.

When one turns to the problem of the empirical foundation of the concept of matter he is impressed with an important feature. Things are complexes of properties and aspects. A table, for example, is a complex of shape, color, weight, hardness, gloss, smoothness, warmth, etc. Furthermore, it appears at the empirical level that by the substance or material of a thing one can mean only this complex or some selected part of it. There is nothing given in observation that can be called the substratum of the qualities. One can observe only the qualities, not "that which possesses the qualities." At this level matter must be defined not as that which explains the given, but as the given itself. Locke's conception of substance as the unknowable substratum, Berkeley's conception of substance as God, Mill's conception as the Permanent Possibility of Sensation, and all historic atomisms, are not descriptions of the given but interpretations of it through constructs and hypotheses. On the strictly empirical level a thing must be defined simply as a certain togetherness of sense-data; the object *is* the totality of its manifestations or attributes.

However, although matter is capable of definition only as a complex of qualities and relations, certain of the latter are considered more basic than others, and hence as affording the *defining* properties of matter. The empirical properties of matter are approximately as follows:

1. *Location and continuous endurance through time.* A complex of properties which is called "matter" seems to have at least an approximate location in time, i.e., one is able to say that there are certain complexes which it precedes and certain others which it follows. This involves no more precision than is possible upon the empirical level. Furthermore, the complex seems to endure, usually through a longer rather than a shorter time; hence there is no such entity as an instantaneous thing, and a complex of properties is more certainly characterizable as matter if it lasts for a relatively long time. Finally, matter seems to have a temporal identity; it endures continuously and exhibits no breaks in its life.

2. *Relative permanence.* A complex which is called "matter" is usually relatively unchanging. When the change is rapid the complex is commonly spoken of as a happening rather than as a thing; a flash of lightning, the fall of a leaf, an explosion, and a fire are not matter but happenings. Here, again, there is nothing empirical corresponding to "that which changes" except as one means by this phrase simply the complex itself. "Consider, say, a wall-paper which fades in the course of years. It is an effort not to conceive of it as one 'thing' whose color is slightly different at one time from what it is at another. But what do we really *know* about it? We know that under suitable circumstances—i.e., when we are, as is said, 'in the room'—we perceive certain colors in a certain pattern: not always precisely the same colors, but sufficiently similar to feel familiar. If we can state the laws according to which the color varies, we can state all that is empirically verifiable; the assumption that there is a constant entity, the wall-paper, which 'has' these various colors at various times, is a piece of gratuitous metaphysics. We may, if we like, *define* the wall-paper as the series of its

aspects. These are collected together by the same motives which led us to regard the wall-paper as one thing, namely a combination of sensible continuity and causal connection." [1]

3. *Location and continuous extension in space.* The complex exhibits relations of distance and direction in space to other complexes. This feature of matter is commonly considered as one of its most basic properties. Furthermore, at the empirical level matter is also extended, and therefore exhibits shape and size. Moreover, it has spatial identity; i.e., it occupies an unbroken extent. Finally, by virtue of its location in space it is capable of motion, since it is not tied down to one point in space rather than to another.

4. *Force.* One element of the complex is that property which is empirically revealed as force. This shows itself both in a relative impenetrability, exhibited in the resistance which the complex offers to pressure, and in weight, exhibited in the resistance which the complex offers to movement away from the earth. The property of force is also considered to be one of the most basic properties of matter. Matter is what one bumps against in the dark, and what one experiences when he carries a heavy suitcase.

5. *Secondary qualities.* In addition, matter is presumed to possess certain qualities such as heat, color, sound, taste, and odor, which, because of their great variability, are not taken as defining properties. Some of them may, on occasions, be completely lacking.

MATTER: SCIENTIFIC CONTENT

Matter as it functions in science is a refinement of empirical matter. The common practice in science involves the substitution of more precise terms for the essentially vague terms applicable at the empirical level. Lenzen uses the term "body" to designate this basic entity of physical science. "A thing considered merely from the point of view of physical properties will be called a body; that is, a body is something which has general measurable properties and

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[1] Bertrand Russell, *Our Knowledge of the External World,* p. 106.

relations."[1] An alternative term is "particle," which has already been employed earlier in the chapter. A particle may then be defined as that which exhibits certain measurable spatial, temporal, and force properties. A particle may be defined by approximately the following postulates:

1. *Every particle exhibits location in time.* This is simply the refinement of the corresponding empirical property of things. It asserts that a particle occurs in the temporal stream, though it does not state that the particle endures.

2. *Every particle exhibits location in space.* This, again, asserts not extension but mere location. Spatial location is considered one of the basic properties of particles.

3. *Every particle is at some point or other at all instants.* This expresses the enduring feature of particles, and justifies the principle of the indestructibility of matter. It demands that every particle have a certain recognizable identity through its history; this persistent property is, as will be seen immediately, its mass. Whatever changes in space or time a particle undergoes, its mass remains constant (subject to certain corrections due to the general theory of relativity).

4. *No particle can be at two different points at the same instant.* This states definitely that a particle is not extended, a refinement which is introduced by science to avoid complications of size and shape. In many mechanical transactions a thing acts as though it were located at a point, hence variations of extent and shape are of no significance.

From these postulates the property of motion may be deduced as a corollary. If a particle occupies and endures through time, and if it occupies one but not more than one point in space at an instant, it is capable of motion and rest as these notions were defined earlier in the chapter.

5. *No two particles can be at the same point at the same instant.* This is the justification for the principle of the impenetrability of matter.

6. *Every particle exhibits mass.* This is the scientific substitute for the vague empirical notion of force. It is de-

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[1] *Physical Theory*, p. 19.

manded, as has been seen, by the necessity for finding a method of measuring the latter. Mass as a property of particles is preferable to weight, since this varies from point to point on the earth's surface and limits the generality of one's system of mechanics. Mass is the one property of particles which is preserved throughout mechanical transformations, hence it becomes their identifying feature. From this point of view matter is commonly defined in science as that which exhibits mass.

By means of some such postulate scheme as this, the scientific notion of matter receives definition. It is to be noted that all of its properties are expressible in terms of numbers which are obtainable through the medium of clocks, meter rods, and coördinate systems. Time, space, and mass are, in fact, the three basic quantitative notions in physics. Together they define matter, and in various combinations afford the foundation for all other physical measurements. It should be noted, further, that the scientific conception of matter involves no reference to the so-called secondary qualities; particles do not exhibit heat, color, sound, taste, or odor. These are not features in which things act as though they were located at points; on the contrary they are properties known to be related to qualitatively distinct *parts* of things, e.g., molecules, atoms, electrons, and neutrons, etc. They are therefore to be accounted for in terms of hypotheses of matter, not in terms of idealized constructions.

MATTER: OPERATIONAL DERIVATION

As in the case of force, matter functions in science only as *measured* matter. The operational transformation of the empirical concept of matter therefore consists in the introduction of the refinements which are necessary in order to permit the basic properties of matter to be measured. These fundamental qualities are of three kinds: space, time, and force. Hence the operational route from empirical matter to scientific matter involves the passage from empirical space to geometrical space, from empirical time to scientific time, and from empirical force to mass. These routes have already been examined. They involve the attempt to replace the crude and complex qualities of empirical data by the more refined and simpler quantities of science. The result is the definition of "particle" which was given in the preceding section.

But it should be noted that there is an alternative definition of the concept of particle which claims to be more empirical. Instead of defining it as *that which possesses these measured values*, one may define it merely as *the complex of these measured values, together with the functional relations connecting them.* If empirical matter is to be defined in terms of the totality of its qualitative manifestations, scientific matter should be defined in terms of the totality of its quantitative manifestations. Now the difficulty with the former conception of particle is that it retains the unjustifiable empirical notion of a substratum as that in which the qualities inhere; a particle is defined as *that which* possesses the measured values rather than as the *mere collection of the measured values.* But if there is no evidence at the empirical level for this underlying substance, there can be no evidence at the scientific level. Hence the alternative definition claims to be more empirical. Matter should be defined merely as it can be made known to the scientist, viz., as a cluster of measured values having a certain constancy as a group and having a certain functional interdependence among themselves. Scientific matter, as empirical matter, is the class of its manifestations; but for science these manifestations occur in the form of numbers on recording devices, not in the form of qualitative data. Hence a particle should be defined not as something which possesses certain measurable properties but as the complex of laws stating the interrelations of certain measured values.

An examination of actual scientific treatises, however, reveals the fact that the more empirical definition of scientific matter has not been achieved in fact. It is clear that the closer empirical reference is retained only at a great cost to simplicity. The scientist finds it much more convenient to talk in terms of bodies and particles as though they were *at* points, and *at* instants, and *exhibited* forces. Upon the basis of careful operational derivation one should say, rather, that the collection of measured values exhibits a dominant spatial value, or temporal value, or force value. As a result of the awkwardness of this expression the scientist still retains the terminology of the substratum, though he may admit that this terminology is a mere convenience and is not to be taken at its face value. Scientific matter may consequently be defined in terms of the *particle*, as that which exhibits certain measurable spatial, temporal, and force properties.

In summary, matter at the empirical level is simply a group of qualities, or aspects, which seem to act essentially as a unit; fundamental among these aspects are those associated with space, time, and force. At the scientific level matter is simply the refinement which is introduced into this concept when the attempt is made to determine precisely what its spatial and temporal locations are, and how intense is its force manifestation. This results in the definition of particle, either as that which exhibits a number of measured values, or as the complex itself of these measured values. The operational route employed in this transition is such as is involved in the passage from empirical space, time, and force to the scientific refinements of these notions.

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CHAPTER XVI

LAW, CAUSE

The concepts to be discussed in this chapter occupy a slightly different position in the table of the sciences. They function not explicitly, as do the concepts already examined, but implicitly. Number, order, quantity, time, space, force, motion, and matter—each of these concepts constitutes, with the possible exception of *time*, the subject matter of one or more of the special sciences. Hence, in each of these cases the scientist tells—not always in clear terms, to be sure—what he means by the concept in question. A continuation of this type of discussion would require similar examination of the concepts of life and mind, which constitute the basic notions of the biological and psychological, or humanistic, sciences. Since the present book makes no claim to completeness, and since the concepts in these more complex sciences are even more controversial as to meaning and empirical reference than in the cases of the concepts already examined, the analysis of the explicit concepts will stop at this point. But there runs through the sciences a certain group of basic concepts expressive rather of the structure than of the content of the investigations—concepts which are not, as a rule, subjected to critical examination by the scientists who use them. Typical among such concepts are the following: event, quality, relation, operation, uniformity, objectivity, change, symbol, hypothesis, experiment, law, and cause. Some of these concepts have already been subjected to critical examination in Part I, and the present chapter might well have occurred in that context. However, the concepts of *law* and *cause*, intimately associated as they are with such notions as measurement, time, space, and force, are better examined in the discussion context of these latter notions. The present chapter will approach these concepts from the point of view of the triadic pattern employed in the preceding chapters.

However, the interrelation of the two concepts makes a separate examination of each of them unnecessary. A causal correlation of events is obviously a special type of repeated association; hence if a law represents the repetition of connections between events, a causal law will represent a specifiable kind of connection. The notion of cause enters into science—if it enters into science at all—only in association either with causal laws as stated in symbols, or with causal ties as holding between events. But in either case the notion of law is directly involved. Consequently, the discussion in the present chapter will center about the notion of law, of which causal laws will be shown to be a special case.

LAW: EMPIRICAL FOUNDATION

However nominalistic one may be in his attitude toward scientific law, considering it merely as "conceptual shorthand" or "convention," he is obliged to recognize that there is something at the empirical level to which it has direct or indirect reference. Nature, even to the most skeptical, exhibits approximate uniformities, repeated associations, recurrent sequences, or something of an equivalent character. As was pointed out in Chapter VI,[1] events have the property of occurring together—occasionally, frequently, or universally. While the events themselves do not properly recur, since an event is unique in any situation in which it occurs, nevertheless approximately similar events do occur in other situations and often with approximately the same associates. This fact demands recognition in any symbolic scheme, for it suggests that the repeated association of an event of one kind with an event of another kind is an important bit of information about the character of each of the events. As a consequence, the symbolic statement of this repeated correlation, which is called a law, has informative importance with reference to the *meanings* of the concepts

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[1] Page 123.

functioning in the law. A law may be tentatively described at the empirical level, then, as a statement of a *repeated association of events.* The examination of this formulation will require a more detailed consideration (1) of the kinds of events which may be correlated, (2) of the nature of the correlation, and (3) of the meaning of "repeated."

(1) In general, any event whatsoever may be correlated with any other event. The coming of spring is associated with the appearance of robins; the ring around the moon with stormy weather; the excessive size of an elephant with excessive weight; freezing weather with bursting water pipes; drinking arsenic with death; the equality of angles in a triangle with equality of sides; black hair with dark skin; and so on. Events, in other words, tend to gather into clusters, and the clusters tend to repeat. In fact, as was seen in the last chapter, the ordinary conception of "thing" is precisely such a cluster of events, having a certain unitary character which permits inference from one of its aspects to another. The statement of the correlation of these aspects constitutes a law. Whether it is events or aspects which are correlated is of no importance, since the difference is merely in mode of speech. Strictly speaking, only events are correlated, though when events are united into complexes one tends to describe the elemental events as aspects of the complex event.

What requires special mention at this point is the fact that on the strictly empirical level the events which are correlated are usually described in qualitative rather than in quantitative terms. It should not be insisted dogmatically, however, that an empirical law cannot be quantitative. Much of the literature of science, for example, distinguishes between two types of quantitative law. On the one hand are empirical laws which are expressed not as functional relations between variables but as tables of crude measured values which seem to exhibit a semblance of proportionate variation, as in the measured values of the spaces covered by a ball rolling down an inclined plane correlated with durations. On the other are theoretical laws which are expressed as functional relations between variables. The latter type of law is strictly scientific, and an analysis of it will be made in the proper place. The question at issue is whether the tables of values (or the plottings of them on coördinate schemes) constitute laws at the strictly empirical level. The question is not important since, as has already been seen, there are many levels of the empirically given. Throughout the discussion thus far the point of view has been taken that the recognition of precise quantitative features of the given involves the beginnings of the transition to the scientific point of view. At the lowest of the empirical levels, though quantitative differences are obscurely recognized, measurement techniques have not yet been employed; hence at this level a law states merely the fact of presence or absence, with little or no awareness of the possibility of degrees of presence or absence. For this reason a table of measured values, though it is often called an empirical law, will be considered from the point of view of this discussion as a rudimentary scientific formulation.

(2) The structure of correlations is the structure of space and time, hence the types of laws will be determined by the types of spatio-temporal relations. Events occur at the same or at different times, and they may exist at the same or at different spatial locations. This fact determines two general types of laws: laws of coexistence and laws of succession, each of which may describe the correlation of events which occupy the same space or different spaces. If the world were a perfect system, and if one's knowledge of its laws were complete, then from a knowledge of an event here and now one could determine by means of laws of coexistence what is happening now everywhere, and by means of laws of succession what will happen and what has happened. Such a state of affairs is not at all apparent on the empirical level, however. All that seems to be given is the clustering of events into spatial groups and into historical continuities; the persistence of this associative character leads to the formula-

tion of laws of coexistence and laws of succession. A law of coexistence states that an event of a certain kind is repeatedly found at the same time as an event of another kind. A law of succession states that an event of a certain kind is repeatedly found preceding (or succeeding) an event of another kind.

It seems clear that causal laws constitute a special type of law of succession. Whatever else may be said as to the nature of the cause and effect correlation, certainly the cause must always precede the effect. But the problem in this connection is to determine precisely what it is that constitutes the unique feature of a causal law as distinguished from a law of *mere* succession. Can it be maintained, as Hume, Mill, Mach, Pearson, and the contemporary positivists have insisted, that causal connection is indistinguishable from mere succession? Or must it be insisted, as Whitehead has claimed, that a causal succession exhibits a peculiar feature which must be recognized? The importance of this issue warrants a brief examination of it.

Hume's analysis of causal connections has become classic in the history of philosophy. "When we look about us towards external objects, and consider the operation of causes, we are never able, in a single instance, to discover any power or necessary connexion; any quality, which binds the effect to the cause, and renders the one an infallible consequence of the other. We only find that the one does actually, in fact, follow the other. The impulse of one billiard-ball is attended with motion in the second. This is the whole that appears to the *outward* senses. The mind feels no sentiment or *inward* impression from this succession of objects. Consequently, there is not, in any single, particular instance of cause and effect, any thing which can suggest the idea of power or necessary connexion."[1] Furthermore, argues Hume, no idea of necessary connection can be derived from feelings of "internal power." "An act of volition produces motion in

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[1] *An Enquiry Concerning Human Understanding* (Chicago: Open Court, 1912), p. 64.

our limbs or raises a new idea in our imagination. This influence of the will we know by consciousness. Hence we acquire the idea of power or energy; and are certain that we ourselves and all other intelligent beings are possessed of power." But this does not demonstrate the existence of a necessary connection between will and motion. "This influence, we may observe, is a fact, which, like all other natural events, can be known only by experience, and can never be foreseen from any apparent energy or power in the cause, which connects it with the effect, and renders the one an infallible consequent of the other. The motion of our body follows upon the command of our will. Of this we are every moment conscious. But the means, by which this is effected; the energy, by which the will performs so extraordinary an operation; of this we are so far from being immediately conscious, that it must for ever escape our most diligent enquiry." [1]

Sharply opposed to this is the claim of Whitehead. Suppose that in the dark an electric light is suddenly turned on and a man's eyes blink. "The sequence of percepts, in the mode of presentational immediacy, are flash of light, feeling of eye-closure, instant of darkness. The three are practically simultaneous; though the flash maintains its priority over the other two, and these two latter percepts are indistinguishable as to priority. According to the philosophy of the organism [Whitehead's own philosophy], the man also experiences another percept in the mode of causal efficacy. He feels that the experiences of the *eye* in the matter of the flash are causal of the blink. The man himself will have no doubt of it. In fact, it is the feeling of causality which enables the man to distinguish the priority of the flash; and the inversion of the argument, whereby the temporal sequence 'flash to blink' is made the premise for the 'causality' belief, has its origin in pure theory. The man will explain his experience by saying 'The flash made me blink'; and if his statement be doubted, he will reply, 'I know it,

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[1] *Ibid.*, p. 66.

because I felt it.'"[1] Effect arises out of cause because the future is immanent in the present. "The future is immanent in the present by reason of the fact that the present bears in its own essence the relationships which it will have to the future. It thereby includes in its essence the necessities to which the future must conform. The future is there in the present, as a general fact belonging to the nature of things. It is also there with such general determinations as it lies in the nature of the particular present to impose on the particular future which must succeed it. All of this belongs to the essence of the present, and constitutes the future, as thus determined, an object for prehension in the subjective immediacy of the present."[2]

If Whitehead's statement is obscure, that of one of his followers is unambiguous: "In examining my experiences I discover cases of causal efficacy: for example, I may be angry at one moment and calmer at the next. I not only perceive that the moment of calm follows the moment of anger, but I perceive the moment of calm rise out of the moment of anger. In the very moment of calm I perceive the influences of the previous feeling. The experience not only follows but inherits the emotional tones of past experience. In general, I perceive genuine causal influence when I become conscious of the relations of emotions and feelings. Regarding each feeling or emotion as an event, there is here an indubitable experience of the causal relation of events."[3]

The decision between these sharply opposed points of view is a difficult one. From one point of view the issue is irrelevant to science. As will be seen later, regardless of what may be the case at the empirical level, at the scientific level nothing like causal efficacy seems to play an important part; for science a purely descriptive view is adequate. But from another point of view the issue *is* relevant. Though science does not require the notion of causal efficacy it does

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[1] *Process and Reality* (New York: Macmillan, 1929), pp. 265–266.

[2] *Adventures of Ideas* (New York: Macmillan, 1933), p. 250.

[3] Mortimer Taube, *Causation, Freedom, and Determinism* (London: Allen and Unwin, 1936), p. 155.

require, at least according to the prevailing view, the notion of *necessity*, and the problem arises as to what can be the empirical foundation for this notion. Is necessity a property of scientific law merely because, as Hume maintained, the correlation occurs *repeatedly?* If so, it is hard to see how necessity can be found in a collection of cases if there is not the slightest hint of necessity in the individual case. Or is necessity a property of scientific law because, as Whitehead maintains, each individual case exhibits necessity? If so, it is hard to see how one can ever be mistaken as to a causal connection. The view which will be taken on this issue can be more clearly indicated by turning to the third aspect of law, viz., that which is involved in the notion of repetition.

(3) It seems unquestionable that a single correlation or association of events does constitute a fact about each of the events. An event is not merely what it is "in itself" but what it is in its associations with all other events. Hence to know with what events a given event is *once* associated, *occasionally* associated, *frequently* associated, *universally* associated, and even *never* associated, contributes to the understanding of the nature of the given event. Furthermore, it seems hard to deny that the information contributed in each of these cases is different in degree. One may call this feature of an association its *empirical necessity*. An association which occurs only once has a low degree of empirical necessity; associations which occur occasionally, frequently, and universally have continuously increasing degrees of empirical necessity.

At the empirical level little or no attention is paid to the degree of empirical necessity involved in any association. Unless a specific problem is involved cases are not counted, and exceptions are not noted. Presumably no attention is paid to any association unless it occurs at least occasionally. This accounts for the fact that a law is customarily described as a repeated association of events. The operational transformation of the notion of empirical law involves, as will be seen later, the refinement in the recording of the number of associations, the ascertainment of the degree of similarity holding between them, the noting of exceptions, if any, and consequently an assignment of the *degree of probability* to the scientific law. Degree of probability and necessity constitutes the scientific refinement of variations in empirical necessity. The further complications in this problem will be discussed in the proper place. Here all that is required for emphasis is that a single correlation involves a finite degree of empirical necessity. Whether this is to be reduced, as in the case of Hume, to a subjective factor such as expectation, or whether it is to be considered as existing in the association itself as an objective property capable of additive increase through multiplication of cases—these are questions which are perhaps not important for a philosophy of science.

