Science and Civilization

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Science and Civilization

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Publisher's Note

For bibliographical reasons, Professor Robert C. Stauffer, chairman of the committee responsible for this book, has been designated editor. Listing volumes of essays written under separate authorship presents problems to bibliographers which do not easily lend themselves to practical solution. The Press, therefore, feels that scholars will be grateful for a simple entry under which this book may appear in files, catalogues, and bibliographies.

Preface

The conflict between the demands of narrow specialization and broad understanding poses a tremendous problem for the educated man living in our present complex technical society. Among today's specialists the scientists have achieved most spectacular successes by their intensive concentration of effort upon isolated problems. It would be futile to deplore their specialization, and it would be unjust to forget that scientists as a group are becoming increasingly concerned over the general social implications of their individual discoveries. Nevertheless specialized research demands interpretation—not only of its technical implications but of its general implications as well. And so modern science, which to the layman so often seems a conglomeration of esoteric detail or even a variety of magic, can achieve its full potential value only when it is made understandable by extensive and intelligent interpretation.

The understanding of science in all its major aspects, a problem as important to the specialist as to the layman, demands the fusion of many different approaches. With this in mind the History of Science group at the University of Wisconsin, with the generous support of the University's Centennial Committee, invited a group of scholars to discuss aspects of the problem of science and civilization, one of the symposia held in celebration of the centennial year.

Our speakers were invited to consider the problems of science from their own special vantage points; philosophy, history, physical science, biological science, and social science offer their own special insights into the problem of the relations between science and civilization.

No single discussion can do more than expose some of the implications of such a problem, but our symposium is only one of many. Our very title was borrowed from the excellent conference which Francis Marvin organized for the Sixth Unity History School in England in 1922. This volume is to be regarded as one contribution to a subject which merits continual and extensive cooperative study.

In behalf of the History of Science group, I should like to express our appreciation for all the generous assistance which made this symposium possible, and to acknowledge the cooperation of the symposium committee, the members of the discussion panels, the principal speakers, and the Division of the Natural Sciences of the Rockefeller Foundation, whose grant made it possible to print this book.

The Wisconsin History of Science group includes Erwin H. Ackerknecht, Marshall Clagett, Robert C. Stauffer, and George Urdang. The other members of the committee for the symposium, Science and Civilization, were Paul Farmer, William W. Howells, Aaron J. Ihde, Lowell E. Noland, Albert G. Ramsperger, and Thompson Webb, Jr.

R.C.S.

Foreword

Back in the summer of 1932, in an old town of southern France, I heard nightingales sing