In summary, an empirical law is a repeated association of events. The events so associated are usually qualitative rather than quantitative in character, and the association itself is either of coexistence or of succession. Causal laws are the most important type of law of succession, and may designate either mere sequences which occur repeatedly, or connections which exhibit causal efficacy. Every empirical law possesses a greater or lesser degree of empirical necessity, depending upon the frequency with which the association occurs.

LAW: OPERATIONAL DERIVATION

An empirical law becomes a scientific law through three significant operations: (1) measurement, which replaces qualitative events by quantitative ones, (2) a highly complex operation involving generalization, interpolation, and approximation, which replaces special types of empirical correlation by timeless logical interrelationships, discontinuous values by continuous variables, and highly complex correlations by simple functions, and (3) ascertainment of frequencies and degrees of analogy, which replaces vague recognition of empirical necessity by more or less precise statements of necessity and probability. Each of these will be examined in greater detail.

(1) Although, as will be shown in the next section, the question as to whether all scientific laws are quantitative rather than qualitative in character is debatable, nevertheless from the point of view of operational techniques measurement is the most important method by which precision is introduced into science. If one agrees, for the moment, that most scientific laws are expressible in terms of measured values, the meaning of the notion of scientific law must depend on an analysis of the operational techniques employed in procuring measured values. This implies that the meaning of scientific law is tied up with measurement and all of the assumptions on which it is based. It implies, further, that from this point of view a scientific law states not a correlation between events in the strict sense of the term but rather a correlation between numbers on measuring instruments and recording devices. For example, the law of falling bodies replaces spaces by numbers on meter sticks, and times by readings of clocks; what it states is that the numbers obtained from one recording device vary in a certain way as compared with the numbers obtained from another device. And in a deeper sense the meaning of the law is a function not only of the methods of obtaining the numbers, but also of the methods of constructing and calibrating the instruments. This dependence of law upon the techniques by which it is derived is in complete harmony with the dependence of scientific concepts in general upon the methods by which they are measured. Repeated reference to this feature of scientific concepts has been made in the preceding chapters.

(2) The operational techniques which are involved in the transformation of empirical correlations into precise functional relations are highly complex in character. They may be discussed briefly as (a) *generalization*, (b) *interpolation*, and (c) *approximation*. (a) *Generalization*, or abstraction,

is the method by which the difference between laws of coexistence and laws of succession is lost in science. Empirical necessity becomes generalized into logical or mathematical necessity. Empirical laws state the dependence of the occurrence of one event upon that of another; scientific laws state the dependence of one measured value upon another. In the case of causal laws one may say that science generalizes the notion of cause and effect into the notion of reason and consequence—an operation which involves loss of the temporal features of the empirical correlation. The outcome of this operation, and its justification, will be discussed further in connection with scientific law. (b) *Interpolation* is the serial operation by which the discontinuity of measured values is replaced by a continuous function. The presumption is that the measured values obtained are an adequate indication of the general type of serial order involved, and hence that interpolation of probable values between any two actual values is justified. In the case of a ball rolling down an inclined plane, for example, the discrete values representing the positions of the ball at successive moments may be replaced by a variable which changes continuously, since the reasonable assumption is that the ball behaves in between any measured points in essentially the same way as it does at the measured points. (c) *Approximation* is the method of idealization by which a simple mathematical function is substituted for a table giving parallel columns of measured values. Such a complex table is, of course, a scientific law in the most general sense of the term. But simplicity of correlation is much to be desired in the interests of intelligibility; consequently simple rather than complex relations are sought in every case. Approximation is the method by which discrete values are replaced by points plotted on a coördinate system. Due to the inaccuracies of measurement the points seldom lie on a smooth curve. But by increasing the number of points one can obtain a general notion of the path of the curve, and the line may be drawn even though it does not pass through all of the points. If this line can be expressed in a simple functional formula, the desired approximation has been achieved. Examples of this process can be found in almost any elementary textbook of science.

(3) Since scientific laws state *repeated* associations of events, an analysis of this important notion is required. Whether a law is asserted as true without exception, or merely as true in general, is a question of its derivation or justification, i.e., a question of the logical techniques by means of which it was obtained, or by means of which it must be verified. A problem of this kind is usually called *inductive* according to one of the many meanings of this ambiguous word. A few remarks may be made on the inductive problem.

The character of the problem is not hard to discern. Empirical correlations are, as was seen, of various kinds—single, occasional, frequent, and universal. Such correlations exhibit increasing degrees of empirical necessity, and hence increasing degrees of relevance in the problem of understanding the structure of nature. It seems clear that science is more interested in correlations possessing a high degree of necessity than in those possessing a low degree. Hence it is one of the important aims of science to express scientific laws with as wide a scope of generality as the evidence permits. The occurrence of a given correlation creates a finite probability that it will repeat, and the common assumption of science is that, as the number of situations in which the correlation occurs increases, the probability that it will be found in still further situations also increases. There is always a tendency, therefore, for the scientific formulation of laws to run ahead of the evidence, i.e., for laws to be asserted with wider generality than the actual situations justify. This determines the inductive problem, which may be formulated approximately as follows: *On the basis of an association known to hold in a finite number of cases, what may be asserted either as to other cases not examined or as to all possible cases?*

Though the problem has had a long and venerable history, the developments since John Stuart Mill are of particular importance. Mill and his predecessors seem to have believed that universal laws could be logically justified if one could only find the proper premise. This premise was commonly located in a feature of nature which was called its uniformity. Granted that nature is uniform, then what is true in a few cases will be true in all cases, *provided the cases in question are typical.* But this is precisely the difficulty; how can one determine whether the cases are usual or unusual? The fact seems to be that nature exhibits disuniformity as well as uniformity, and one cannot tell in any given case which is revealed. What is required, therefore, for the logical deduction of the principle of induction, is not merely the principle of the uniformity of nature but a further principle which will tell whether the cases observed are such as to come under the general assertion of uniformity.

This need seems to have turned the attention of investigators into new directions. The focal center of the problem of induction has shifted, particularly in the present century, from *necessity* to *probability.* It is now seen that the principle of the uniformity of nature, being itself an inductive generalization, can hardly afford a basis for grounding any particular law. Attention is therefore turned to the actual cases, with a view to determining whether they are such as to permit generalization. Progress on the inductive problem can be made if principles can be formulated describing the effect which such evidence as the number of instances of the association and the degree of similarity of the various instances has upon the probability of the law. No law is stated to be necessarily true. Every law has a higher or lower degree of probability, and probability is meaningless without reference to the evidence on which it is based. The two most important contributors to the problem in recent years have been Jean Nicod and J. M. Keynes. A brief reference may be made to the essential contribution of each.[1]

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[1] J. Nicod, *Foundations of Geometry and Induction* (New York: Harcourt, Brace, 1930); J. M. Keynes, *Treatise on Probability* (London: Macmillan, 1929).

Keynes insists that the problem of induction is intimately connected with two problems, both of which are important in the determination of the probability of a generalization. On the one hand is the *likeness* which holds between the several cases; this is called Analogy. On the other hand is the *number* of cases; this is called Pure Induction. But Keynes insists that "an increase in the *number* of experiments is *only* valuable in so far as, by increasing, or possibly increasing, the variety found amongst the non-essential characteristics of the instances, it strengthens the Negative Analogy."[1] By Negative Analogy Keynes means the dissimilarity in non-essential characteristics of the instances. Hence in the supposition, say, that metals conduct electricity, the probability of the law is increased by finding more and more cases in which it holds; but the effect of these instances is not in their number but rather in the fact that by increasing the *variety* of the cases one is able to narrow the scope of the generalization. By this method the known negative analogy is increased. But the positive analogy must also be increased. This can be done both by decreasing the determinateness of the predicate-term and by increasing the determinateness of the subject-term of the law. By this method the cases which do not properly come under the law are excluded, and possible exceptions are thus avoided. Symbolically stated, a law of the form "all A is XY" is less probable than one of the form "all A is X," and this, in turn, is less probable than one of the form "all AB is X." For example, that all Irishmen are red-headed and quick-tempered is less probable than that all Irishmen are quick-tempered; and this, in turn, is less probable than that all Irishmen from Cork are quick-tempered. Hence probability can be increased by defining less narrowly the known resemblances as expressed in the predicate-term, and by defining more narrowly the known resemblances as expressed in the subject-term.

Nicod agrees with the greater part of this analysis, but

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[1] *Op. cit.*, p. 219.

feels that Keynes has neglected the effect on the probability of a law due to the mere increase in the number of cases. Nicod is responsible for the introduction of the convenient terms "confirmation" and "infirmation."[1] A law "all *A* is *B*" is *confirmed* by any case in which *A* and *B* occur together, and *infirmed* by any case in which *A* occurs in the absence of *B*. It is generally recognized that infirmation definitely decreases probability but not that confirmation increases probability. Nicod, however, insists upon the fact that mere multiplication of instances does increase the probability of a law, entirely apart from the fact to which Keynes referred that such multiplication increases the negative analogy. Such a contention, however, rests upon two assumptions which Nicod states explicitly. "It is at first necessary that the law possess, from the very start, a probability that is not null no matter how small it may be." And "it is necessary, besides, that on the hypothesis that the law is false, its successive verification in an infinite number of cases is infinitely improbable; or in more precise terms, that its improbability exceeds any limit for a sufficiently large number of cases."[2] According to Nicod's thesis, therefore, if one grants that a single instance of an association contributes a finite probability to the law (or possesses a minimum of empirical necessity, in the terminology employed earlier in the chapter), and if one grants that the multiplication of such instances to infinity would be infinitely improbable if the law were false, then each new instance even though it introduces no differing factor does increase the probability of the law. For example, the illustration of the Irishmen may be formulated, somewhat inaccurately, in the following terms: If the existence of a single Irishman who is quick-tempered makes it at least slightly probable that all Irishmen should be so characterized, and if it is highly improbable that one should continue to find Irishmen possessing this feature if the law that all

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[1] These words have already been introduced, though in a slightly different sense. See above, Chapter XI, p. 222.

[2] *Op. cit.*, p. 274.

Irishmen are quick-tempered is false, then the finding of a single additional case does increase the probability of the law. Under such conditions the probability of the law is increased by the mere number of cases, without regard to the element of variety which these cases contribute.

Still another approach to the problem of the operational derivation of scientific laws, closely associated with the work of Keynes and Nicod, is the attempt to determine probabilities on the basis of frequencies. Techniques of this kind become especially applicable when the correlation is known not to hold universally. The problem then centers about the attempt to determine the proportion of favorable cases as measured against the totality of cases, favorable and unfavorable, in order to determine the probability that the correlation will occur in any given case. The probability of any given case is then determined by the limiting value of the ratio of the number of favorable cases to the total number of cases, when the total number becomes very large. The probability of throwing a two on a die is $\frac{1}{6}$ since *in the long run* a two will be thrown once in every six throws. A law derived in this way is called a *statistical* law since it has meaning only if applied to large numbers. If it were known, for example, that there are Irishmen who are not quick-tempered, the probability of any given Irishman possessing this quality would be determined by counting the cases of Irishmen who exhibit this feature and then dividing by the total ascertained number of Irishmen, i.e., those possessing the property and those not possessing it.

It is well to be as clear as a brief discussion of this kind permits concerning the distinction between probabilities of the kind sought by Keynes and Nicod, and those which are based on frequencies. The former are universal laws which are only probably true, while the latter are statements of frequencies. In the former the evidence is only affirmative, in the latter it is both affirmative and negative. Both state only probabilities with reference to any individual case, but the former forbid certainty because the universality of the law is not assured, while the latter forbid certainty because the specific features of the individual are not known. Universal laws are *distributively* applicable to individual cases, while statistical frequencies are *collectively* applicable.[1] When one states that *all* Irishmen are probably quick-tempered he means that there is a certain probability that *each* Irishman is quick-tempered; but when one states that the frequency of quick-tempered Irishmen is 86 in 100 he is attributing a certain number, a ratio, to a group as a whole, and this states nothing whatsoever with reference to *each* Irishman, except that he is a member of a group possessing a certain collective property. Statistical frequencies state distributions, and distributions are applicable, basically, only to collections.

The summary of these various operational approaches to the problem of scientific law will be made in connection with the analysis of the scientific content of law. The purpose of this reference to the work of Keynes and Nicod, and to the problem of statistical correlations, has been to show that the problem of induction is no longer concerned with the establishment of necessary laws. The operational techniques are directed to the ascertainment of probability, either in the law itself or in the behavior of an individual case. Probability depends on evidence, and the ascertainment of evidence depends on the establishment of laws connecting evidence of certain kinds either with laws of certain degrees of probability or with predictions of certain degrees of probability concerning individual cases. The problem is one of great complexity, and is not likely to be solved in the immediate future. The present discussion has attempted merely to locate the problem in its proper context.

Reference must be made, however, to an alternative operational technique by which universality in a scientific law may be achieved. Though this method is employed in science with great frequency, there is some question as to whether it should be called an *operational* technique. It

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[1] See Chapter XIII, p. 262.

accomplishes its end by mere *fiat.* Any law may acquire universality by a simple act which denies the possibility that the law should have exceptions. This does not deny the existence of the so-called exceptional cases, but merely that they are properly to be called exceptions; they are asserted as lying outside the scope of the law. The law that unsupported bodies fall is not violated by balloons, since these are supported bodies and hence do not come under the scope of the law. A law which has been observed to be true in a large number of cases may be asserted as legislative over all future cases, and incapable of violation. Such a law may be said to be *nomically* necessary, i.e., true by an arbitrary fiat. In empirical laws the events are first characterized, and the association is then asserted to hold between the characterized events; but in nomically necessary laws the characterization of at least one of the events is determined by the existence of the association. If it is an empirical law that water boils at 100°, there must be some means for identifying a substance as water independent of its boiling point; but if this law is nomically necessary then whatever boils at this temperature *must* be water, and whatever boils at some other temperature *cannot* be water, for water is identified by this property. Nomically necessary laws function as definitions, since they cannot be refuted.

It seems clear that the necessity which is a feature of such laws is obtained only at the cost of objectivity. Though one can know that the law is incapable of refutation, he can never know whether it is any longer descriptive of events. Considerations of the kind raised in Chapters VII and X become relevant. The intensional features dominate over the extensional. Meaning and truth are determined not by illustration but by definition. Interest has shifted from events to symbolic systems. Knowledge is passing from description to explanation. Hence the appearance of nomic necessity in a scientific law is a signal for the passing of the science from the descriptive to the rational or explanatory stage. Nomic necessity can always be achieved through the devising of a postulate system, for any law which can be derived from such a system becomes true *no matter what;* i.e., its truth becomes of an intensional kind which is dependent merely upon a symbolic scheme, and independent of empirical reference. All laws of pure mathematics, all laws of rational mechanics, and many of the laws of physics are necessary in this sense. Since the attribution of nomic necessity to a law is an arbitrary act, any law in any science may become nomically necessary. V. Stefansson [1] argues that the definition of an ostrich as a bird which buries its head in the sand when pursued, even though there is no such bird as this, has some advantage over the definition in terms of biological properties. Since most people believe that the ostrich does this, such a definition would have social approval. A new term could then be invented to name the biological specimen. This illustrates clearly the relation between a law which is empirically necessary and one which is nomically necessary. Usually, however, the development of a postulate system is required to give sufficient stability to the intensional definition of the concept entering into the law. The future of the biological and humanistic sciences presumably lies in the development of postulate systems and the emergence of more and more laws which are nomically necessary.

The operational derivation of scientific law may be summarized in the following terms: An empirical law becomes scientific through measurement, which replaces vaguely described qualitative events by precisely designated quantitative events; through a complex operation involving generalization, interpolation, and approximation, which results in a further refinement; and, finally, through such activities as the multiplication of cases, the ascertainment of the degree of analogy applying to them, the determination of frequencies, and certain acts of arbitrary fiat—all of which replace a vaguely grasped empirical necessity by more or less precise statements of probability and necessity. These state-

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[1] *The Standardization of Error* (New York: Norton, 1927), p. 17.

ments are scientific laws, and their character may be examined immediately.

LAW: SCIENTIFIC CONTENT

Law, at the scientific level, is simply a refinement of empirical law. A scientific law also states a repeated association of events. But greater attention is paid to precision and accuracy of formulation. This feature of scientific law may be discussed from the point of view of the three aspects considered in the previous section: (1) the kinds of events correlated, (2) the nature of the correlation, and (3) the meaning of "repeated."

(1) The important problem in this connection is associated with the question of measurement. Is there such a thing as a qualitative law, or must a scientific law be quantitative? Many would insist that qualitative laws are not laws at all. Yet this seems hardly fair to the non-physical sciences. The biological and the humanistic sciences are not in a position to use techniques of measurement as successfully as are the physical sciences, since the character of the subject matter does not always permit it. Presumably all that can be required of them is the introduction of methods of measurement where the content of the science permits, and the use of other techniques of precision—such as accurate definition and ascertainment of frequencies—where the subject matter does not permit measurement. Qualitative correlations occur even in physics, and the question as to whether these are properly to be called physical laws is debatable. "Shall we classify in the list of physical laws such purely qualitative observations as the propositions: copper conducts electricity, the melting point of ice is lowered by pressure, a grating forms a spectrum, etc., or such numerical statements as that there are three states of matter, there are two types of waves possible in an elastic medium—compressional and distortional, etc.? It is conceivable that a difference of opinion may be honestly entertained here. Some authors take the view that these really

are examples of physical laws. No one doubts that they express physical facts, i.e., routines of experience. Nevertheless they do appear in a different category from the mathematical relations among symbols to which numbers may be attached by means of operations performed in the laboratory. These relations are the ones which we shall consider as forming the class of real physical laws in the sense in which the term is used in the present text." [1] However adequate such a view may be for the purpose of writing a book on physics, it is hardly proper for a general logic of science. One may well admit that a functional correlation represents the ideal of all science, but one need not admit that only ideal science is science. Clearly, carefully stated qualitative laws are scientific laws in the most general sense of the term.

Furthermore, if the scope of science is limited to quantitative laws, there is no place for those peculiar laws which describe the technique of measurement itself. In a specific case, for example, one may perform certain operations, the outcome of which would be expressed in the judgment, "The length of a certain line is 2 centimeters." This states a correlation between something qualitatively given—a length—and something quantitatively given—a number. The generalization of this process results in a number of statements which would be called the laws for the measurement of lengths. They state the general type of correlation which holds between lengths and their measured values. Laws of this kind are implicit in the functioning of all automatic measuring devices. They are not properly quantitative laws since qualitative events enter into them. Hence it seems preferable to define "scientific law" in such a way as to permit the inclusion of laws of this kind.

(2) However, although qualitative laws may occur in science, they differ from empirical laws not in the character of the correlation but in the care and precision with which the respective elements of the law are defined and identified.

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[1] R. B. Lindsay and H. Margenau, *Foundations of Physics*, p. 20.

Hence the main difference between scientific laws and empirical laws lies in the fact that functional correlations constitute an important type of the former, while they occur rarely or not at all as instances of the latter. For this reason, an analysis of the notion of *function* is important in the understanding of scientific law. One may define a function, of which the simplest type is a single-valued function, as follows: "A single-valued function of a variable x is a second variable y so related to x that whenever a value is assigned to x from the x-range, a corresponding value of y is uniquely determined in the y-range."[1] A variable represents any qualitative event which is capable of quantitative variation. Hence a scientific law, from this point of view, states not mere presence or absence but degree of presence or absence; it states not merely that two events are found frequently together, but how much of one is found with how much of the other. This involves recognition of the dependence of the law upon measured values, to which reference was made in the previous section. A law states a functional relation between variables which are qualitatively distinct from one another, and which are qualitatively distinct from variables symbolized in other ways and occurring in other laws. The fact that kinetic energy is equivalent to $\frac{1}{2}mv^2$ and the space of a falling body is equal to $\frac{1}{2}gt^2$ does not make the laws identical; for m, v, g, and t represent different variables whose values are determined by different techniques of measurement. Though the general form of the function is the same, neither the entities measured nor the techniques employed are the same; hence the laws *mean* something different in the two cases. This fact is often lost from view in theories which attempt to reduce science to mathematics.

By virtue of the establishment of functional relationships the distinction between laws of coexistence and laws of succession becomes obliterated. Mathematical laws state merely the *logical* dependence of one measured value upon

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[1] Quoted by G. A. Bliss from Dirichlet in J. W. A. Young, *Monographs on Modern Mathematics* (New York: Longmans, Green, 1924), p. 266.

another, and no attention is paid to the spatial or temporal locations of the events concerned. This is accomplished, in the case of temporal laws, by inserting the value giving the temporal duration into the equation as one of the independent variables; instead of saying that a certain value of y is followed by a certain value of x it states that a certain value of y is a function of a certain value of x and a certain value of t—the value of t, of course, being properly expressible as $(t_1 - t_0)$ since it represents a duration, i.e., the difference between two clock times. Similar considerations apply to laws of coexistence, in so far as they involve spatial intervals. The reduction of laws of succession to this purely logical form is more important since these laws "allow us to predict in advance a certain course of events; much less frequently do we need those which allow us to conclude from a state of affairs at one point or another, what is happening simultaneously elsewhere. But in principle, these two cases of 'dynamic' and 'static' causality, as we might call them, have no priority one over the other. The essential feature, the conclusion from a determined A to a co-determined B, is in both cases exactly the same, only that in one case time occurs among the necessary variables, and not in the other case." [1] Hence one may say that the difference between laws of coexistence and laws of succession lies not in the character of the functional relation itself, but in the character of the variables which are thus related; the former contain no time variable while the latter do.

But the matter cannot be dismissed too lightly, for it is tied up with the question of causal laws in science. Many believe, for example, that whatever may be said as to the existence of causal connections at the empirical level nothing of the kind is required for science. There are in science no causal laws if one means by these certain unique types of correlation differing from functional relations. For there are only two respects in which such laws could differ from the more general types of mathematical correlation—in

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[1] B. Bavink, *The Natural Sciences*, p. 75.

their reference to the property of *causal efficacy*, or in their reference to the property of *temporal asymmetry*. But the former of these properties is not a datum, and the latter is not required in the statement of a law. For example, "in the extension of a wire by weight or other force (Hooke's law), the force has popularly been called the *cause* of the extension, and the extension then figures as the effect of the force, the idea being that with the force in operation the extension takes place, and that without it the extension would not take place. . . . Now there may be a certain advantage to this popular version; certainly, it is an instinctive stand taken in everyday life. But on careful examination it must be confessed that the view has little value for physics. Thus, in the example of Hooke's law, as far as the law itself is concerned in its symbolic form, it is just as sensible to call the extension the cause and the force the effect as vice versa. . . . All that the laws state is a *relation* among symbols which represent well-defined operations in the laboratory, and no notion of precedence or antecedence, or dynamic enforcement is involved in them."[1] If, as Hume maintained, causal efficacy cannot be discovered, it cannot appear in the law as an empirical datum. Furthermore, even granting that something like "compulsion" is discoverable, certainly it cannot be measured and hence cannot occur as a variable in the mathematical statement of the correlation. On the other hand, if, as has just been seen, laws of succession can be reduced to functional relations in which time occurs as an independent variable, then even the unique before-after character of cause and effect correlations is lost. One can infer just as readily from an effect and a time to a cause as from a cause and a time to an effect, since each is the ground of which the other is a consequent. The sole differentiating features of causal laws are therefore lost, and laws of this type may be replaced by statements of functional relations.

This presumed reducibility of causal laws to functional

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[1] R. B. Lindsay and H. Margenau, *Foundations of Physics*, p. 18.

relations may be indicated in another way. As Bacon, Mill, and most other writers on causality have indicated, a causal connection can be assured only when some sort of concomitant variation has been noted with reference to the cause and effect. This is tested by introducing a variation in the intensity of the cause and noting whether there is also a variation in the intensity of the effect. If this variation can be noted, and if it is of a comparatively direct sort, the causal correlation may be presumed to be a genuine one. For example, if as the cause increases the effect increases at the same rate, the situation is judged to be truly causal. In fact, even if the cause increases more rapidly than the effect, or the effect more rapidly than the cause, there is still a reasonable ground for concluding that a causal connection exists. These would be considered direct variations. But as the variation in the values of cause and effect, respectively, becomes less and less direct, the conclusion to a causal connection becomes more and more conjectural. If, for example, a succession of changes in the cause in some interval produces no changes in the effect, the conclusion as to a causal correlation becomes relatively precarious.

Now if this feature of direct and concomitant variation in cause and effect may be presumed to characterize the causal situation definitively, there can no longer be any objection to the reduction of causal correlations to mathematical functions. For the series of proportionate values can be expressed through the method of approximation as a simple function, $e = f(c)$. In the well-tested cases e must be a single-valued function of c; otherwise a variation in e could occur without a variation in c. Now this looks very much like a causal law in the sense that it asserts a dependence of e upon c. But the distinction between dependent and independent variables in a functional relation is purely relative; consequently the law can also be represented in the form, $c = f'(e)$, in which c is also a single-valued function of e. However, this no longer looks like a causal law, for it presumably expresses a dependence of c upon e.

Yet one should note that a cause which is both *sufficient* and *necessary* does depend upon its effect in the sense that from the existence of the effect one could infer both the fact and the degree of the cause. It must be admitted, therefore, that a causal law is reversible in the sense that from the cause one can infer the effect and from the effect one can infer the cause. As a consequence, if one admits that concomitant variation characterizes the causal correlation definitively, he finds it hard to deny the legitimacy of reducing the causal law to a functional relation.

But this is just what many would deny. For in eliminating from causal correlations everything but concomitant variation, they would insist, one is rejecting precisely that feature which is essential, and which was described a moment ago as "causal efficacy" and "temporal asymmetry." In other words, there is a peculiar asymmetry in the relation of cause and effect; in the general correlational sense cause may be inferred from effect, and effect may be inferred from cause. But because of this uniquely asymmetrical feature effect depends upon cause in a way in which cause does not depend upon effect. It thus appears that nature exhibits in still another form a factor which vitiates the reduction of causal connection to functional relation. *Causal laws can be expressed as functional laws only if one neglects the fact of causal efficacy, or temporal asymmetry, or one-way dependence.* It may be that this unique and elusive feature cannot be described or located at all. But if it can, the reduction of causal laws to functional relations can be accomplished only by neglecting it.

Can this unique feature be neglected? Certainly it can upon grounds of method. Science is admittedly abstract; it employs the principle of isolation and ignores those features of the given which are presumed to be irrelevant. If the scientist considers the asymmetry of the causal relation to be unimportant, one cannot object if he chooses to exclude it in the formulation of descriptive laws—provided one recognizes that the resulting laws *are* abstract. As was suggested in Chapter XIV,[1] time is often considered at that level of abstraction which neglects its arrow. The question can be settled, therefore, largely on the grounds of convenience.

Can this peculiar asymmetric feature of nature be located and symbolized? Clearly, of the three candidates—causal efficacy, temporal asymmetry, and one-way dependence—the second appears most likely. Is there any scientific technique by which the directional feature of time may be ascertained? Is there any method by which the distinction between earlier and later may be recorded? Is there any device by which the scientist is able to distinguish between what Eddington calls a "becoming" and what he describes as an "unbecoming?" [2] The mere fact that the passage of time can be measured does not suffice. It is true, of course, that t may appear as an independent variable in the functional statement of a law. But it occurs in such an equation, in general, only as a lapse of time, i.e., a duration, and one may substitute for it indifferently a positive or a negative value. But if the functional statement is causal, i.e., of the form $e = f(c, t)$, substitution must be made subject to the condition: "t must be positive," or "if t is given a negative value this law describes an unbecoming." The question involves considerations which lead into the very heart of physical science, and hence cannot be raised here.[3] Many believe that in the phenomenon of entropy one has precisely what is required. Nature seems to exhibit a one-way process which may be described as the increase of randomness. Differences of energy tend to become equalized, hence energy passes from an available to an unavailable form. This is expressed in the second law of thermodynamics by saying that entropy continually increases. For example, if a hot and a cold body are placed in proximity, and the difference in their temperatures immediately recorded, the hot body will cool and the cold body will become warm with the result

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[1] Page 288.

[2] *Nature of the Physical World*, p. 94.

[3] See A. S. Eddington, *Nature of the Physical World*, Chaps. IV, V; R. B. Lindsay and H. Margenau, *Foundations of Physics*, Chap. V; J. Jeans, *New Background of Science* (New York: Macmillan, 1933), Chap. VIII.

that the difference in their temperatures will become less; hence a thermometer reading which exhibits great difference will always be earlier than one which exhibits small difference. Any physical process in which a reading of this kind is significant must be called an irreversible process. Examples of such phenomena were listed in Chapter XIV.[1] In situations of this kind, at least, the directional feature of time must be retained.

Hence the state of causal laws in science seems, at present, to be about as follows: Science recognizes the distinction between two types of process—reversible and irreversible—both of which seem to be exemplified in nature. Reversible correlations are capable of adequate representation in science through functional relations in which t occurs as an independent variable, indifferently positive or negative. Irreversible correlations are incapable of such representation, and require description in the general form $S_2 - S_1 > 0$, where S_1 and S_2 represent measured values indicative, respectively, of an earlier and a later state of a certain closed system. If $S_2 - S_1 = 0$ the process is reversible. If $S_2 - S_1 < 0$ the process represents an "unbecoming," hence is not empirically found. Whether or not this difference in scientific law represents adequately the difference in empirical processes, cannot perhaps, at this stage in the development of science, be decided. Only two remarks may be made. In the first place, there is no certainty that the fact of entropy is the essence of irreversible processes; hence there is no assurance that in the scientific formulation of irreversible processes the important feature of causal sequences has been retained. In the second place, the notion of causal efficacy has been lost in the scientific formulation, unless by this awareness of the immanence of the effect in the cause one means simply the temporal irreversibility of the association. These remarks are merely suggestive of further problems.

(3) Scientific laws state *repeated* associations of events. As was shown in the operational derivation, the question as

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[1] Page 289.

to whether a law is universal or less than universal is intimately associated with the problem as to whether the law is necessary or probable. An examination of the operational techniques suggested that the search for laws which are both universal and necessarily true has been more or less completely abandoned; in its stead has appeared an operational route which endeavors to base universal laws—where such laws are at all possible—on probabilities, and to substitute for universal laws—where such laws are not possible—statistical frequencies. The result of this approach is the diversification of scientific laws into somewhat the following types:

(a) Laws which are universal and necessary, but whose universality and necessity are determined intensionally rather than extensionally. This is the status of all nomic laws, which include the propositions of all "rational" sciences, especially those laws which are called "ideal." Such laws are necessary and true by virtue of the postulate systems from which they can be deduced, rather than by virtue of empirical reference.

(b) Laws which are universal, but only probably true. This is the status of most scientific laws which are not nomically necessary. Such laws have a definite empirical reference to examined cases through routes which describe the number of instances, and the degrees of similarity and dissimilarity exhibited by the instances.

(c) Laws which state statistical correlations only, and are based on examined frequencies. This is the status of all scientific laws which are not of the other two types. These laws also have a definite empirical reference, but to collections or aggregates rather than to individuals, and determined through routes which are descriptive of the counting and sorting of cases.

Such a classification is at least suggestive of the main types of law. It is probably not exhaustive. For example, no reference has been made to the distinction between microscopic and macroscopic laws, which has played an important part in recent discussion of the foundations of science. Whether this distinction is purely relative and therefore not determinative of distinctions between *kinds* of laws, or whether it is more basic than this, is not easy to state. Unquestionably the problem is intimately tied up with the distinction between individual and statistical laws, for if an event is considered to be constituted by microscopic elements it behaves both as an individual and as an aggregate. But the problem is too involved to be examined here.

Furthermore, these classes of laws are not mutually exclusive. It is possible, for example, that a law should be both nomically necessary and empirically probable. Any law which is an empirical generalization but which is beginning to take a place in a rational symbolic scheme would be such a law. And, as has just been seen, a law which is descriptive of an aggregate (and is therefore statistical) may also be descriptive of that aggregate acting as a unit, hence laws of the type (c) are not sharply distinguished from those of the type (b). Again the problems are too complex to be examined in an elementary treatise.

Reference may be made in a final section to a problem intimately connected with this, viz., the problem of indeterminacy in nature.

INDETERMINISM

Recent approaches to the problem of induction differ from earlier approaches, as has been seen, in an increased interest in probability. It has been recognized that there exists no principle called the uniformity of nature by which inference to universal scientific laws may be justified. But this does not involve an abandonment of the belief that there are strains of uniformity in nature. Even probable inferences are based on certain assumptions of constancy, and it is the task of science to select these features of uniformity and to formulate them in laws.

One of the simplest and most pervasive of the types of uniformity is that exhibited in the system of classical mechanics. Until a few years ago the laws descriptive of the movements of particles acted upon by forces were presumed to be the most perfect illustrations of purely deterministic systems. Though the systems were known to be ideal, since particles are portions of matter without extension, nevertheless the presumption was that any actual system could be made to approximate as closely as one wished to such ideal systems by progressive refinement. The law descriptive of behavior in such a system took the form of a functional relation between one variable representing the state of a system (its position and velocity) at one time and another representing its state at an earlier or later time. This law was presumed to be universal. It was, of course, nomically necessary in the sense that it was deducible from the basic postulates of mechanics and therefore could not possibly be violated. But it was also empirically verified in all cases in which a high degree of refinement could be introduced. It was therefore presumed to represent a basic structure of nature. Hence nature was supposed to be a deterministic system, at least so far as its mechanical features were concerned.

The generalized mechanical view of nature which arose out of this conception has been accurately formulated by Laplace. "We ought then to regard the present state of the universe as the effect of its anterior state and as the cause of the one which is to follow. Given for one instant an intelligence which could comprehend all the forces by which nature is animated, and the respective situation of the beings who compose it—an intelligence sufficiently vast to submit these data to analysis—it would embrace in the same formula the movements of the greatest bodies of the universe and those of the lightest atom; for it, nothing would be uncertain and the future, as the past, would be present to its eyes. The human mind offers, in the perfection which it has been able to give to astronomy, a feeble idea of this intelligence. Its discoveries in mechanics and geometry, added to that of universal gravity, have enabled it to comprehend in the same analytical expressions the past and future states of the system of the world. Applying the same method to some other objects of its knowledge, it has succeeded in referring to general laws observed phenomena and in foreseeing those which given circumstances ought to produce." [1]

It seems safe to say that some such view as this was held by most scientists prior to the discoveries which eventuated in the quantum theory. The recent interest in indeterminacy in nature can therefore be traced to those scientific discoveries which suddenly threw this whole conception into confusion. In outline, these facts were approximately as follows: In the attempt to apply the general functional equation stating the relation between two states of a given system to very small particles such as electrons, certain obstacles appeared. According to the Heisenberg principle, the two values—position and velocity—which indicate the state of a particle at a time cannot both be determined with a high degree of accuracy. Since the particle is very small, a very short wave must be used to determine its position. But a radiation of short wave length has a high momentum, since wave length and momentum are in inverse ratio to one another. Consequently, the electron when struck by this wave will be pushed from its position, and its motion changed. If this difficulty is avoided by using a radiation of long wave length, the momentum is decreased and there is less recoil, but now it is no longer possible to tell precisely *where* the electron is. Hence there arises an interdependence in the measurement of the position and velocity. If Δx be used to express the possible error in fixing position, and Δv to express the possible error in fixing velocity, this interdependence may be formulated,

$$\Delta x \cdot \Delta v \geq h/m$$

where h is Planck's constant (magnitude 6.55×10^{-27} erg-seconds) and m is the mass of the particle. Since m functions in the denominator of the fraction, the indeterminacy for

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[1] *Philosophical Essay on Probabilities* (New York: Wiley, 1902), p. 4.

masses of appreciable size is very small, hence is practically equal to zero. This accounts for the fact that no difficulties of the kind here indicated are noticed in connection with gross objects. But for particles of the size of electrons the indeterminacy becomes larger. The essential meaning of this principle for the deterministic view of nature is that position and velocity (the necessary and sufficient conditions for ascertainment of state) cannot be determined at the same time with a high degree of accuracy. Position can be determined accurately, but then velocity is measured only very inaccurately; or velocity can be determined accurately, but then position is measured only very inaccurately.[1]

The inferences from this fact to a general view of nature are of two kinds, and physicists seem to be divided about equally into two classes on the basis of their views as to these consequences. On the one hand are such men as Eddington and Compton, who conclude that the world cannot be a rigidly deterministic system; on the other, such men as Planck and Einstein, who insist that the difficulties lie merely in our techniques of observation and not in nature itself. A brief examination of these contrasting views may be made.

The former view, which may be called objective indeterminism, argues essentially as follows: The meaning of any physical concept is determined by the operations which are employed in locating the object to which the concept refers. Concepts, to be meaningful, must refer to events which can be produced in observation by describable techniques. If the operations purporting to locate an event are self-contradictory, or such as cannot be carried out without violating recognized physical laws, the concepts which

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[1] Professor Carl Eckart has called the author's attention to a very important feature of the principle of indeterminacy. There is actually no way of measuring the position and momentum of an electron or any other small particle accurately enough to confirm or infirm this inequality. On the other hand there are actual experiments that confirm the classical theory and thus contradict the principle of indeterminacy. This is, indeed, a noteworthy fact, and weakens to a very great extent the positions of all of those who argue for an objective indeterminacy upon the basis of the principle. It is surprising that some of the popularizers of the theory (Eddington, Compton, and Planck) have not chosen to emphasize this fact.

require definition through such operations are strictly meaningless. This means, in effect, that "only those aspects of the world have reality which are capable of showing themselves in some way to the observer."[1] The operational theory insists "that the meaning of position amounts to a specification of a *method* of reaching the particle."[2] Reference to this point of view has been made repeatedly in the course of the preceding pages; according to this theory, for example, *motion means measured motion*, *scientific mass means measured mass*, and so on. Hence the meaning of any quantitative notion in science involves explicit reference to the techniques employed in obtaining the measured value which is attached to it.

What, then, on the basis of this theory is the meaning of the mechanical principle which asserts a functional relation between two states of a system—state being indicated by position and velocity? Such an equation is meaningless unless the processes to be employed in measuring a state can actually be carried out. But according to the Heisenberg principle this is just what cannot be done if the state in question is that of an electron. For accurate measurement of position is incompatible with accurate measurement of velocity. The obvious conclusion is that the notion of state is meaningless in such a case, and the law stating a relation between states must also be meaningless. This seems a safe conclusion on the grounds of the operational theory. If by causal determination one means *predictability*, then causal determination must be abandoned. For if one cannot *know* both position and velocity—which are the sole grounds for prediction—one cannot *predict* the behavior of any particle.

But the objective indeterminist is not satisfied with this somewhat conservative conclusion. He takes another step, which is much more important in the determination of his outlook. He insists that *if a concept is meaningless it cannot designate any natural entity*. Hence there cannot *be* any

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[1] A. H. Compton, *The Freedom of Man* (New Haven: Yale University, 1935), p. 40.

[2] R. B. Lindsay and H. Margenau, *Foundations of Physics*, pp. 80–81.

event in nature which is of such a character that it must be located and measured by operations which are self-contradictory or physically impossible. The application of such a principle in the case at hand is obvious. Since there can be no joint *determination* of position and velocity, there can be no joint *fact* of position and velocity. The difficulty is not merely one of prediction. For if the particle cannot be known to have both position and velocity at the same time, it cannot *have* them at the same time, hence "it cannot itself know" what its behavior is to be. Not even Laplace's omniscient intelligence could predict its behavior, for the conditions of its activity are non-existent. This is, in effect, a complete denial of uniformity in certain ranges of nature. It is an insistence upon the fact of objective indeterminism.

The opposing view, while recognizing the importance of operations, denies that they function in so legislative a way in science. To be sure, argues this position, knowledge is a function of knowing techniques, but it is also a function of that which is known. Knowledge cannot reduce to mere knowing, for there is an object as well, and there are alternative routes for knowing a given object. Objective indeterminism makes the mistake of supposing that obstacles in knowing are obstacles in nature, and that incompatibilities in knowing techniques justify legislating natural objects out of existence. It projects features of knowing into the known without justification. It reduces to the absurdity of supposing that being half-conscious that one's brother is present is the same as being conscious that one's half-brother is present. Such implications are not justified by the principle of indeterminacy. All that this important principle argues for is the breakdown of *prediction* in certain situations in nature. It shows that science was wrong in supposing that from a knowledge of position and velocity a future state could always be predicted. However, this is erroneous not because there is no functional relation between the two states—one has no justification for arguing this feature out of nature—but because one cannot *know* the required state.

If the principle of indeterminacy is true, prediction is no longer possible upon the grounds disclosed by the traditional physics. But unpredictability is a limitation in our knowing and cannot reasonably be employed as a basis for arguing to the character of that which is known.

Planck argues in somewhat this vein. The only indeterminism which is demonstrated by the Heisenberg principle is a subjective indeterminism. He admits that the quantum hypothesis has introduced a disturbance into the classical theory of physics, and that "one cannot yet definitely say what influence the subsequent development of the hypothesis may have on the formulation of physical laws. Some essential modification seems to be inevitable; but I firmly believe, in company with most physicists, that the quantum hypothesis will eventually find its exact expression in certain equations which will be a more exact formulation of the law of causality." [1] On the same question Einstein reports: "I am entirely in agreement with our friend Planck in regard to the stand which he has taken on this principle, but you must remember what he has written. He admits the impossibility of applying the causal principle to the inner processes of atomic physics under the present state of affairs; but he has set himself definitely against the thesis that from this *Unbrauchbarkeit* or inapplicability we are to conclude that the process of causation does not exist in external reality." [2] "The indeterminism which belongs to quantum physics is a subjective indeterminism." [3]

It is likely that the time is not yet ripe in physics for the final solution to this controversy. More careful formulation of the operational point of view is necessary before any reconciliation is possible. In particular, the assumptions involved in measurement must be brought to light. Emphasis has been placed earlier in the book on the necessity for recognizing that an operational theory which admits measurement techniques but denies measuring instruments is insufficient. Measuring instruments are devised to measure

[1] *Where Is Science Going?*, p. 143.

[2] *Ibid.*, Epilogue, p. 210.

[3] *Ibid.*, p. 202.

something, and they must be calibrated over against that something. Hence the method of measuring a given entity cannot be exhaustive of the meaning of the symbol for that entity. By position is meant not merely the method of reaching the particle, but the qualitative feature of the particle which has been reached by this route; by force is meant not merely the method of reading indicators on spring balances and scales, but that qualitative feature of nature which is correlated in a constant way with springs and movements of beams; by motion is meant not merely the quotient of a meter rod reading and a clock reading, but that qualitatively given correlation of space and time which exhibits itself to observation. Whether this refined notion of operation has any important implications for the conclusion which may be drawn from the principle of indeterminacy remains to be seen. The importance of the principle lies in the fact that it has compelled the scientist to pay attention to operations; no longer can the method of knowing an event be neglected in the characterization of that event. But, on the other hand, the principle has had the unfortunate consequence of leading the scientist to believe that operations are exhaustive of meaning, and hence that the characterization of the event is simply what is involved in knowing the event. Both of these facts are important. What must be decided now, before any ultimate reconciliation of the opposed points of view can be achieved, is the precise extent to which one's knowledge of events is determined by operations upon events and the extent to which it is determined by the events themselves. This is the problem of the future.

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PART THREE

SPECULATIVE PROBLEMS

CHAPTER XVII

GENERAL CHARACTER OF SPECULATIVE PROBLEMS

Those problems in the philosophy of science which are called "speculative" constitute a rather miscellaneous group which defies precise demarcation from other problems and permits almost no systematization or organization. One might almost say that problems of this character are what remain in the philosophy of science when the strictly logical problems, and the problems of the meaning of the basic concepts—both of which are capable of fairly precise formulation—are removed. Speculative problems of the type to be here discussed constitute a part of the field of the philosophy of science, for they draw their data, at least apparently, from the content and the methods of the sciences. They thus build upon the sciences, and if the sciences were different either as to content or as to procedure the speculative problems would also demand new solutions. Hence they require a knowledge of the sciences for their solutions, yet they are not themselves scientific problems in the strict sense of the word.

The essential reason why the classification of speculative problems is difficult is that the systematization of such problems is itself a solution to the speculative problem. One of the most important of the speculative problems, as will be seen immediately, is that which revolves around the attempt to classify and organize the sciences and thus to construct a total picture of the various aspects of the universe in their interrelations. If each of the sciences is a specialized approach to the world of events, a totalitarian view which integrates the sciences into a systematic whole is clearly required. But such an integrated view would be precisely a classification of speculative problems, for it would attempt to show what the possible problems are, and why their solutions are required for a satisfactory view of things in general. One cannot legislate as to what the possible types of problem are and as to the way in which they are interconnected unless he has already had that inclusive view of the universe which is precisely the solution to one of those problems. One must solve the speculative problem in order to know what the possible speculative problems are.

As a consequence, one is compelled to resort to the empirical approach in the consideration of problems of this kind. Instead of saying, "Here is an exhaustive list of the possible speculative problems," one is obliged to say, "An examination of the literature in this unique field of overlapping philosophy and science discloses the fact that such and such problems have been considered most frequently by writers, and may therefore be presumed to be both important and capable of solution." Such an approach gives one not a *classification* of problems, but merely a *listing* of them. It does not show an interdependence of solutions upon one another, nor does it establish the fact that the given problems are exhaustive of the field. It does not attempt to show that there is one type of solution which is most plausible, nor even that there is a general method which is common to all of the problems. In fact, the great diversity in solutions offered—though often based upon what appear to be the same data—suggests rather the absence of any common method. For example, it is not far from the truth to say that from the mathematical character of scientific symbols Jeans infers that God must be a mathematician, and Eddington infers that He cannot be.

THE MAIN SPECULATIVE PROBLEMS

Adopting the empirical approach, one finds at least three problems lying within this field. They may be characterized, respectively as (1) *the problem of the classification of the sciences*, (2) *the problem of human freedom*, and (3) *the problem of the nature of reality*.

Certain remarks may be made at the outset with reference to this list. It is not, of course, meant to be exhaustive. There is, for example, the problem of the social implications of science, which has been treated extensively by such men as Julian Huxley, J. B. S. Haldane, and Bertrand Russell. Closely connected with this is the problem of the general effect of science in any historical period in determining the "value outlook" of that period; for example, what is to be understood when a given epoch—such as the present one—is characterized as an "age of science"? Neither of these problems is included in the list here given, yet they are genuine problems. Nor are the problems here mentioned presumed to be mutually exclusive. Often the attempt to solve one of them demands data which are properly relevant only to one of the other questions. The problem of the relation between science and religion illustrates this overlapping clearly. Presumably science has information relevant to the problem of the existence of God; this is an aspect of the problem of the nature of reality. Yet there can be no justification for using this information in the sphere of religion until the problem of the precise interrelation of the fields of science and religion has been solved; but this is an aspect of the problem of the classification of the sciences. Accordingly, one may consider the three types of problem here listed merely as illustrative of the kind of speculative problem on which scientists and philosophers have chosen to express themselves in recent years. The omission of certain problems should not be taken as implying a judgment of their unimportance, nor should the apparently sharp character of the classification of the problems be taken as implying a judgment of their mutually exclusive reference.

The general character of all speculative problems is essentially the same. Each of them attempts to show how, starting from data all or most of which are disclosed by science, one may, by an inference having a certain plausibility, conclude as to the existence of facts which are not themselves directly revealed by science. The assumption in each case is that science gives hints of facts which are not themselves scientific. Accordingly, by a careful examination of the data within science, one may be led by an inference of greater or lesser cogency to a conclusion applying outside of science. In all of the problems, therefore, three features must be made explicit: (*a*) What, precisely, are the data from which the inference starts? (*b*) What is the nature of the inference itself? (*c*) What is the character of the conclusion inferred? With reference to (*a*), it will be found that the data, in so far as they are drawn from the sciences, are not usually the *specific* facts of science itself. For example, it seems unlikely that the fact of gold melting at 1075° would have any important implications outside of science. It is rather the more *general* facts which seem to have this inferential value, e.g., the general usefulness of mathematical symbols in physics, or the inevitably analytic character of scientific symbols, or the fact of an unavoidable uncertainty in the precise location of very small particles. Yet the line of demarcation between the general facts and the specific facts cannot be drawn sharply, and it is likely that certain relatively specific facts may have important speculative implications. For example, the permanent deformations in structure which certain metals undergo when subjected to excessive strains—the phenomenon known as hysteresis—may afford justification for the contention that there is in the inorganic realm something analogous to memory, and hence that the line between the organic and the inorganic cannot be sharply drawn. With reference to (*b*), the character of the inferential route must be made clear. Is the conclusion a definite implication of the data, i.e., deducible from them, or is it merely an hypothesis having a greater or lesser degree of probability? Is it obtained through the method of construction, or through the method of hypothesis? If it is obtained by the latter method, is it based upon analogy, interpolation, serial extension, or some other recognized inductive technique? With reference to (*c*), the nature of the inferred entity must be made as clear as it can be in view of the fact that it lies outside the field of science and is therefore presumably not demonstrable by recognized scientific techniques.

A brief analysis of each of the types of problem from the point of view of these three features will be helpful as an introduction to the discussion which is to follow. A more detailed examination will be given in the specific chapters.

(1) *The classification of the sciences.* (*a*) The data in this problem are drawn exclusively from the sciences, except in so far as they must include the studies of the religious, esthetic, and moral aspects of human life—which are usually presumed to lie outside of science. The point is, merely, that any classification of the sciences must include not only the sciences proper, e.g., the mathematical, physical, biological, social, and psychological sciences, but the historical disciplines and all those normative pursuits which examine man's valuational responses. The data may be, as will be seen, either the subject matter of the various disciplines, or the methods. But unless they are drawn from a sufficiently inclusive realm the resulting picture will be partial rather than complete. (*b*) The inference is inductive in character, and endeavors by synthetic operations to ascertain the character of the whole into which the individual sciences may be presumed to unite. The whole is commonly taken to be an hypothesis rather than a construct, since it is employed for the purpose of explaining the individual sciences. The inference is from parts to whole, or from parts as unrelated to parts as interrelated. (*c*) The inferred fact is the classificatory scheme itself, which shows the precise interrelations of general, special, and coördinate sciences, pure and applied sciences, historical and non-historical sciences, composite and simple sciences, concrete and abstract sciences, experimental and non-experimental sciences, and so on. The aim of such a scheme is to reveal those relations between the sciences which are the most fruitful hypotheses for the understanding of the sciences themselves.

(2) *Human freedom.* (*a*) The data in this problem are any facts relevant to the existence or non-existence of causal connections in nature. Presumably the most significant data would be found in the realm of the humanistic sciences themselves. But the value of such data is decreased by the double fact that they are incapable of any precise formulation and they are hopelessly entangled with introspective phenomena such as the "feeling of freedom," or the "feeling of causal efficacy"; consequently, in the fields of the humanistic sciences themselves the problem seems insoluble. Recent approaches to the problem have endeavored to solve it in terms of data drawn almost exclusively from the realm of physical events. Here, at least, precision is possible. Unfortunately the problem is complicated by the fact that the data, even in this field where a maximum clarity can be achieved, do not speak unambiguously. Reference has already been made to the fact that the greatest disagreement exists among the physical scientists themselves on the question as to whether even the physical world is a deterministic scheme. The data revealed by quantum physics suggest that it is not; macroscopic data, on the other hand, suggest that it is. Consequently one's inference as to whether human freedom is or is not a fact is determined by his answer to this still more basic question. (*b*) The actual inference is, of course, merely probable, since it is based upon the legitimacy of inferring from what is true in the physical world to conditions in the realm of human behavior. If man is a part of nature the inference becomes deductive, for what is true of natural objects in general must be true of man as a specific kind of natural object. But if man is something superadded to nature the inference becomes analogical and inductive, for man may be an exception to natural objects and exhibit properties peculiar to himself. (*c*) The inferred fact is, at least in the present case, human freedom, though the alternative position of human determination is equally a solution to the general problem. The assertion of human freedom

may mean either that man is free in the sense that his actions do not violate any of the laws of physics (Eddington and Compton) or that he is subject to causal laws but unpredictable in his behavior (Planck). Both views permit man to do as he pleases, and this seems to be the demand of human freedom.

(3) *The nature of reality.* (*a*) The data in this problem are of the greatest variety. For three of the writers who will be considered, the data are features of method; for the fourth, they are a feature partly of subject matter and partly of method. The most common solution to this problem takes its origin in the recognition of the essentially limited character of science; the scientific method is shown to predetermine the sort of object which the scientist will find and to prevent him from finding an object of another kind; for example, because the scientist employs mathematical techniques he discovers only mathematical objects, and because he excludes preferential judgments he fails to find values. Accordingly, there is almost always drawn into this problem, as an essential datum, the fact of an independent knowledge of the non-scientific objects. Though the scientist does not find God in his laboratory he does find him when he leaves the laboratory and begins to live his life in the broader sense of the term. Hence the problem in this case often consists in showing the compatibility of the conclusions of science with the convictions derived through intuition, mystic insight, and revelation. (*b*) The inferences are correspondingly various. In three of the cases to be examined they proceed from the character of scientific symbols to the character of that which is symbolized; but in two of these cases the referent of the symbol is considered to be essentially *unlike* its symbol, and in the third case essentially *like* its symbol. In all three cases the inference has some plausibility only because it is based upon an inadequate theory of the nature of symbols. In the fourth case the inference is analogical, and passes from series which have limits to other series which appear to have no limits

but are presumed to have by virtue of their similarity to those which do; this permits inference to a realm of the superrational. (*c*) The inferred fact is a realm of Reality, which is presumed to lie beneath, behind, or above the data of science. Often this is identified with God, though it need not be; an atheistic materialism is frequently adopted as the solution to the problem of the nature of Reality. The solutions to be offered for examination may be called Spiritual Idealism (Eddington), Mathematical Idealism (Jeans), Logical Realism (Keyser), and Creative Evolution (Bergson). They must be considered as a very limited selection from a wide range of metaphysical positions claiming to be founded on science.

CHARACTERISTIC FEATURES OF SPECULATIVE PROBLEMS

An examination of the typical solutions to these three main speculative problems discloses certain features which may be considered as characteristic of them. In the first place, they are not such as to require solution *for science itself;* hence they are pursued by the investigator not as scientist but as philosopher. This does not, of course, make the pursuit of the problems unimportant. Sir James Jeans says, for example, with reference to the general speculative problem of the nature of reality, "I believe, in common with most scientific workers, that without a background of this kind we can neither see our new knowledge as a consistent whole, nor appreciate its significance to the full." [1] Eddington insists that such excursions into extra-scientific territory afford the scientist a better view of his own domain.[2] But the search for the view of the whole, and the appreciation of significance, are not strictly part of science in the narrow sense. Accordingly, one may say that these problems are of interest not to the critical philosopher who is concerned with the foundations of science, but to the speculative philosopher interested in the larger issues of life. As

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[1] *New Background of Science* (New York: Macmillan, 1933), p. vii.

[2] *Nature of the Physical World,* p. vi.

such, problems of this type are distinguished from questions of pure method, and from questions of the meanings of the basic concepts, for in both of these latter cases there is reason to believe that the philosophical problems are in some very intimate way tied up with the scientific problems, and hence to be looked upon as their logical development. A good scientist must be a logician and a critical philosopher. But a good scientist need not be a speculative philosopher. Many, in fact, would insist that a scientist who pursues speculative enterprises ceases thereby to be a scientist, for he abandons critical methods. This may be true, but at least the scientist is under no obligation to occupy himself with these problems so far as the strict demands of his science are concerned. It is still true, as Laplace remarked to Napoleon, that the astronomer does not need the hypothesis of God. The chemist, as chemist, need not concern himself with the possible social applications of poison gas and high explosives. Nor should the physicist refrain from his investigations until he has succeeded in proving abstractly that he is free to do as he pleases, or that there really is a world outside of himself which is there to be known. Science goes on its happy way independently of these more speculative considerations. They belong, as many would maintain, rather to the frills of science, to be pursued as an entertaining and harmless pastime by those whose minds turn readily to such speculative enterprises.

In the second place, speculative problems seem to depend for their solutions upon the more critical problems of the philosophy of science. Speculative philosophy, in other words, presupposes either the logic or the metaphysics of science. This seems to be due to the fact that the more synoptic problems are founded, properly speaking, not upon science itself but upon certain general features of its method or certain general properties of its concepts. These more philosophical aspects of science must be recognized before the step to speculative considerations is possible. For example, a classification of the sciences is based either on method or on subject matter; in the former case reference must be made to such methodological features as the differences between rational and empirical sciences, experimental and non-experimental sciences, statistical and individual sciences, quantitative and non-quantitative sciences, etc.; in the latter case reference must be made to such metaphysical problems as the classification of the categories, "levels" of reality, dependence and independence of the sciences, etc. The solution to the problem of human freedom presupposes that generalization has already been made as to the existence or non-existence of causal determinations in the inorganic realm. Problems of the nature of reality are based either upon logical considerations pertaining to the character of scientific symbols and the general nature of scientific explanation, or upon metaphysical considerations reflecting the interrelations of the sciences. Thus the solutions to speculative problems approach a higher degree of adequacy to the extent to which they are based upon careful analyses of the more critical problems. The strict nature of this dependence will be brought to light in the illustrations to be given in the following chapters.

In the third place, since the inference in the case of each of these problems is to a realm which is extra-scientific, there is reason to doubt whether the criteria of scientific verifiability are strictly applicable. Just because the inferences extend out into that wider realm in which man reacts morally, religiously, and esthetically, one may not be justified in estimating the results in the cold light of reason. Broad suggests that conclusions in speculative philosophy are almost certain to be influenced by the condition of one's liver and one's bank account. Many of the inferences are clearly little more than rationalizations of beliefs held upon grounds other than those afforded by science. Eddington suggests that we must avoid the "tendency to use 'reality' as a word of magic comfort like the blessed word 'Mesopotamia'";[1] we are continually confusing "reality" with

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[1] *Nature of the Physical World*, p. 327.

"reality (loud cheers)." [1] Many of the conclusions of R. A. Millikan with reference to problems of religion, though they are hardly to be called wishful thinking, are so little based upon the data of science and so significantly founded on a deep, personal religious experience, that they are hardly to be classed as attempts at speculative philosophy in the narrow sense in which the term is here being used. The scientist who becomes enraptured by the beauty of a deductive scheme, who marvels at the grandeur of the heavens, who wonders at the complexity of life—and concludes therefrom a Spirit behind nature, is not making a judgment of fact based upon the data of science but a judgment of value based upon an esthetic experience. But if it is the emotional man who is speaking, is one justified in using the abstract, rational man as the judge of what is being said?

It seems important to recognize, therefore, in the fourth place, that speculative problems almost always involve implicit recognition of data which are other than those offered by science. Eddington cannot conclude anything as to the existence of a Spirit behind nature from the facts of science alone, for scientific symbols are absolutely incapable, on his own grounds, of providing such information; it is rather the mystic experience and the introspective experience which justify the inference. Bergson concludes to the mobile and enduring character of reality not through science, for science can reveal only the static and discontinuous, but through intuition. Compton talks about problems of religion not as a scientist but as a man attempting to guide his own individual life.[2] It often turns out, therefore, that the essential inference to the extra-scientific domain is based upon data which are also extra-scientific, and the speculative problem then reduces to one of showing that the existence of such a domain is *compatible* with the data of science. The inference is, consequently, not from science to philosophy, but entirely within philosophy (or, at least, entirely outside of science). The result is that the

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[1] *Ibid.*, p. 287.

[2] *The Freedom of Man*, pp. ix-x.

problem reduces to an attempt to show that nothing in science is incompatible with the conclusion thus established. Eddington, for example, expresses himself as opposed to any attempt to base religion on scientific discovery; all that can be concluded is "that the recent changes of scientific thought remove some of the obstacles to a reconciliation of religion with science."[1] This destroys, definitely, the intimacy of the relation between science and the larger problems, and suggests that perhaps no inference from the one to the other has any measure of justification.

For this reason, finally, one should recognize the precarious and conjectural character of the solutions to the speculative problems. They are not certainties, nor are they even very high probabilities—if one attempts to measure them according to scientific criteria. There is reason to doubt whether they ought even to be called hypotheses. Logical positivism has denied that problems of speculative metaphysics are philosophical problems; they are either pseudo-problems, indistinguishable from poetry, producing emotional responses but not asserting anything, or else problems lying within the fields of the sciences, and hence scientific problems.[2] This probably represents an attitude as extreme as its opposite, which insists that problems of this kind can be answered with a definiteness and finality which negates the possibility of debate. Both positions are probably wrong. It seems likely that speculative problems are genuine problems, though their terms are extremely obscure as to meaning, and the evidence on which they are based very elusive. The fact that such problems are being linked up with science, if not actually based upon scientific data, argues both for the fact that they are meaningful and for the fact that there are data relevant to their solutions. These are important conditions for increasing the probability that one solution rather than another is correct in each case.

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[1] *Science and the Unseen World* (London: Allen and Unwin, 1930), p. 45.

[2] See above, pp. 11–12.

CHAPTER XVIII

THE CLASSIFICATION OF THE SCIENCES

It was pointed out in the preceding chapter that the problem of the classification of the sciences occupies a peculiar position among the speculative problems. Although it is itself one of these problems, it is nevertheless synoptic in character and thus, in a sense, determines the limits and interrelations of all of the problems. As will be illustrated by Peirce, a properly constructed classification of the sciences must contain as one type of legitimate discipline a study whose task is precisely the construction of such a classificatory scheme. If the making of surveys and reviews is a legitimate enterprise, it must have its place in a general survey and review of legitimate enterprises.

GENERAL CHARACTER OF THE PROBLEM

The designation of the problem as the classification of the *sciences* is somewhat misleading. Though there is such a problem, it is obviously only a small part of a more extensive problem of the same kind which is the classification of human intellectual disciplines. The scientific method is not the only way of knowing, and lines of demarcation between science and philosophy, science and history, science and technology, science and art, etc., are far from clear. It is almost impossible to say, for example, where mathematics, which is quite obviously a science, passes into logic, which is just as obviously a philosophical discipline; it is difficult to discern the line separating anthropology as a science from anthropology as a historical study; it is hard to say whether medicine is a pure or an applied science; it is absurd even to attempt to state at what point a dissertation on a scientific topic ceases to be a treatise on science and becomes an example of literary art. Hence the problem of the classification of the sciences, properly conceived, is rather a problem of the organization of knowledge.[1] It is defined in Baldwin's *Dictionary of Philosophy* [2] as "the systematic arrangement of the various branches of knowledge or of positive science, in order to fix their definitions, determine their boundaries, bring to light their interrelations, and ascertain how much of the task of science has been accomplished and what remains to be done." Accordingly, it includes not only what are ordinarily called the sciences, but also the philosophical disciplines, historical studies, trades and technologies, and even religion and the fine arts in so far as these presume to assert facts and convey messages.

With the problem thus conceived, a question arises as to what can be the possible motive for carrying on speculations of so extensive and conjectural a character. As is common to all speculative problems, there seems to be nothing within the special disciplines themselves that demands such considerations. It is rather the encyclopedic or synoptic interest of the human mind that appears to be the determining factor. Organizations and systems are always to be preferred to mere aggregates and collections. Sometimes, to be sure, more specialized interests prevail. The individual pursuits must be placed within a general scheme if one is to avoid conflicts and hostilities, if one is to organize tasks upon coöperative lines, and if one is to avoid duplication of labor. But a more basic interest is commonly to be discerned in the long list of historical attempts to wrestle with this problem. The speculative philosopher seems to feel that a classification of intellectual disciplines somehow reveals important information about reality itself. The various ways in which one talks about the world at least indicate the various kinds of things which may be talked about. "The system of the sciences is broadly a synthesis of concepts and conceptual relations, believed to be correlative to objective realities and

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[1] This is the title of a book by H. E. Bliss, to which more specific reference will be made later in the chapter. It is probably one of the most satisfactory and comprehensive treatments of the problem in contemporary literature.

[2] New York: Macmillan, 1901, p. 188.

real relations. Conversely, the systems of nature are discovered and synthesized by science, and the real universe is revealed, though imperfectly, to human intelligence. The system of verified knowledge progressively reveals reality. The intellect aspires ultimately to comprehend the universe. This intellectual tendency is in some minds consciously purposive. In religious minds the purpose is regarded as related to a universal purpose that is causative, or teleological, creative or divine." [1]

It is not likely that a problem of this scope will receive a solution which is anything like final. The complete data are all of the facts of method and subject matter as related to the various disciplines, and are obviously too extensive for a single mind to grasp. No man can know all things. Accordingly, there must be a generous reliance upon the method of authority and other indirect methods. One is entitled, for example, to rely more or less completely upon the specialized investigator for information pertaining to his particular field. This method has its obvious disadvantages. The specialized investigator is seldom in a position to see his field in perspective; the man who is in love cannot analyze love, the man who is immersed in the religious experience cannot study religion, and the physicist cannot say clearly what physics is. But this is the only method by which the philosopher can hope to comprehend the great mass of necessary data. As an alternative to this procedure he must *infer* as to the character of the specialized disciplines from what the individual investigators say and do—a method which is open to dangers that are equally obvious. But even after the philosopher has been provided with the necessary data, many of which are conflicting among themselves, he is confronted with the precarious task of assigning the special studies to their places in the total scheme, a task founded upon estimates of importance and unimportance, generality and speciality, dependence and independence, all of which are almost certain to be reflections of an individual value outlook.

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[1] H. E. Bliss, *The Organization of Knowledge* (New York: Holt, 1929), p. 409.

The most convenient form for the representation of such a synoptic view is a logical scheme of genus-species relations like the following:

$$\text{A}\begin{cases}\text{AK}\begin{cases}\text{AKX}\\ \text{AKY}\end{cases}\\ \text{AL}\\ \text{AM}\begin{cases}\text{AMR}\\ \text{AMS}\\ \text{AMT}\end{cases}\end{cases}$$

This affords a general structure within which the various disciplines may be located as superordinates, subordinates, or coördinates. A table of this kind should provide a place for highly general studies, such as science, philosophy, and history; for the more specific disciplines, such as physics and chemistry; and for the highly specialized investigations, such as optics, acoustics, and electricity. The principle of division need not be that of dichotomy, nor of trichotomy, but will be determined, in each case, by the nature of the subject matter. It would seem advisable to introduce the principle of serial arrangement as well as mere classification wherever the material permits, i.e., if the species of a given genus exhibit ordinal properties they should take on a form which clearly indicates this fact. The different levels of subordination may be indicated in a tabular form either by means of columns or by means of rows. Where cross-classification exists, i.e., where a species lies in two different genera, this may often be represented by a two-dimensional pattern; for example, the two divisions of biology into physiology and morphology and into botany and zoölogy might be indicated as follows:

$$\overbrace{\text{Botany}\qquad\qquad\text{Zoölogy}}^{\text{Biology}\ \begin{cases}\text{Physiology}\\ \text{Morphology}\end{cases}}$$

A table in which this principle is employed, however, takes on a high degree of complexity, and loses its schematic form. No convenient tabular representation is possible when the dimensional variations exceed two. Elaborate geometrical figures consisting of intersecting triangles, circles, etc.,[1] which aim to indicate the highly complicated relations between the various disciplines may prove helpful, though they are definitely limited in scope. It seems better to omit some of the detail, even at the expense of accuracy, in order that the more significant relations may be clearly indicated.

DIFFICULTIES IN THE CLASSIFICATION OF THE SCIENCES

Though the difficulties experienced in constructing a classification of the intellectual disciplines are not insurmountable, they are great and a frank consideration of some of the most important of them is a valuable preliminary to the examination of the actual problem. In many cases they must be met by arbitrary acts, as in the classification of mathematics under the form of an independent discipline in spite of its obvious use in most of the sciences; in other cases the obstacles must be overcome by admitting defeat, as in the mere attempt to list the various trades, occupations, and avocations; in still other cases the difficulties must be met by open compromise, as in the attempt to make any classificatory scheme represent all of the ramifications of natural relations. Some of the most obvious of the difficulties will be considered in the following paragraphs.

The most superficial examination of the problem convinces one that any classification must be highly schematic in character. It seems clear that no such table can represent all of the interrelations which nature itself exhibits. In fact, it is questionable whether one would wish a classification of the sciences to do this; if a map of nature were as detailed as nature itself, it would lose most of its advantages. The construction of tabular schemes proceeds, as does all science, by the method of abstraction; the attempt is made not to mirror the complexity of nature but to simplify it by selecting only those events and relations which seem important. Every science bears to other sciences relations which are essential and relations which are unessential. The decision

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[1] Such as those given by Bliss, *op. cit.*, pp. 402–403.

between these two types of relations is, of course, not easy to make; hence an element of arbitrariness always enters. But, granting that it has been correctly made, the neglect of the unessential is presumed not to introduce any important element of artificiality into the tabular representation. The point which is here emphasized may be well illustrated with reference to mathematics. This science has relations to almost all of the other sciences, but the relations are so various in character that no conceptual scheme could adequately represent them. By virtue of its deductive structure and its concern with highly abstract notions, mathematics is closely affiliated with logic; by virtue of its application as a technique of measurement, it is connected with physics; by virtue of its non-experimental character, it tends to be grouped with the sociological sciences; and by virtue of the "mental" and idealized character of its subject matter, it becomes closely associated with the psychological sciences. The problem in this connection has to do not merely with representing the fact of the various relations, which could be easily indicated merely by the drawing of lines, but with representing the fact that each of these relations is different in kind from the others and would therefore require a different type of connecting link. As soon as the classificatory scheme adapts itself to these highly complicated and involved elements, its diagrammatic character is destroyed. Hence it seems preferable to neglect all but one of these relationships, even at the risk of introducing artificiality into the representation. A classificatory scheme must reveal nature in the same way that a well-fitting garment suggests the underlying figure, not by portraying clearly all details of its form but by neglecting or hiding some of its features, and emphasizing and exhibiting others. A table of the sciences represents a compromise between an ideal form and the gross complexities of nature.

As a consequence, one must be continually on his guard against constructing a scheme which is in accord with an *a priori* philosophical point of view rather than with the existent sciences. Peirce condemns certain classifications on these grounds. "Many of these schemes introduce sciences which nobody ever heard of; so that they seem to aim at classifying, not actually existent sciences, but possible sciences." [1] He says of his own scheme, on the other hand: "This classification which aims to base itself on the principal affinities of the objects classified, is concerned not with all possible sciences, not with so many branches of knowledge, but with sciences in their present condition, as so many businesses of groups of living men." [2] Both the Kantian and the Hegelian schemes strike one as attempts to force the existent sciences into preconceived forms. Equally artificial are the classifications which legislate for the future. The problem seems to be essentially an empirical one, and demands the techniques which are adapted to this type of problem.

It follows, however, that a classificatory scheme is relevant to the historical scene out of which it emerges, and its adequacy should be measured in these terms. "If Plato's classification was satisfactory in his day, it cannot be good today; and if it be good now, the inference will be that it was bad when he proposed it." [3] The development of thought exhibits both the origination of new disciplines, and the disappearance of pseudo-disciplines. One should hardly expect the medieval scheme to have a place for psychology and anthropology, and one should rightly condemn any modern scheme which included alchemy and astrology on the same level as chemistry and astronomy. There is reason to believe that the future will witness the combination of disciplines now thought to be independent, and the separation of disciplines now thought to be united. It seems inevitable that there should be a continued emergence of interconnecting sciences, such as physiological psychology and bio-physics, and a progressive specialization such as has already given rise to quantum physics, parasitology, and ophthalmology.

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[1] *Collected Works,* Vol. I, par. 1.203. [2] *Ibid.*, par. 1.180. [3] *Ibid.*, par. 1.203.

A difficulty which is somewhat more specific arises in connection with the attempt to locate the sciences which are themselves synoptic in character. One of the best examples of these is geography. This science is obviously composed of material drawn from geology, biology, and sociology, to mention only the most important. The question may be argued as to whether it contains any unique subject matter, or is merely a study of the interrelations of these fields. In either event it cannot be placed on the same level as the other sciences, for the result would be a cross-classification. The difficulty may be avoided by making an initial distinction between the special sciences and the synoptic, or complex, sciences, but this only creates a new problem, for every science except the most highly specialized may be considered as composite with reference to its parts. For example, physics may be considered as composite relatively to optics, acoustics, electro-dynamics, etc. Similar difficulties arise in the attempt to place disciplines which cut across the more basic lines of demarcation, e.g., in the location of mathematical logic, which is both science and philosophy, and in the placing of geology, which is both science and history.

Even more serious are the obstacles which are encountered in the form of disciplines about disciplines. Recent thought has compelled the recognition of such studies as the history of science, the philosophy of science, and the history of philosophy. The location of such pursuits creates obvious difficulties. The situation is not helped any by the appearance of such disciplines as the history of history, and even the history of the philosophy of science and the philosophy of the history of science. These are apparently legitimate enterprises, each of which is acquiring a literature and defining its problems. It is part of the task of the classification of the sciences to assign each of them to its proper place.

A difficulty to which Bliss calls attention is important since it injects an element of arbitrariness into the problem.[1]

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[1] *Organization of Knowledge*, Chap. XII.

Sciences may be arranged in alternative orders, and the choice of order is essentially a matter of purpose and interest. Bliss distinguishes five orders: the order of nature, the developmental order, the pedagogic order, the logical order, and gradation by speciality. There is reason to believe that this classification of orders could itself be improved upon, but this is not the concern at the moment. The point is that those sciences which are most basic from the logical point of view—in the sense that all of the sciences depend upon them—are not necessarily those which emerged first in the history of thought, nor are they those studies which should be taught to the child in the elementary stages of his education; that which is logically first need not be temporally first either for the race or for the individual. Furthermore, the ways in which the sciences are used are not necessarily a reflection of their inherent characters, hence a classification of the arts and technologies need not parallel a classification of the theoretical sciences. It follows that a library classification of books about nature—which is Bliss's main problem—need not reflect accurately the structure of nature itself, though it should clearly conform as closely as this is possible. The agreement which Bliss finds among the four alternative orders is somewhat surprising. There is reason to believe that there should be an essential conformity so far as the order of nature, the logical order, and the gradation by speciality are concerned. But there seems to be no reason why this order should agree either with the pedagogic or the developmental orders. Neither man nor the race grows logically. Hence the speculative philosopher must recognize the possibility of alternative schemes, and the relativity of any given scheme to the intention of the classifier. This makes classificatory schemes arbitrary but not whimsical.

Finally, certain terminological difficulties may be mentioned. The fact that the term "natural philosophy" has often been employed historically for "physics" does not mean that physics should be classified as philosophy rather than as a science. The same consideration applies to the use of "mental philosophy" for "psychology." The older term "political economy" has been quite generally supplanted by the term "economics." The use of suffixes such as "-ology," "-osophy," and "-ography" cannot be taken as infallible indications of the characters of the disciplines to which they are applied. Nor does it assure one of the legitimacy of the studies, as witnessed by astrology and theosophy. Disagreements among authorities as to the proper scope of a given term are not uncommon, and they create difficulties of classification. For example, epistemology is sometimes included in metaphysics and sometimes in logic; social studies such as sociology and economics are sometimes called sciences and sometimes not; and the inclusion of ethics and esthetics in most colleges and universities in departments of philosophy does not make them philosophical disciplines. Many disciplines which are ordinarily called sciences, such as archaeology, paleontology, epigraphy, and heraldry, are just as significantly to be classed as histories. All of these facts suggest that the construction of a classificatory scheme must be preceded by accurate definitions of the main disciplines; ambiguity and vagueness in terminology is no less a vice in the speculative problem than it is in the problems of the special sciences.

HISTORICAL ILLUSTRATIONS

History abounds in examples of classificatory schemes. Most philosophers and many scientists and literary men have felt the urge to solve this synoptic problem.[1] Many of the historical instances, while no longer entirely adequate to the modern scene, exhibit important lines of cleavage and linkage. The division into ethics, physics, and dialectics, usually attributed to Plato, suggests the parallel division into problems of human life, problems of nature, and prob-

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[1] The availability of several good books devoted to the history of this problem obviates the necessity for a more detailed consideration at this point. See H. E. Bliss, *op. cit.;* E. C. Richardson, *Classifications, Theoretical and Practical* (New York: Scribners, 1901); R. Flint, *Philosophy as Scientia Scientiarum* (London: Blackwood, 1904).

lems of knowledge—a classification which would be not without value today. Aristotle's threefold division into the theoretical, the practical, and the productive has much to commend it. Though the medieval division into the trivium (grammar, logic, rhetoric) and the quadrivium (geometry, arithmetic, astronomy, music) seems somewhat artificial today, it throws a certain light on the possible affiliations of these studies. Bacon's division into history, poesy, and philosophy, based on the three fundamental "faculties" in man—memory, imagination, and reason—would no longer be accepted in view of the general abandonment of the faculty psychology. The classification of Hobbes, which divided studies into those dealing with fact (history) and those dealing with relations between antecedents and consequents (science), offers a suggestive manner of distinguishing between these main types of discipline. Locke's division into physics (knowledge of things), practics (skill of right action), and semeiotics (doctrine of signs) is important in its recognition that the study of symbols is a legitimate and important enterprise. Comte's hierarchy consisting of mathematics, astronomy, physics, chemistry, biology, and sociology is important because of the serial principles employed in its construction; the arrangement exhibits a progressive decrease in generality and an increase in complexity and dependence. The classification of Bliss, which will be examined in a moment, is based upon it.

For the purposes of more detailed examination, two classificatory schemes have been selected from contemporary writings. One is that of the philosopher, C. S. Peirce, and the other is that of the librarian, H. E. Bliss. These have been selected from a host of candidates for several reasons. The two men are drawn from the contemporary scene, and one may therefore examine their products in the light of the existent sciences. They include in their classificatory schemes not merely the sciences in the narrower sense, but philosophy, history, and the practical arts as well. They use the characteristic modes of classification—the one (Peirce) employing a form which is essentially that of logical subordination and not obviously one of serial arrangement, the other (Bliss) employing a form which is predominantly ordinal. Though Peirce is motivated by the philosophical or theoretical interest, and Bliss is concerned with the practical desire to construct an adequate classificatory scheme for books, nevertheless Bliss is convinced that such a scheme will be adequate to the extent to which it corresponds closely with the order of nature; hence the problem is similarly conceived by both writers.

PEIRCE

Peirce's classification is predominantly trichotomic, most of the studies being divisible into three species according to a repeating plan, "the First of the three members relating to universal elements or laws, the Second arranging classes of forms and seeking to bring them under universal laws, the Third going into the utmost detail, describing individual phenomena and endeavoring to explain them." [1] This type of division can be illustrated by psychical science, which is subdivided into nomological psychics or psychology, classificatory psychics or ethnology, and descriptive psychics or history. "Nomological psychics discovers the general elements and laws of mental phenomena. . . . Classificatory psychics classifies the products of mind and endeavors to explain them on psychological principles. . . . Descriptive psychics endeavors in the first place to describe individual manifestations of mind, whether they be permanent works or actions; and to that task it joins that of endeavoring to explain them on the principles of psychology and ethnology." [2]

All studies are either sciences of discovery, sciences of review, or practical sciences. Elsewhere [3] he groups the first two under the general heading, theoretical science. The science of discovery consists of mathematics, philosophy, and idioscopy (the special sciences). The subdivisions of

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[1] *Collected Works*, Vol. I, par. 1.180. [2] *Ibid.*, par. 1.189. [3] *Ibid.*, par. 1.239.

these general disciplines will be clearly evident from the following table:

PEIRCE'S CLASSIFICATION[1]

- Science of Description
  - Mathematics
    - Mathematics of Logic
    - Mathematics of Discrete Series
    - Mathematics of Continua
  - Philosophy
    - Phenomenology
    - Normative Sc.
      - Ethics
      - Esthetics
      - Logic
        - Spec. Grammar
        - Critic
        - Methodeutic
    - Metaphysics
      - Ontology
      - Religious Metaphysics
      - Physical Metaphysics
  - Idioscopy
    - Physical Sc.
      - Genl.
        - Molar
        - Molecular
        - Ethereal
      - Class.
        - Crystallography
        - Chemistry
        - Biology
      - Desc.
        - Geognosy
        - Astronomy
    - Psychical Sc.
      - Genl.
        - Introspective
        - Experimental
        - Physiological
        - Child
      - Class.
        - Special
        - Linguistic
        - Ethnology
      - Desc.
        - History Proper
        - Biography
        - Criticism
- Science of Review
  - e.g., Comte's *Philosophie positive*, Spencer's *Synthetic Philosophy*
- Practical Science
  - e.g., Pedagogics, Gold-beating, Etiquette, Pigeon-fancying, Surveying, Navigation, etc.

Peirce asserts that "by 'science of review' is meant the business of those who occupy themselves with arranging

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[1] Constructed from *Collected Works*, Vol. I, Bk. II, Chap. I.

the results of discovery, beginning with digests, and going on to the endeavor to form a philosophy of science. Such is the nature of Humboldt's *Cosmos*, of Comte's *Philosophie positive*, and of Spencer's *Synthetic Philosophy*. The classification of the sciences belongs to this department." [1] Hence Peirce might himself label his classification of the sciences by calling it an instance of a science of review. Peirce does not tell us what he means by his third great division—practical science. In this, as in the case of sciences of review, he offers not a classification but illustrations. He finds himself somewhat bewildered by the motley crowd of practical sciences, but comforts himself in the belief that the classification of this branch is of no logical importance.

BLISS

The basic principle which Bliss employs is that of gradation by speciality. "It may be comprehensively defined as *the principle by which the several sciences and studies, distinguished by their conceptual scope and their relations to the real order of nature, are arranged in serial order from the most general to the most special*. . . . The generalizations and laws of each more general science are true in some measure of all the more special sciences. . . . But the laws or truths of the more special sciences rarely apply to the more general sciences or solve their problems. The special sciences, however, supply materials to which the generalizations and laws of the more general sciences are generally applicable, and by which they may be verified. Here are involved the principles of the *dependence* of the special sciences on the general, the *interdependence* of the several sciences, general and special, and *filiation* of the more special sciences in successive *derivation* from and dependence upon the more general." [2]

Combined with the principle of gradation by speciality, however, there is a further principle. It permits the fourfold division of studies into philosophy, science, history, and

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[1] *Ibid.*, par. 1.182.

[2] H. E. Bliss, *op. cit.*, pp. 217–218.

applied science. Each of these constitutes a class of disciplines within which there is gradation by speciality. Furthermore, the gradations in the four fields parallel one another, with certain minor exceptions. Thus the scheme of the intellectual disciplines is two-dimensional, the vertical dimension representing progressive specialization as one passes from the top of the column to the bottom, and the horizontal dimension representing parallel philosophies, sciences, histories, and applied sciences, as one passes from the left to the right.

The four main divisions are described as follows: "Science is verified and organized knowledge, rationally and methodically proceeding from empirical and experimental data, simple concepts, and perceptual relations to generalizations, theories, laws, principles, and explications, and to more comprehensive conceptions and conceptual systems." [1] "Abstract conceptions that by rational processes are reared too remote from empirical bases, with regard rather to ethical, religious, and esthetic implications, Science assigns to Philosophy. . . . True philosophy, however, as a superstructure resting on the foundations of common knowledge and science, proceeds to more abstract conceptions, more transcendent relations, and more metaphysical implications." [2] "By generalization science is more positively distinguished from *history* than it is from philosophy. History for the most part deals with concrete, or discrete, objects and individuals, with particular events, parallels, movements, tendencies, and developments, as antecedents and consequents, though it also considers the determinative, or *causal*, relations, or 'forces.'" [3] Finally, as to applied sciences, "there are certain fields of study or research that are cultivated more in solving practical, technical, or economic problems, are more concerned in applying known data and principles by means of known methods, or methods readily derived from those known, than they are in discovering new data, relations, and principles. . . . It is these sciences

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[1] *Ibid.*, p. 190.

[2] *Ibid.*, p. 193.

[3] *Ibid.*, p. 195.

and studies that may provisionally and for convenience be termed *applied*." [1]

The construction of the table proceeds, then, with the arrangement of the philosophies, sciences, histories, and applied sciences in parallel columns, each exhibiting gradation by speciality. The column of the sciences is taken as the pattern. "Our point of view being dominantly that of natural science, the main divisions of our system are likewise consistent with the fundamental sciences as graded by speciality, and the correlative, or parallel, branches of philosophy and of history are there accordingly placed under the respective main divisions, and so also are the more scientific branches of technology. This is the schematic statement for the most comprehensive and fundamental structure of our system." [2]

The tabular arrangement which Bliss proposes is as follows:

BLISS'S CLASSIFICATION [3]

Philosophy	Science	History	Technologies and Arts
Principles	Science in general	History of Philosophy and Science	
Metaphysics			
Abstract Sciences and general methods			
Logic			
Mathematics			
Philos. of Nature	*Natural Sciences*		Applied Science
	Physical Sciences		Technology:
	Physics		Physical; Engineering;
	Chemistry		Chemical
	Special Natural Sciences and descriptive Natural History		
Cosmology	Astronomy		
	Geology	Historical Geology	Economic Geology Economic Geography
	Geography		
	Meteorology		

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[1] *Ibid.*, p. 213.

[2] *Ibid.*, p. 252.

[3] H. E. Bliss, *A System of Bibliographic Classification* (New York: H. W. Wilson, 1935), p. 75.

PHILOSOPHY	SCIENCE	HISTORY	TECHNOLOGIES AND ARTS
Philos. of Life	*Biological Sciences*		
	Biology		
	Botany		
	Zoölogy		
Philos. of Human Life	*Anthropological Sciences*		Humanities and Humanitarian Studies
	Anthropology	History of Mankind and of Human Life	
	Physical		Medical Sciences and Hygiene
		Archeology	
	Psychological Sciences		
Mental Philos.	Psychology		Applied Psychology
	General and Comparative		
	Individual Psychology		
	Abnormal Psychology		Psychiatry
Social Philos.	Social Psychology		Education
	Social Sciences		
	Sociology	Social-political History	Applied Social Science
	Ethnology		
Theology	Religion	History of Religions	Church-work
		Mythology	
Ethics	Morals		Applied Ethics
Political Philos.	Political Science		Government
Philos. of Law	Jurisprudence	History of Law	Practice of Law
Philos. of Econ.	Economics	Economic History	Economies
			Industries
			Business
			Arts:
Aesthetics		History of Arts:	Technic of Arts:
			Industrial;
			Fine Arts, etc.
			Music
	Philology		
	Linguistics and Languages;		Rhetoric, Oratory, etc.
	Literature		Drama and Theatre

GENERAL COMPARISONS

Although the two classifications given exhibit apparent discrepancies, they are not altogether unlike so far as underlying structure is concerned. Peirce's threefold division into nomological, classificatory, and descriptive sciences is essentially identical with Bliss's division into philosophy, science, and history; for the first group represents the study of the most general laws without any direct reference to specific phenomena, the second represents the study of laws as applied to the phenomena, and the third represents the study of the individual phenomena. Furthermore, Peirce, in making the trichotomic division a repeating form, accomplishes essentially the same end as Bliss does in his use of columns, though he does not succeed so clearly in representing the parallelism of the main disciplines. Though Peirce's scheme is not obviously ordinal it exhibits serial properties by virtue of the repetition of the trichotomic divisions. For the nomological divisions of nomological studies are the most definitely nomological, and the descriptive divisions of descriptive studies are the most definitely descriptive; hence the former head the series as the most abstract of the studies, and the latter terminate the series as the most concrete. The parallelism of mathematics near the top, and of literature, biography, and criticism near the bottom in the two tables cannot easily escape notice. The Peircean table has the advantage of affording a definite place for synoptic sciences, though the author's failure to introduce classification at this point prevents one from knowing whether this division is limited to the all-inclusive synoptic studies which are usually called philosophies, or extends also to the less inclusive sciences, such as anthropology and geography. The parallel representation of the practical sciences, as in the table of Bliss, is to be preferred to the plan of Peirce, for it seems likely that they are reflections of corresponding theoretical studies, and hence may be tabulated by virtue of the latter.

Both classificatory schemes are defective in the representation of disciplines about disciplines. No doubt studies of this character are puzzling, for they require that the scheme should be reëntrant, e.g., if there are not only philosophies and sciences but philosophies of science, it follows that science must occupy a double place in the table—it must be both a type of study and a subject matter. An adequate classificatory scheme must indicate, therefore, that philosophy may study science in essentially the same way that science studies nature. Similar difficulties arise in connection with such studies as the history of philosophy and the history of history. One way of meeting this difficulty is to make the classificatory scheme perfectly general, i.e., to formulate it not in terms of science, or philosophy, or history but in terms of *studies*, meaning by this concept an abstract pursuit which may be interpreted according to any one of the three disciplines. Then *studies of studies* may be considered as indicating the history of philosophy, or the philosophy of science, or the history of history, according to the interpretation desired. This representation complicates the table greatly, but it results in a more adequate scheme.[1]

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[1] The author has attempted to solve the problem along these lines in his book mentioned in the bibliography given immediately above.

CHAPTER XIX

HUMAN FREEDOM

The problem of human freedom labors under two important difficulties. In the first place, it is, perhaps, the most pressing of the speculative problems. Many have felt that both morality and religion must disappear unless it can be solved—solved, that is, in such a way as to make human freedom possible. For this reason most speculation on the problem suffers from an initial partiality. Both because of the urgency of the solution and because of the odds in favor of one solution rather than another, reflection on the problem has been tinged with emotion. Fallacies of *non sequitur* and begging the question abound. Frequently the conclusion is drawn from an implicit premise, such as the unbearable character of a world in which strict determinism prevails, or the futility of striving after the Good in a world which is governed by Fate.

But, in the second place, the problem is—perhaps because of the emotional associations—inaccurately formulated. The essential terms employed in its definition and solution —"freedom," "determinism," "volition," "self," and "self-determination"—are vague and ambiguous, and little effort is expended toward their clarification. As a result, one cannot always tell whether what is demonstrated on the grounds of science is freedom in the sense of absolute determinism or in the sense of self-determinism; nor can one ascertain whether it is man's behavior or man's volition which is proved to be free, or whether freedom consists in the complete absence of causes or merely in the unpredictability of behavior. Terminological difficulties of this kind make it possible for Planck and Compton both to demonstrate human freedom, though the former considers nature to be causally determined and the latter believes that the

Heisenberg principle argues for breaks in causal determination; for what Planck demonstrates is freedom in the sense of unpredictability, and what Compton demonstrates is freedom in the sense of the absence of physical determination.

The problem is not, of course, in its broadest sense, necessarily solved on the grounds of data offered by science alone. Presumably there may be data drawn from a wide range of experience which would be relevant to it. But, considered as a problem of speculative philosophy, it is limited for its data to material offered by the subject matter or method of science. Science, in its very fundamentals, is built around the problem of the ascertainment of uniformities. Whether the regularities are presumed to lie in nature itself, or whether they are supposed to be merely techniques of explanation is unimportant at this point; in either case they are the *sine qua non* of science. For science attempts to assert generalizations, and generalizations are impossible without uniformities. Uniformities imply repeated connections and the consequent absence of spontaneity. Thus the success of science in its search for regularities is a measure of the existence of regularities, or, at least, of the availability of principles of regularity as techniques of explanation. But man is obviously a part of nature. Whether he is merely a part and therefore manifests the characteristic features of the rest of nature, or an exceptional part and therefore exhibits properties not common to nature as a whole—these are questions which must be answered. But the legitimacy of inferring the nature of human behavior from the behavior of events in general seems never to have been called into question.

It is interesting to note that the attitude on the question of human freedom or human determination has been throughout the course of history an accurate barometer of the status of science in any given period. Ages in which science has been quiescent have usually been periods in which man forgot the regularity and uniformity of natural processes and conceived of himself as master of his fate. On the other hand, ages in which science has been active have been periods in which man became conscious of the lawfulness of nature and of his consequent ineffectiveness. This parallelism suggests that there is some connection between the two facts, at least for the popular mind. The absence of law is associated with human freedom, and the presence of law is associated with human determination.

The best historical illustration of this is to be found in the contrast between the world-view which was characteristic of the Middle Ages—a period which according to all records was especially deficient in scientific achievements—and the scientific world-view which followed it. "For the Middle Ages man was in every sense the centre of the universe. The whole world of nature was believed to be teleologically subordinate to him and to his eternal destiny. . . . The prevailing world-view of the period was marked by a deep and persistent assurance that man, with his hopes and ideals, was the all-important, even controlling fact in the universe. . . . An explanation in terms of the relation of things to human purpose was accounted just as real as and often more important than an explanation in terms of efficient causality, which expressed their relations to each other. Rain fell because it nourished man's crops as truly as because it was expelled from the clouds. Analogies drawn from purposive activities were freely used. . . . The whole universe was a small, finite place, and it was man's place. He occupied the centre; his good was the controlling end of the natural creation." [1]

Sharply contrasted with this was the world-view which developed with the advent of science and its discovery of the mechanical character of natural processes. "Just as it was thoroughly natural for medieval thinkers to view nature as subservient to man's knowledge, purpose, and destiny; so now it has become natural to view her as existing and

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[1] E. A. Burtt, *Metaphysical Foundations of Modern Physical Science* (New York: Harcourt, Brace, 1925), pp. 4–6.

operating in her own self-contained independence, and so far as man's ultimate relation to her is clear at all, to consider his knowledge and purpose somehow produced by her, and his destiny wholly dependent on her." [1] Nature is no longer teleological but causal. Man, instead of being the controlling factor in the universe, does not even have control over his own destiny; he is the innocent victim of blind forces operating according to mechanical principles. Rain falls not only to nourish his crops but to drench him in tornadoes and floods, and to wipe out his home and family. The universe is infinite, and man occupies a minute spot which is but local and temporary, soon to be eradicated by the irresistible march of unseeing forces. Man cannot be free.

The recent interest in the problem of human freedom is, similarly, a direct reflection of a state of affairs in the physical sciences. However, the situation is somewhat different because the agitation for human freedom which has characterized much of the contemporary literature in the philosophy of science has been the result of a period not of scientific quiescence but of scientific activity. The point is that this activity has itself disclosed within the very field of science a fact which is incompatible with the assumption of a strictly lawful nature. Attention has already been called in Chapter XVI to the formulation by Heisenberg of the principle of indeterminacy. Whether or not this principle argues for an actual break in the causal structure of nature is, as has already been seen, debated by scientists. But even if it does not demonstrate the existence of actual spontaneity, it at least establishes the impossibility of making predictions with regard to certain types of phenomena which had been presumed to be causal in character. As a consequence, the principle has been enthusiastically welcomed by the advocates of human freedom. It is not always maintained by these advocates that the principle actually demonstrates human freedom; but it is commonly asserted that the prin-

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[1] *Ibid.*, pp. 10–11.

ciple at least leaves room for a human freedom which may perhaps be demonstrated on other grounds. At any rate there is no longer any conflict between the assumptions of physical science and the demands of the moral consciousness. Even granting, therefore, that science is entitled to legislate for human life, it cannot destroy human freedom, for something analogous to this is manifest even in the physical world. Nature exhibits loopholes in uniformity; hence, if man is a part of nature he may still be free. This is approximately the state of affairs which has given rise to the recent interest in the problem. The problem itself may now be subjected to analysis before an examination is made of the views of certain contemporary writers.

GENERAL CHARACTER OF THE PROBLEM

What, then, is the problem of human freedom considered in the context of science? It may be formulated somewhat as follows: The complete act of behavior may be analyzed in two ways. On the one hand there is the physical analysis, which breaks up the total act into the movement itself, such as raising the hand, or speaking, the brain-state which is presumably its cause, then the brain-state which is presumably the cause of this, and so on backward in time. The brain is the central organ of control with regard to bodily processes, and is responsible for the instigation of all activity which is not purely reflexive. On the other hand there is the psycho-physical explanation, which breaks up the total act into a similar bodily movement, the brain-state which is presumably its cause, and the volition or mind-state which is the cause of this. This analysis also admits that the brain is the central organ of control and is responsible for the instigation of all activity, but it insists that the brain is only the instrument of the volition. Here, in fact, is the origin of the problem of freedom *versus* determinism. The physical and the psycho-physical explanations are identical with regard to the movement and the brain-state. But the problem lies in the exact status of this brain-state. Is it the effect of an earlier brain-state, or is it the effect of something which is not a brain-state (though it may have a parallel brain-state), viz., the volition or decision?

It seems clear that the deterministic solution to the problem of human behavior will follow the lines laid down by the physical analysis. An act may be said to be determined if there is an anterior state which, being known, would enable one to predict the act. This is at least determinism in the sense in which Laplace supposed the world to be determined. If act, brain-state, anterior brain-state, and so on, are all well-defined bodily states, the causal chain is clearly evident. Like Laplace the determinist would be able to say that if a being of sufficient intelligence knew the condition of every particle in the brain of an individual he could predict all of the individual's future behavior. Determinism of this type which involves no reference to mind or volition is commonly characterized as materialistic, or epiphenomenalistic.

But it seems equally clear that the psycho-physical analysis may also eventuate in a determinism. Determinism need not be materialistic. Even if at some point in the regress from act to brain-state and brain-state to its cause volition enters as a causal factor, there is no reason to abandon determinism. All that is required is the recognition of volition as a legitimate anterior state, knowledge of which would permit prediction of activity. If mind-states are effective, the Laplacian being could predict just as accurately on the basis of such knowledge as he could in the former case on the basis of knowledge of brain-states.

However, for some reason, a determinism of this kind does not appear so distasteful as the materialistic kind. In fact psycho-physical determinism reduces to self-determinism, and this is what is commonly meant by freedom. For if the volition is itself the outcome of a previous mind-state, this of a still anterior one, and so on, what this amounts to in effect is the progressive building up of the personality, or self. I do not object to the supposition that my volition was determined by a number of factors which I call motives, reasons, desires, past experiences, etc.; in fact I insist that my volition is precisely the outcome of the survey of just such factors. But this examination of motives, reasons, etc., is nothing other than the act of making the decision. My decision is not independent of these factors; it is these factors themselves blended into a harmonious whole. Hence when I say that my decision has been determined by a previous mind-state, all that I am asserting is that I myself have determined the decision, for by my self at any moment I mean simply the mind-state at that moment, which includes, of course, the effects of all previous mind-states. But to act according to the dictates of my self is not to be determined but to be free. Self-determinism *is* freedom.

Apparently, therefore, the differences between the physical and the psycho-physical analyses of the act of behavior are two in number: (a) The latter theory permits a physical state to be caused by something which is non-physical, i.e., by a mind-state. (b) The latter theory permits a mind-state to be caused only by something which is non-physical, i.e., another mind-state. The former theory permits neither of these to be true. Both theories are causal, for they both insist that the eventual movement is the outcome of previous events. In fact both theories claim that the movement is the immediate outcome of an event in a brain. The difference arises when the analysis is pushed back further. What is to be done with this brain-state which is the immediate cause of the act? Is it caused by a previous brain-state, and only by such? If so, the act of the individual is part of a physical causal chain and is determined. Is the brain-state caused by a previous mind-state, and only by such? If so, the individual is part of a causal chain which is not wholly physical in character, and this is partly what is meant by saying that the individual is free. He is free in the sense that his behavior follows from his decisions. In this sense the individual is free to act as he wills. But is he free to *will* as he wills, i.e., is the individual's volition itself an event in a causal chain? As has been seen, the fact that volition is the outcome of earlier events does not destroy the freedom of the individual provided the causal chain is kept entirely in the realm of mind-states. Hence the further demand of the concept of freedom is that the volition be nowhere an immediate or indirect effect of a brain-state. There is only a one-way causal action, from mind-state to brain-state, but not in the reverse direction. These two demands of a theory of freedom may be stated as follows: *There is no law which asserts that a volitional state is a function of an earlier physical state. There are laws which assert that physical states are functions of earlier volitional states.*

It is not likely that such an analysis of the problem of freedom and determinism is a fruitful one for the ultimate solution of the problem. Recent philosophy and psychology have become convinced of the dangers inherent in the use of the terms "mind" and "body," and have attempted to avoid them. But the writers whose positions are to be examined in the following pages have used this terminology, and it is for this reason that the words and the dichotomy based upon them have been used. The analysis which is here given may be taken merely as a general matrix to be used for the clarification of certain contemporary views on the problem of human freedom.

EDDINGTON

Eddington has been among the boldest of those who draw speculative conclusions outside of the realm of physics upon the basis of indeterminacy revealed in physical phenomena. Eddington's data are of two kinds. On the one hand are those conclusions of physical science to which reference has already been made in Chapter XVI. The realm of physical entities can no longer be looked upon as a strictly deterministic scheme since small-scale phenomena, by virtue of the principle of indeterminacy, are unpredictable as to behavior. "Strict causality is abandoned in the material world. Our ideas of the controlling laws are in process of reconstruction and it is not possible to predict what kind of form they will ultimately take; but all indications are that strict causality has dropped out permanently. This relieves the former necessity of supposing that mind is subject to deterministic law or alternatively that it can suspend deterministic law in the material world."[1] Thus in the realm of physical phenomena there is no longer any reason for asserting that nature is completely causal in character.

But on the other hand there are important data which are not strictly in the field of physics. These are discerned by an examination of the methods of knowing objects. When one examines the character of our knowledge of physical objects he is impressed by an important fact. We know them not directly but through the medium of symbols—pointer readings. Our knowledge of space, time, motion, force, and so on is merely that of numbers on recording instruments. The inner nature of objects—if they have any inner nature at all—is forever hidden from our awareness. Now one of the physical objects which we know in this way is the human brain. Our knowledge of the brain is such as is obtained by measuring its volume, its duration, motions exhibited in it, its heat, its energy, and so on. But in this "one case—namely, for the pointer readings of my own brain—I have an insight which is not limited to the evidence of the pointer readings. That insight shows that they are attached to a background of consciousness."[2] Introspection therefore comes into the picture, and affords a method for getting at the inner nature of the brain. Thus we can know the inner nature of our own brain in a way in which we cannot know the inner natures of ordinary physical objects. Hence, "there is nothing to prevent the assemblage of atoms constituting a brain from being of itself a thinking object in virtue of that nature which physics leaves undetermined and undeterminable."[3] Introspection reveals something which cannot be revealed by science, and yet cannot be gainsaid by science. The traditional solution to the problem

[1] *Nature of the Physical World*, p. 332.
[2] *Ibid.*, p. 259.
[3] *Ibid.*, p. 260.

of human freedom has been one-sided, since it has recognized the conclusions of science but disregarded the dictates of introspection. The modern solution demands that these data be admitted as of equal significance.

From these premises only one conclusion follows. When I examine my own consciousness I find that its essential feature is volition. "We may now feel quite satisfied that the volition is genuine. The materialist view was that the motions which appear to be caused by our volition are really reflex actions controlled by the material processes in the brain, the act of will being an inessential side phenomenon occurring simultaneously with the physical phenomena. But this assumes that the result of applying physical laws to the brain is fully determined. . . . If the laws of physics are not strictly causal the most that can be said is that the behavior of the conscious brain is one of the possible behaviors of a mechanical brain. Precisely so; and the decision between the possible behaviors is what we call volition. . . . At some brain center the course of behavior of certain atoms or elements of the physical world is directly determined for them by the mental decision—or, one may say, the scientific description of that behavior is the metrical aspect of the decision." [1]

Unfortunately the meaning here is none too clear. Apparently what Eddington has in mind is something like the following: By virtue of the principle of indeterminacy the occurrence of a particle in a given state is unpredictable, i.e., the anterior state is ascertainable only by knowing both position and velocity of the particle—which is impossible. If one argues from this fact to objective indeterminism he concludes that any state of a particle may occur without cause, i.e., without physical cause. Now let us suppose that the particle in question is a brain particle. Any given state of this particle may occur without physical cause, i.e., without anterior brain-state. But introspection is a witness to the fact that a brain particle has an inner nature which

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[1] *Ibid.*, pp. 311–312.

is not measurable by physical techniques. Consequently there is nothing opposed to the supposition that this inner nature which we call volition is effective in the determination of the state of the particle. A particle may then take on a certain state without violation of the laws of physics, yet in response to the dictates of volition. This permits an action of "mind" on "matter" without necessitating any interruption of physical processes, for "mind" enters only when there is an inadequacy in physical explanation.

This seems to guarantee the desired freedom of the will. The will is effective over the brain, yet is not itself affected by physical processes. It has the advantage, therefore, over all materialisms and epiphenomenalisms which either deny the reality of mind or else reduce it to the mere afterglow of brain processes. Yet it does not violate the law of conservation of energy since volition is not an intrusion into the physical realm. Hence on the grounds of scientific data and introspection the freedom of the will is demonstrated.

COMPTON

The position of Compton in this issue is essentially the same as that of Eddington, though its details are more intricately worked out. For Compton the problem is not one of deducing the fact of human freedom from the laws of nature. This places human freedom in a derivative position whereas, in fact, it is fundamental. "It seems unfortunate that some modern philosopher has not forcibly called attention to the fact that one's ability to move his hand at will is much more directly and certainly known than are even the well-tested laws of Newton, and that if these laws deny one's ability to move his hand at will the preferable conclusion is that Newton's laws require modification."[1] Such a step, however, is not necessary, for physical science has itself shown that "natural phenomena do not obey exact laws."[2] Science, in other words, must itself "abandon its cherished law of causality."[3] "I should consider it more

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[1] *The Freedom of Man*, p. 26.

[2] *Ibid.*, p. 7.

[3] *Ibid.*, p. 23.

likely," insists the author, "that the principle of the conservation of energy or the second law of thermo-dynamics would be found faulty than that we should return to a system of strict causality." [1] Hence for Compton the problem is one of constructing a theory from which it will be possible to deduce both the fact of human freedom and the fact of a non-causal nature.

The unpredictability of natural events means that in connection with small particles "knowledge of the initial conditions does not enable us to predict what will happen, for with the same initial conditions we cannot consistently produce the same effect." [2] Hence a particle which chooses one path would be physically indistinguishable from a particle which chooses another. The question then arises as to whether there would be any distinction at all between two such particles. The principle of operationalism demands that "only those aspects of the world have reality which are capable of showing themselves in some way to the observer." [3] It apparently follows from this that a distinction which is physically undetectable is non-existent, and this seems to be the only conclusion which we can draw as physical scientists.

But this point of view "is not broad enough to cover all of our experience." [4] Hence we must extend our postulate in such a way as to permit the discerning of differences where there are no physical differences. This is apparently done by assuming that physical events have properties which are not detectable by physical techniques. On the one hand such an assumption allows for the possibility that two physical particles should differ, say, in "goodness" and "badness." [5] Such a distinction would not be open to physical inspection, but it would not be therefore non-existent. On the other hand such an assumption asserts "that the matter in our brains may occur in conditions which though physically indistinguishable nevertheless correspond to distinguishable states of consciousness." [6] Hence

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[1] *Ibid.*, p. 23.

[2] *Ibid.*, p. 39.

[3] *Ibid.*, p. 40.

[4] *Ibid.*, p. 43.

[5] *Ibid.*, pp. 39–40.

[6] *Ibid.*, p. 44.

two brain particles may be physically identical, yet they may correspond to different states of mind. We are presumably aware of this non-physical difference in brain particles through introspection, though we are not informed of the channels through which we become aware of such properties as the "goodness" and "badness" of non-brain particles.

It follows that what is for physical science random action may be for introspection caused and predictable action. Given a brain particle in a certain state, physical science can make no definite prediction as to its behavior, since the ascertainment of both position and velocity of the particle may be impossible. But introspection, being provided with additional information about the particle, can anticipate its behavior. Through introspection one can know whether the physical state of the particle is associated with a decision of one kind or with a decision of another kind; hence one can predict what the behavior of the particle will be. Physics must here admit its definite limitations. For, "if true freedom is postulated, with a knowledge of motives, a more accurate prediction of the actions of a living being is possible than can be made from a knowledge of the physical conditions alone." [1]

Two further features are required in order to complete the theoretical structure. One is the volitional act itself. A brain particle must be subject to control by will. Here it is important to refer to Compton's figure of the mind as provided with a shutter by means of which it controls the orifice through which the electrons pass, allowing only the "good" electrons to pass and preventing the "bad" electrons from entering. Thus mind is able both to distinguish between the "good" and "bad" particles, and to control their movements. Now it is clear that such a selective and controlling act will be possible provided it does not conflict with any physical law. But here one need have no fears. Physical science can predict only on grounds of probability, i.e., it can assert that of the possible behaviors of a particle one is

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[1] *Ibid.*, p. 28.

more or less probable than another. But under such conditions the intervention of an act of will is physically undetectable. If the selective acts of volition were so adjusted that over a long period the probability distribution of the paths of particles were interfered with there would be a physically observable effect, and the act of intervention could be inferred. But in a single case, or in a limited run of cases, the presence of an act of decision could not be detected. Hence one's direct consciousness of freedom conflicts in no way with the conclusions of physical science.

The second feature required is the assumption that organic behavior is such as to permit large-scale results to follow from small-scale causes. There must be something like "trigger action" in the human body. For the indeterminism in nature is applicable only to minute particles, hence the selective action must be applicable only to small-scale phenomena. But "the living organism, in turn, acts as an amplifier of very great power, which may be set in operation by events on a scale comparable with the elementary events which we know to be indeterminate. Considering the complexity of the small scale events associated with any of our deliberate acts, one may say with assurance that on a purely physical basis the end result must have a relatively great uncertainty." [1]

By means of some such theory as this our intuitive awareness of freedom may be justified and shown to be perfectly compatible with modern science. Physical laws "serve to define the limits within which action is possible. Within these limits there may be a wide range wherein a man may do as he pleases without violating any physical law. That he actually does as he pleases is a matter of everyday experience. A man's pleasure, in other words consciousness, is thus an additional determining factor which supplements the physical laws in defining his actions." [2] Such a theory clearly satisfies the theoretical demands of freedom. For, in the first place, volition is not itself determined by any

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[1] *Ibid.*, pp. 50–51.

[2] *Ibid.*, pp. 64–65.

physical features of events. It may be said, in a sense, to be controlled by a non-physical property of events, since volition is affected by the "goodness" or "badness" of events. Hence volition is causally responsible not to the physical realm, which would destroy freedom, but merely to an anterior mind-state which includes the recognition of moral qualities of events; this, however, as was seen, reduces to self-determinism, which is equivalent to freedom. Accordingly, the theory allows for freedom in the sense that it retains the aloofness of volition from the physical processes. But, in the second place, volition is effective over physical processes; brain particles are under the control of will. However this causal efficacy involves no interruption of the physical course of events, since it occurs only in those situations where physics is unable to ascertain causes. Consequently the theory allows for freedom also in the sense that it permits the formulation of the laws which state the functional dependence of a physical state upon an earlier volitional state.

PLANCK

The main difference between Planck and the two writers just considered is that the former argues for human freedom independently of the principle of indeterminacy. As was shown in Chapter XVI, Planck insists that the principle of indeterminacy has no objective implications. To conclude to a complete breakdown of the law of causality seems to him premature. "It is far more natural to avoid the difficulty by another method, a method which has often rendered good service in similar cases and which consists in assuming that it is meaningless, with respect to physics, to ask for the simultaneous values of the coördinates and of the velocities of a material point or for the path of a photon of a given color. Evidently the law of causality cannot be blamed because it is impossible to answer a meaningless question; the blame rests with the assumptions which lead to the asking of the question, i.e., in the present case with the assumed structure of the physical world image."[1] It seems, however, that any system which is substituted for the classical one must exhibit a causal structure, though "causal" may require some modification for alternative schemes.[2] "In my opinion, therefore, it is essential for the healthy development of Physics that among the postulates of this science we reckon, not merely the existence of law in general, but also the strictly causal character of this law."[3] On such an unequivocal assumption there would seem to be no hope that freedom could be demonstrated by evidence drawn from the principle of indeterminacy. Yet Planck does just this, though his use of the principle is somewhat indirect.

He begins by insisting that there is no question as to the fact of causal laws in the realm of human affairs. "The highest types of human intelligence are subject to the causal law in the processes that result in even their greatest achievements."[4] "We must admit that the mind of each one of our greatest geniuses—Aristotle, Kant or Leonardo, Goethe or Beethoven, Dante or Shakespeare—even at the moment of its highest flights of thought or in the most profound inner workings of the soul, was subject to the causal fiat and was an instrument in the hands of an almighty law which governs the world."[5]

The surprise which one experiences in the presence of such a statement, argues Planck, is due to his failure to distinguish between the *validity* of the causal principle and the *practicability* of its application. "Under all circumstances the law of causation is valid, because of its transcendental character."[6] But "it must be remembered that we ourselves are only common mortals, and that we could never hope to be in a position to follow out the delicate play of cause and circumstance in the soul of a genius. . . . The whole point lies in the inadequacy of the observer. Just so the macroscopic physicist is entirely unable to pursue microscopic workings in natural phenomena, yet, as we have seen, this

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[1] *The Philosophy of Physics*, pp. 63–64.

[2] *Ibid.*, p. 73.

[3] *The Universe in the Light of Modern Physics* (New York: Norton, 1931), p. 89.

[4] *Where Is Science Going?*, p. 158.

[5] *Ibid.*, pp. 157–158.

[6] *Ibid.*, p. 157.

does not mean that the law of causality is not valid for these microscopic happenings."[1]

What is the sense, then, Planck asks, in speaking of causal relations in cases where the difficulties of observation render their applications impossible? The analogue of the quantum phenomena[2] is enlightening here. The principle of indeterminacy shows the impossibility of measuring both the position and the velocity of a particle with the necessary accuracy. This involves the conclusion that *prediction* of the behavior of a particle is impossible, since it depends on *knowledge of state* and this can never be obtained. Now if this view is combined with the operational theory, the impossibility of knowledge becomes the ground for the impossibility of existence; hence there is an objective indeterminism. But this step Planck will not take. He insists that the difficulties in the ascertainment of position and velocity of a particle are due to the techniques of measurement, i.e., to the relation of the observer to his phenomena. They are not difficulties of the *validity* of the causal principle but of the *practicability of its application.* The virtue of the principle of indeterminacy is that it has shown us the importance of recognizing the observer and the methods which he employs in observing. More specifically, it has shown us the general principle to be used in determining the applicability of a causal law: "The smaller the distance between the investigator and the object . . . the more uncertain and fallible will be the causal and scientific treatment."[3] "Laplace held that if there were a super-intelligence standing entirely outside of the facts occurring in the universe, this intelligence would be able to see causal relations in all happenings of the world of man and nature, even the most intricate and microscopic. It is only by aiming at this sort of distance that the individual could establish the required detachment of the perceiving subject from the object of his research, which we have already seen to be an inevitable condition for the

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[1] *Ibid.*, p. 156.

[2] *The Philosophy of Physics*, p. 80.

[3] *Where Is Science Going?*, pp. 155–156.

application of the causal method in research. The nearer we are to events in time the more difficult it is to trace their causal structure." [1] This, in fact, is the explanation of the use in science of such a thing as an "ideal spirit," or the Laplacian omniscient being. "Scientific thought always requires a certain distance and a clear separation as between the thinking subject and the object of his thought, and this distance is best guaranteed by the assumption of an ideal spirit." [2]

We are now ready for the application of this notion to the problem of human freedom. It has already been shown that human behavior is regulated by causal laws; hence there is no reason for denying the *fact* of causation as applied to human beings. But what may be said of the *applicability* of the law? Here a genuine difficulty arises. "There is a point, one single point in the immeasurable world of mind and matter, where science and therefore every causal method of research is inapplicable, not only on practical grounds but also on logical grounds, and will always remain inapplicable. This point is the individual ego." [3] What are these grounds, practical and logical? The practical grounds are easy to discern. In practice we can never discover the causal connections in our own personal conduct, "because this would mean that the observing subject would also be the object of research. And that is impossible; for no eye can see itself. But in so far as any man is not entirely today that which he was years ago there is a relative degree to which he might subject his own experiences to causal scrutiny." [4] Presumably this limitation is due, then, merely to the limitations of the human intelligence; were our understanding more comprehensive we should be able to make a complete analysis of ourselves. This, however, is erroneous because of the difficulties constituting the logical grounds. "It would be a complete mistake to attribute the impossibility of forecasting the subject's actions on purely causal lines to a lack of knowl-

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[1] *Ibid.*, p. 162.

[2] *Philosophy of Physics*, p. 81.

[3] *Where Is Science Going?*, p. 161.

[4] *Ibid.*, p. 163.

edge which might be overcome if the individual intelligence were suitably increased."[1] "Such an attempt is condemned to failure in advance because every application of the law of causality to the will of the individual and every information gained in this way is itself a motive acting upon the will, so that the result which is being looked for is continually being changed."[2] Applying the general principle noted above, "the nearer we are to the events of our own personal experience the more difficult it is for us to study ourselves in the light of these happenings; for the activities of the observer are here partly the object of research and, in so far as that is so, the causal connection is practically impossible to establish."[3] "We cannot possibly study ourselves at the moment or within the environment of any given activity. Here is the place where the freedom of the will comes in and establishes itself, without usurping the right of any rival."[4] "The impossibility of foretelling the subject's actions on purely causal lines is not based on any lack of knowledge, but on the simple fact that no method by whose application the object is essentially altered can be suitable for the study of this object."[5]

If this be freedom, it is freedom of a different kind from that demonstrated by Eddington and Compton. It is not a freedom which is based upon the efficacy of a factor which eludes physical analysis and which is itself not the result of any physical processes. Nor is it a freedom which is self-determination. It is rather a freedom which is equivalent to unpredictability. The self is free because no one short of an omniscient being could ascertain the complete complex of causal factors effective in a given situation. Man acts, therefore, not without cause, but without anticipated knowledge as to how he is to act. He is under the control of a causal law, but it is transcendental not empirical. At the level of his understanding, therefore, he is free. But to assert this means no more than that his actions are unpredictable, for,

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[1] *Philosophy of Physics*, p. 80.

[2] *Loc. cit.*

[3] *Where Is Science Going?*, p. 163.

[4] *Ibid.*, p. 164.

[5] *Philosophy of Physics*, p. 80.

apart from some direct consciousness of freedom, this is all that can be meant by the term.

Criticisms of the foregoing positions on the question of human freedom are not difficult to make, and the reader will be able to supply them readily. As in the case of all speculative problems, highly cogent arguments are not to be found. The dependence of speculative considerations upon the more critical questions is here more apparent, perhaps, than anywhere else in the speculative field. Clearly the solution to the problem of human freedom is possible only on the grounds of a more precise analysis of such terms as "cause," "law," "determination," and the like, which are common to the sciences, and such terms as "self," "volition," "mind," and the like, which are peculiar to the psychological sciences. It seems safe to say with reference to the future that the problem of human freedom cannot be solved until clarification is introduced into these more basic notions.

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CHAPTER XX

THE NATURE OF REALITY

The last of the speculative problems to be considered is, in a sense, the most important of the three. At least it is that which has been most debated by scientists and philosophers. Much of the history of philosophy can be formulated in terms of the controversies over the precise limits of the realm of the given, and the justifiability of making inferences beyond the obviously given to that which is required in order to make it intelligible. All materialisms and idealisms, subjectivisms and objectivisms, theisms and atheisms, monisms and pluralisms, to mention only a few of the outstanding metaphysical positions, are attempts to solve this problem in one way or another. Hence so far as range of significance is concerned, speculative problems in this field are important among the problems in the philosophy of science.

This very fact makes a discussion of such problems difficult, for one hardly knows how to make a selection among the possible candidates for consideration. To insist that only those positions should be examined which are founded on the facts of science is to employ a variable criterion, for practically every metaphysical position attempts to accommodate itself to science. Similarly, to include only those positions which proceed in the essential spirit and method of science may be unduly severe, for, as was shown in Chapter XVII, probably none of the speculative problems can be solved by rigid adherence to the critical methods of science. Yet to select on the grounds of personal bias, i.e., to include only those positions which seem to the writer to be most adequate, is hardly fair.

The principle employed in the selection of the following illustrations is very simple. It has resulted, perhaps, in an unbalanced list. But since it is offered merely for purposes of example, there is no danger of misleading. The common feature of all of the following positions is that they attempt to draw conclusions as to a theory of reality upon the basis not of the *facts* of science but of its *methods*. According to all of them not *what* the scientist knows but the *way* in which he knows is important for a more inclusive outlook. For example, in three of the following cases there is specific reference to the character of scientific symbols and the way in which they presumably designate their referents; in the fourth the data are to be found in the method of idealization, which is, again, a fact about the knowing rather than that which is known. The basing of speculative problems on the techniques of knowing rather than on the actual conclusions seems to give them a firmer foundation. For any fact of science is more or less specific. But methods, in the very nature of the case, are more general and permeate all of the sciences to a certain extent. Furthermore, they are more permanent, hence make the speculative superstructure less precarious. It is probably true that each of the positions to be considered makes an erroneous analysis of the specific feature of method to which it refers, but this is of no importance when one's purpose is mere illustration. The actual ways in which they make use of these methodological aspects can be made clear only if there is a preliminary consideration of the general problem.

GENERAL CHARACTER OF THE PROBLEM

The first feature to be emphasized is the fact that the inference to a realm beyond that which is obviously given is, in a certain sense, unavoidable. Though science—and even philosophy—professes to be satisfied with a pure phenomenalism, it has not often remained long in this unstable position. As Bacon long ago pointed out, the mind is prone to soar into flights of the imagination, and can be restrained only with difficulty; there is so little difference between the disciplined imagination which is reasoning, and the undisciplined imagination which is speculation that the mind passes almost imperceptibly from one to the other. But the instability of phenomenalism lies not merely in the character of the mind; the given itself offers hints of something beyond. No phenomenalism can fail to recognize the distinction between appearance and reality, yet the distinction, once admitted, compels one to abandon phenomenalism. Even though reality is defined to be merely *alternative appearances*, positivism must be forsaken; for the given then becomes not that which is *actually* given, but that which is *potentially* given as well, i.e., that which *may be inferred to be given* on the basis of that which *is given.* To assert this, however, is to abandon the phenomenalistic position and to admit the legitimacy of the speculative problem. The views which will be considered in this chapter are at one in their insistence that the given is not to be identified with the obviously given; the obviously given permits inference of greater or lesser cogency to a less obviously given. The fact that the inferred realm is often spoken of as being "hidden," or as being "behind" or "beneath" the more apparent world should not lead to the impression that it is an unknowable world, or that it is a product of the purely creative imagination. It is always knowable at least inferentially (sometimes directly as well as inferentially), and it cannot be purely imaginative for its character is determined by certain data; if the data were different the inferred realm would have a different character. Concern with the speculative problem of the nature of reality is nothing more than the frank recognition that the hints which events give as to the existence of other events beyond themselves are to be taken seriously, and are to be followed out to see where they lead.

The second feature to which attention should be called is the possible ambiguity associated with the word "reality." Unquestionably the word is essentially vague, and much could be accomplished by following the suggestion of the logical positivists and abolishing it entirely from the philosophical vocabulary. What should be noted is merely the tendency in any explanatory act for the explaining entity to assume greater importance than the explained entity. Explaining is almost always assumed to be equivalent to explaining *away*. When heat has been explained in terms of molecular motion there seems to be no further occasion to refer to heat since the molecules have taken its place; hence heat is put into a subjective realm, or a realm of appearance, and the molecules are located in the "real" world. That which is logically more basic seems necessarily more fundamental in the structure of the world; consequently it tends to displace that which is psychologically more primitive. The most real aspect of the world then becomes not that which is most obviously given, but that from which the given can be derived by logical processes. The given then becomes appearance, and the logically basic becomes reality. It is only in this sense that the speculative problem may be said to be a search for reality. In each case there is a search for something beyond the obviously given in terms of which it may be rendered intelligible. When this has been found, and when it has been shown to explain the given, it takes on a greater importance than the given and becomes thereby more real. Nothing of a more metaphysical character should be attributed to the word "real" than this.

The third feature of the speculative problem is the religious tinge which is commonly given to it. The "reality" which is found is frequently identified with the God of the religious experience. The reasons for this identification are well recognized, and probably, in some sense, justify it. But this close association of an emotional ideal with a more or less rational hypothesis results in a certain amount of confusion. It turns out, more often than not, that the essential grounds for inference to a realm "behind" the given are not the feature of science to which acknowledgment is made but a much more subtle intuitive or mystic experience. As a result the problem is not one of inferring to a realm of reality but rather one of reconciling the facts of science with the information disclosed by the emotional experience.

Science does not prove God, though the religious experience does; and the problem is to fit God into a scientific scheme in which he is not obviously present. This makes the speculative problem less an inference from the facts of science and more an inference from the facts of religion or intuition. There is probably something of this in all of the writers to be examined. It does not necessarily invalidate their results, but it makes their conclusions somewhat less directly dependent on science.

In considering the various solutions to the problem of the nature of reality it will be well to follow the outline suggested in Chapter XVII. Accordingly, an attempt will be made in each case to show (*a*) what the initial data are, (*b*) what the character of the inference is, and (*c*) what the nature of the inferred realm is.

SPIRITUAL IDEALISM

The data with which Eddington starts are such as constitute the world-view of the sophisticated common sense man, who proves to be really the scientist himself. For such an individual the world consists of two kinds of objects, with an important relation between them. On the one hand there are the objects of everyday experience. Eddington suggests that every conscious being is involved in a story, which the perceiving part of his mind tells him of a world in which he lives. This world contains familiar objects such as colors, sounds, and scents, which are located in a boundless space and in a stream of time.[1] Consider, for example, such a thing as a table. It would ordinarily be described as having extension, as being comparatively permanent and colored, and especially as being substantial.[2] But there is another world—the world of scientific objects. Physics contradicts the story-teller. Science tells us that the table is not a substantial desk, not the continuous substance which it is supposed to be according to the story. On the contrary it is a mass of electrical charges in rapid motion. It is more like

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[1] *New Pathways in Science*, p. 1.

[2] *Nature of the Physical World*, p. ix.

a swarm of gnats than a substantial object.[1] The speculative problem begins, therefore, not with the fact of objects but with the fact of duplicate objects. There is not only heat but molecules, not only substantiality but disembodied electric charges, not only colors but electro-magnetic waves.

What prove to be the real data upon which the speculative problem builds, however, are certain facts pertaining to the relation between these realms. The task of science is that of constructing a world which is to be symbolic of the world of commonplace experience.[2] This must be a "world which will imitate the actual behavior of the world of familiar experience." [3] Using the word "infer" in a somewhat broad sense science may be said to infer its description of the external world from the facts of sense experience.[4] This determines an important relation between the realms. In the vocabulary of the physicist one finds such words as "length," "angle," "velocity," "force," "potential," "current," etc., which are called "physical quantities." It is now being asserted that these should be *defined* no longer in terms of any metaphysical significance which may have been attached to them but according to the way in which they are actually identified when we are confronted with them.[5] But how do we recognize them? By reading scales, dials, clocks, and other measuring devices. In other words the entities of science prove to be nothing but pointer readings on recording instruments. Eddington's famous illustration of the method of science in the problem concerning the elephant sliding down the grassy hillside has already been quoted.[6] All of exact science consists of similar pointer readings and other indications on instruments. The conceptions of objects which function in our common view of the world do not enter into exact science. Science must replace such conceptions by quantities representing the results of physical measurement.[7]

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[1] *New Pathways in Science,* p. 1.

[2] *Nature of the Physical World,* p. xiii.

[3] *Ibid.,* p. 249.

[4] *New Pathways in Science,* p. 9.

[5] *Nature of the Physical World,* p. 254.

[6] Pp. 15–16.

[7] *Ibid.,* p. 253.

This fact has important implications for a theory of reality, since it puts definite limitations on the scope of physical knowledge. Our knowledge of the properties of a body consists merely of the responses of various metrical indicators to its presence. Beyond this knowledge cannot go. If one could get the responses of an object to all sorts of devices—scales, clocks, meter sticks, thermometers, etc.,—he would know it completely in relation to its environment. All that he could not know would be its "inner un-get-at-able" nature.[1] Hence it turns out that the world of scientific objects becomes not only symbolic of the world of experience but merely a "shadow" world [2] without the substantiality of the world of everyday objects. It is only such knowledge of that world as we should have of a game of chess if we knew merely the rules of the game and not the nature or appearance of the chessmen,[3] or such knowledge as we should have if we knew only the structural form of the world and not its content.[4] Our conception of the physical world today is essentially *hollow*. It is merely a system of symbols connected by mathematical equations. Such a scheme, Eddington maintains, is essentially a skeleton, and proclaims its own hollowness.[5] In fact, if anyone knew the method of the physicist in advance he would be able to foretell the kind of world which would be revealed through the use of this method.[6]

Such are the data of the speculative problem. What of the inference? It appears that the nature of scientific symbols hints at a reality which is non-metrical. The hollow world "can be—nay it cries out to be—filled with something that shall transform it from skeleton into substance, from plan into execution, from symbols into an interpretation of the symbols."[7] A wave is an abstraction which may be filled with a variety of contents; there may be waves of water, waves of air, waves of ether, and waves of probability. Physics shows only that the world is made up of waves, and

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[1] *Ibid.*, p. 257.

[2] *Ibid.*, p. xiv.

[3] *Space, Time, Gravitation*, p. 184.

[4] *Ibid.*, p. 200.

[5] *New Pathways in Science*, p. 313.

[6] *Nature of the Physical World*, p. 271.

[7] *New Pathways in Science*, p. 314.

leaves one to determine through some other avenue what kind of waves these are.[1] It is readily seen, therefore, that the knowledge with which physics deals is inadequate to the understanding of the human spirit in its broader aspects. One could not live in the scientific world of pointer readings; such a realm could be happily inhabited only by a *symbol.*[2] But man is more than a symbol, and many of the aspects of his life and activity take him beyond the outlook of physics.[3]

But where do we get a more specific indication as to the character of this residue of reality? Here we recognize for the first time that the data upon which the solution to the speculative problem is based are not to be found exclusively in science. Scientific symbols would presumably permit us to infer only a scientific world. But there seem to be two important kinds of knowing which are outside of science. One is the mystic experience. In it "we catch something of the true relation of the world to ourselves—a relation not hinted at in a purely scientific analysis of its content." [4] This intimate and personal knowledge will not permit itself to be symbolized or codified; when we attempt such analysis we find that the intimacy is lost.[5] The mystic experience is such as occurs to us under the starry heavens in the moments just before daybreak, or when we pause for a minute on Armistice Day to commemorate those who died in the war, or when we are carried away by a great poem. Such experiences give us an insight into something which is not science, and which is incompatible with it if science is presumed to give us a knowledge of the inner nature of things. Yet the mystic experience reveals something which cannot be omitted in a total picture of the world.

The second sort of knowledge is the knowledge of ourselves. An individual knows that he thinks with a certainty far beyond that which he can attribute to any physical knowledge. This fact of thinking must be investigated. Suppose the physicist undertakes to solve the problem. He

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[1] *Ibid.*, p. 320.

[2] *Nature of the Physical World*, p. 324.

[3] *New Pathways in Science*, p. 316.

[4] *Nature of the Physical World*, p. 320.

[5] *Ibid.*, p. 322.

brings his instruments and begins to explore. What does he find? Merely atoms, electrons, and fields of force, all arranged in a spatio-temporal system. His knowledge of thinking proves to be just like his knowledge of inorganic objects. If he explores further he finds only such things as energy, temperature, and entropy—none of which proves to be *thinking itself*. What, then, is to be done with thought? Is it to be waved aside as an illusion? There seems to be no reason for this. For there is nothing in his knowledge of atoms that makes it impossible for them to be at the same time thinking objects. Science does not talk about the intrinsic natures of things. An atom is a schedule of pointer readings. Presumably, however, it is not *merely* a schedule of pointer readings. There must be some unknown background to which the symbol is attached. Now in the case of introspective knowledge I know what this background is. It is precisely that spiritual nature which I discover when I examine thinking independently of science. Hence the twofold character of my knowledge of my own thinking permits me to fill in the scheme of pointer readings with a definite content. This content is something which may be called "mind" or "consciousness."[1] One might almost say that modern science "had deliberately left room for the reality of spirit and consciousness."[2]

The character of the inference can now be clearly seen. It is analogical. Both mind and nature are such as to be describable through metrical symbols; but mind is also such as to be describable in qualitative terms; hence nature is susceptible of a similar description. Nature, therefore, may be presumed to be like mind in its essential stuff. But our knowledge of this realm is not merely analogical, since we are immediately acquainted with it in the mystic experience. Hence we know it both indirectly and directly.

The nature of the inferred realm is thereby determined. The "stuff" of the world is mind-stuff.[3] Just what this

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[1] *Ibid.*, pp. 258–259.

[2] *New Pathways in Science*, p. 320.

[3] *Nature of the Physical World*, p. 276.

proves to be is somewhat vague. It is not to be identified with consciousness,[1] and is something more general than our individual minds.[2] However, it may be interpreted in terms of personality,[3] and is that with which one has contact through the mystic experience. The mind-stuff is not spread out in space and time, for the latter are metrical notions associated with pointer readings. In fact the stuff of the world proves to be just those features which elude metrical methods—significance and values, beauty and melody, truth and untruth. When the human heart, perplexed with the mystery of existence, cries, "What is it all about?," the answer cannot be given in terms of pointer readings. The satisfying answer is in terms of "a spirit in which truth has its shrine, with potentialities of self-fulfillment in its response to beauty and right. Shall I not also add that even as light and color and sound come into our minds at the prompting of a world beyond, so these other stirrings of consciousness come from something which, whether we describe it as beyond or deep within ourselves, is greater than our own personality."[4]

It can be readily seen that this world of mind-stuff is not really demonstrated on the grounds of science. Science does not—in fact, science cannot—tell us of this world. Only introspection and the mystic experience can do this. But science *permits* such a world as this to exist by virtue of the character of its symbols; scientific symbols are "hollow" and therefore refer only in a highly abstract way to objects. But this residue of objects, which is not represented in the symbols, must be mind-stuff. Thus the conclusion from the nature of symbols to the character of the real world is based upon the *inadequacy* of symbolic representation. Because symbols are *unlike* their referents, the real world cannot be like our symbols of it; hence, since our symbols are mathematical, the world itself cannot be mathematical. The nature of this inference should be clearly seen, since it has an interesting contrast in the position of Jeans, to be examined immediately.

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[1] *Ibid.*, pp. 277–278.

[2] *Ibid.*, p. 276.

[3] *Science and the Unseen World*, p. 50.

[4] *New Pathways in Science*, pp. 317–318.

MATHEMATICAL IDEALISM

Jeans begins, as does Eddington, with the general fact of science as a system of symbols attempting to portray the character of a world which is directly revealed. But the given world proves to be one of shadows, not realities. In terms of Plato's famous figure, man is imprisoned in a cave with his back to the light, and he cannot observe reality but must rest content with watching the shadows as they play on the wall. The task of science is the task of the man in the cave, viz., to classify and explain the shadows in the simplest possible way.[1] Many historical attempts to explain the shadows have failed. Primitive man tried anthropomorphic concepts, and the science of the last century employed mechanical concepts. Both of these attempts proved unsuccessful. But we have at last discovered an interpretation which seems to be adequate. We find that every successful picture of nature which we now draw is *mathematical.*[2] This, then, must be the magic key which unlocks reality, and our only conclusion must be that nature itself is somehow more congenial to the concepts of pure mathematics than to those of biology or of engineering. This is not to deny that mathematics may, in turn, be a man-made mold. But this symbolic scheme "fits" nature in a way which surpasses those already tried.[3]

It appears, then, that the shadow world is the world of familiar objects. But science is not content with merely accepting this realm as given; it attempts to explain. The result of this attempt is the disclosure of a new realm which might be called the realm of scientific objects, or symbols. Here we find such things as differentials and integrals, space which is curved and expanding, waves of probability and entropy. An examination of these entities reveals their mathematical character; in fact, it shows them to be not merely mathematical but objects of *pure* mathematics. As such they could not have been derived from our experience

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[1] *The Mysterious Universe* (New York: Macmillan, 1932), p. 151.

[2] *Ibid.*, p. 150.

[3] *Ibid.*, p. 158.

of everyday objects, but must be pure creations. Examine some of them—finite space, empty space, expanding space, seven-dimensional space. What are these but structures of pure thought? Or consider the notion of a sequence of events following a probability rather than a causal sequence. What can this be but a pure mathematical creation, "incapable of realization in any sense which would properly be described as material?" [1] Whatever is created out of pure thought must *be* pure thought.

But, granting that this is the starting-point for Jeans's reflections, his conclusion is quite different from that of Eddington. Both astronomers recognize, apparently, that scientific knowledge is merely symbolic. But it is not clear from their writings whether they are conscious of the nature of symbolic reference. As has already been seen in Chapter III, every symbol aims to represent its referent, and thus may be said to be in some sense "like" its referent; but no symbol is able to portray *all* of the features of the referent, hence is obliged to omit one or more of them. Given any symbol, therefore, one may infer the referent, since the symbol resembles it, but not all of the referent, since the symbol is an abstraction. Jeans accepts the former but forgets the latter; Eddington accepts the latter but forgets the former. Hence for Jeans the fact that scientific symbols are mathematical permits one to infer that reality is mathematical, but for Eddington the same fact argues for a non-mathematical residue.

Through this mode of inference Jeans arrives at the conclusion that reality is a world of pure thought. But it is not the same type of thought which Eddington finds. The universe exhibits evidence of a designing power, which has much in common with our individual minds. But it is not emotion, morality, or esthetic enjoyment which we discover in nature. Instead it is something which we find hard to describe, but which we may characterize as "mathematical thinking." [2] The Great Architect of the Universe appears, when we examine his work, not as a personal being but as a pure mathe-

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[1] *Ibid.*, p. 166.

[2] *Ibid.*, pp. 186–187.

matician.[1] The conclusion is analogous to that of Berkeley, who was led to suppose an Eternal Being as the locus of all objects.[2] Such a view does not reduce the world of science to the status of hallucinations or dreams, for whereas the latter are the creation of the individual mind the former is the creation of the universal mind, of which "the uniformity of nature proclaims the self-consistency." [3] Yet the view does much to overcome the dualism between mind and matter. The supposed hostility disappears "not through matter becoming in any way more shadowy or insubstantial than heretofore, or through mind becoming resolved into a function of the working of matter, but through substantial matter resolving itself into a creation and manifestation of mind." [4] There is no longer any problem in understanding the way in which mind can grasp nature, "for it reduces merely to a contact between mind and a creation of mind—like the reading of a book, or listening to music." [5]

The position of Jeans is somewhat less speculative in character than that of Eddington. Jeans does not bring in the extraneous facts of introspection and the mystic experience, and consequently his position may be said to follow more directly from the facts of science. One can hardly deny the effectiveness of mathematical symbols in the explanation of the data of science. But that this argues for a mathematical reality "behind" the world of everyday objects—which then become shadows—is somewhat questionable. Certainly Jeans places too much emphasis on the "pure" character of these mathematical notions; though they are not such as can be exemplified in the gross objects of experience, and hence seem to be simply creations of the mind, nevertheless they can easily be shown to be derivable from these gross objects provided one admits the legitimacy of certain abstractive operations. This makes the "Great Architect" much less a creature of pure thought, remote from experience, and much more a creature in and of the actual world. Other criticisms of Jeans's position will occur readily to the reader.

[1] *Ibid.*, p. 165. [2] *Ibid.*, p. 170. [3] *Ibid.*, p. 175. [4] *Ibid.*, p. 186. [5] *Ibid.*, p. 178.

It is in sharp contrast with another position, to be examined immediately, viz., that of Bergson.

CREATIVE EVOLUTION [1]

In both method and content, the position of Bergson bears closer kinship to that of Eddington than to that of Jeans. All three would agree that the character of the scientific method determines the character of the world which is disclosed thereby; but whereas Jeans insists that the world thus revealed is reality, Eddington and Bergson insist that it cannot be. Science, according to the latter, gives only a partial, and to that extent erroneous, view of reality. Hence whereas Jeans concludes as to the nature of reality from the *adequacy* of symbols, Eddington and Bergson both conclude as to its character from the *inadequacy* of symbols.

The primary data for Bergson are concepts and the conceptual method, which constitute, respectively, the tools and techniques of science. Now the characteristic method of science is analysis.[2] "Analysis . . . is the operation which reduces the object to elements already known, that is, to elements common both to it and other objects." [3] "The different concepts that we form of the properties of a thing inscribe round it so many circles, each much too large and none of them fitting it exactly."[4] "Simple concepts have . . . the inconvenience of dividing the concrete unity of the object into so many symbolical expressions." [5] "These concepts, laid side by side, never actually give us more than an artificial reconstruction of the object, of which they can only symbolize certain general, and, in a way, impersonal aspects; it is therefore useless to believe that with them we can seize a reality of which they present to us the shadow alone." [6]

The real difficulty with the method of analysis is that it substitutes for "the real and internal organization of the thing" something which is merely "an external and sche-

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[1] The position of Bergson has already been considered briefly in another context. See above, pp. 18–20.

[2] *Introduction to Metaphysics*, p. 8.

[3] *Ibid.*, p. 7.

[4] *Ibid.*, p. 19.

[5] *Ibid.*, p. 21.

[6] *Ibid.*, pp. 18–19.

matic representation."[1] Analysis gives not parts of a whole but merely notes of a total impression. For this reason, by no act of reconstruction can one regain the whole upon which an analysis has been performed; thought may proceed from objects to symbols but it cannot retrace the route from symbols to objects. Hence scientific knowledge is always partial and relative, and to this extent erroneous. It supposes that since an object has a number of aspects it is therefore itself a plurality; it endeavors to construct an object by multiplying to infinity the abstract symbols in terms of which it is portrayed; it attempts by representations which are outside the object to get inside it.

One of the best illustrations of the inadequacy of the conceptual approach to reality can be seen in the scientific attempt to explain motion. Motion is analyzed into briefer and briefer durations terminating in mere positions, which represent the points through which the moving body passes. Motion is then defined as a series of such positions. But from a mere series of positions, argues Bergson, we cannot get motion. The positions "are not parts of the movement, they are so many snap-shots of it. . . . The moving body is never really *in* any of the points; the most we can say is that it passes through them. But passage, which is movement, has nothing in common with stoppage, which is immobility." The points "are not, properly speaking, positions, but 'suppositions,' aspects or points of view of the mind."[2] Thus by means of a characteristically scientific approach to reality we find that we have lost that very reality; science is incapable of revealing the true nature of things.

It soon becomes clear that Bergson's theory of reality is based not merely upon the inadequacy of scientific symbols. As in the case of Eddington, Bergson finds the scientific approach to be unsatisfactory because there is another approach which seems to be more satisfactory. It seems impossible to conclude that science fails to disclose reality unless there is some other method which does disclose real-

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[1] *Ibid.*, p. 27.

[2] *Ibid.*, p. 49.

ity. This, for Bergson, is the method of intuition. "By intuition is meant the kind of *intellectual sympathy* by which one places oneself within an object in order to coincide with what is unique in it and consequently inexpressible." [1] The intuitive method is "truly itself when it goes beyond the concept, or at least when it frees itself from the rigid and ready-made concepts in order to create a kind very different from those which we habitually use; I mean supple, mobile, and almost fluid representations, always ready to mould themselves on the fleeting forms of intuition." [2] Intuition gives us that which is essential and unique in the object; it is that mode of knowing in which we are not merely acquainted with the object, but united with it in that intimate way which enables us to grasp its differentiating features. Whereas concepts draw circles around objects, intuition reveals their individualities; whereas conceptual knowledge is external, representing so many points of view, intuitive awareness discloses the inner nature of things; whereas symbolic knowledge is relative and indirect, intuitive knowledge is absolute and direct.

When we recognize the complete range of data upon which the solution to the speculative problem is based, there is reason to doubt whether Bergson has really made an inference from the character of scientific symbols to the character of reality. It is not scientific symbols which reveal the character of reality in a negative way, but the intuitive method which reveals it in a positive way. There is probably an analogical inference of somewhat the same kind as is found in the case of Eddington, though it is not so clearly defined. For Eddington inference may be made from the spiritual character of mind, to the spiritual character of inorganic nature; for Bergson the obvious inadequacy of static scientific symbols in our attempt to understand the character of certain types of object such as the self, motion, and growth, argues for a similar though less obvious inadequacy in the attempt to understand things in general; hence science may

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[1] *Ibid.*, p. 7.

[2] *Ibid.*, p. 21.

be presumed, by virtue of the nature of its method, to lose the significant features of things. In a word, because science fails to reveal mobility in a realm where it is known to be present, this method cannot be expected to reveal mobility in a realm where it is not known to be present; hence reality as a whole may be presumed to exhibit mobility.

The inferred realm is thus defined essentially in terms of mobility and evolution. "Not *things* made, but things in the making, not self-maintaining *states*, but only changing states, exist. Rest is never more than apparent, or, rather, relative. The consciousness we have of our own self in its continual flux introduces us to the interior of a reality, on the model of which we must represent other realities. *All reality, therefore, is tendency, if we agree to mean by tendency an incipient change of direction.*" [1] Reality is duration, but duration which is lived and not symbolized. "We can, no doubt, by an effort of imagination, solidify duration once it has elapsed, divide it into juxtaposed portions and count all these portions, yet this operation is accomplished on the frozen memory of the duration, on the stationary trace which the mobility of duration leaves behind it, and not on the duration itself." [2] This reveals duration only as a multiplicity. "On the contrary, when I replace myself in duration by an effort of intuition, I immediately perceive how it is unity, multiplicity, and many other things besides." [3] "Inner duration is the continuous life of a memory which prolongs the past into the present. . . . Without this survival of the past into the present there would be no duration, but only instantaneity." [4] Reality is thus a growing and evolving entity, characterized essentially by motion, change, and emergence. Time is its most significant property. Its present is to be understood only in terms of the past out of which it is passing, and the future into which it is merging. It is not being, but becoming.

The essential similarity between the position of Bergson and that of Eddington is obvious. Both argue that symbols are incompetent to portray the significant aspects of the

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[1] *Ibid.*, p. 65. [2] *Ibid.*, p. 22. [3] *Ibid.*, p. 23. [4] *Ibid.*, pp. 44–45.

world—aspects which must be revealed, therefore, by some method other than the symbolic one. For Eddington this method is that of introspection and the mystic experience; for Bergson it is intuition. Hence reality for Eddington is essentially spirit and consciousness, while for Bergson it is mainly growth, motion, and tendency. In both cases the important datum offered by science is not a fact about the world, but a fact about method; it is because science has an *improper method* that it fails to grasp reality. For Jeans, on the other hand, science has at last found the *proper method*, hence is able to grasp reality. The fourth position to be examined, that of the mathematician, Keyser, is established upon slightly different grounds. Though it is a fact of method which argues for the existence of a superrational realm, it is a relatively specific kind of method rather than a general feature of the symbolic approach as a whole.

LOGICAL REALISM

C. J. Keyser, too, feels that the importance of science lies not so much in what it says as in what it suggests. Speculation about the sciences is therefore a legitimate enterprise. His main emphasis is upon a feature of the scientific method —"Idealization regarded in the light of what mathematicians call the method or the process of Limits. The central thesis is that this process in the domain of reason or of rational thought indicates the reality and, in part, the nature of a domain beyond, a realm superrational, and that this realm is the ultimate and permanent ground and source of the religious emotions." [1]

The data of the speculative problem are, for Keyser, of two kinds. Though these are primarily features of method, they are features of subject matter as well. On the one hand, there is the fact of idealization. "The countless phenomena in the world of sense form and present to us there innumerable series or sequences having for their limits ideal things that belong only to the world of reason: the realm of

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[1] *Science and Religion* (New Haven: Yale University, 1914), preface.

things *per*ceived has for its border the realm of things *con*ceived." For example, "perfect solids, like perfect gases, are nothing but limits, sheer ideals without existence in the world of sense, pure concepts in the domain of reason." These forms "are never actually realized in the subrational world of sense: they are there but indicated as limits beyond." [1]

On the other hand there is the method of limits, applied within the realm of the rational. If there be inscribed in a circle an equilateral triangle, then a regular hexagon, then a polygon of a dozen sides, and so on, forever, there is created a series of a certain type. "In respect of size, these approach nearer and nearer, as close as we please, to the size of the circle's area, yet they remain inferior to it forever. And we say, in technical language, that the circle's area is the sequence's *limit*. It is important to note that the sequence's limit is not a term in the sequence, for all these terms are polygonal areas—shapes bounded by polygons—but that of the circle is not, for the circle is not a polygon." [2] If the series of polygons be called the Domain of Polygonal Areas, one may say that the circle does not lie in this realm but upon its border. "Here, then, we have a clear presentation, within a given domain, of something that is not within: we have a clear presentation, by the law of an inner sequence, of a limit on the rim—of an ideal, if you please, which, so long as we operate within the domain, may be aspired unto, approached and pursued forever, but can never be attained." [3]

These, then, are the data: two types of serial order exhibiting limits. In the one case the limit is outside of the realm in which the series itself resides; in the other the limit is within the realm though outside of the series itself. What is common to the two cases is the fact that a serial principle of a certain kind apparently affords a basis for an inference beyond the series itself to an entity which is demanded by the series yet exhibits properties essentially different from the members. Keyser's next step is to generalize this principle

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[1] *Ibid.*, pp. 61–63.

[2] *Ibid.*, p. 55.

[3] *Ibid.*, p. 56.

to similar series in which the limit is not given. Hence his inference is essentially analogical in character: Since series in which the limit is not given are like the series in which the limit is given, a limit may be presumed to exist also in the former. This limit proves to be the realm of the super-rational.

The application of this principle, however, demands that the author "produce in the world of logical thought rational sequences that, by the law of their formation and progress, approach and betray as a border-domain a region of reality from which the dominion of logic is forever barred." [1] He finds no difficulty in locating such instances. One has simply to arrange all classes into a series upon the basis of comprehensiveness; such a series would have for a limit the class of all things. But such a class would be a very peculiar thing, for it would have to contain itself—something which cannot occur without contradiction in the world of familiar logic. Hence the fact that there are things demands that there be a universe which includes everything, yet this universe must be significantly different from the things which it contains. Another example can be found in the arrangement of propositions into a series, each proposition being the joint affirmation of the proposition immediately preceding it and some other proposition, until the limit is reached which would be the joint affirmation of all propositions. This, again, is itself a very peculiar proposition, for it must make an affirmation about itself—a fact which is forbidden in the realm of the rational. Hence the fact that there are propositions demands that there be a proposition of a type which is significantly different from the elements which required its existence.

Situations of these types are characteristic of the rational. "In every category where the laws of reason reign we find that the great process of Idealization points aloft to some form above the laws: we find that—like the Class of all Classes, like the Joint Affirmation of all Propositions, like the

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[1] *Ibid.*, p. 64.

Logical Sum of all Relations, like Omniscience, like Beauty Absolute—so, too, Eternity or Fate, Unconditioned Freedom or Self-determination, Perfect Justice, Universal Harmony, the Goodness of God, Felicity Divine, and many other supreme ideals and supreme perfections of rational experience and thought, are all of them forms of Being absolute, constituting an Overworld, a realm Superrational." [1]

Unfortunately Keyser has not told us any of the details of this Overworld. To debate its existence is vain. It is not only demanded by the rational, but required also by our aspirations. "In some sense, whatsoever quickens, lures and sustains, exists." [2] As to its nature, we are left more or less completely in the dark. Since it is superrational we are not entitled to ask that it be described. All that we can know is that it differs from the rational in the same way that a perfect lever differs from actual levers, and a circle differs from the series of polygons. But there seems to be no principle according to which the nature of this difference can be ascertained. Hence we cannot conclude, for example, from the nature of human knowledge to the nature of Omniscience. But we can argue that Omniscience *need not* be the same as finite knowledge, hence the fact that human knowledge implies a realm of the unknown within which it operates is in no way incompatible with true Omniscience. Beyond this we cannot go in the penetration of the Overworld. For the rest we must remain satisfied with the fact that it sheds "a mystic radiance like the 'obscure clarity that falls from the stars.'" [3]

The most interesting feature of Keyser's position is the way in which he endeavors to follow out the hints which the realm of obvious data offers of something beyond itself. Phenomenalism insists that the given should be limited strictly to that which is clearly given. But what is one to do with hints? Is the fact that a datum suggests something beyond itself part of that datum? The way in which a series of elements suggests its own limit is quite compelling. But

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[1] *Ibid.*, p. 73.

[2] *Ibid.*, p. 75.

[3] *Ibid.*, p. 74.

should this limit be included within the realm of data by virtue of having been suggested in this forceful way? An extreme phenomenalism would say, no; a moderate phenomenalism would be in doubt; a realism would say, yes. Keyser deserves credit for having attempted to make as precise as possible the character of the suggestion. Certainly a more critical analysis of the notions of series and limits would help to throw the problem in its proper light. But this is not the place to make that analysis. Keyser's formulation at least outlines the problem, and suggests the direction for further consideration.

What may be said, in conclusion, as to the general character of these solutions to the problem of reality? They are hardly such as to impress a critical mind, and one might easily condemn them to oblivion on these grounds. But in taking this attitude one should note two facts with reference to them. The first is that they are commonly put forward merely as speculative conclusions, having a greater rather than a lesser degree of probability; they are not final solutions but merely somewhat plausible suggestions. They must, accordingly, be estimated from this point of view. The second fact is that they do suggest further problems of a more critical nature. It seems quite clear that the conclusions considered in the foregoing pages can be either more firmly established or greatly weakened by a more careful analysis of the scientific knowing techniques in general, and of symbols in particular. Hence recognition should be made of the fact that the problem of the nature of reality, as the other speculative problems, rests upon the more critical problems of the philosophy of science. It seems likely, therefore, that the problems of the immediate future are the critical rather than the speculative ones. Fortunately, the attention of investigators has been increasingly directed in recent years to problems of formal logic and the general theory of symbols; to problems of actual scientific methodology; and to problems of the foundations and interrelations of the sciences. These offer great promise for the future.

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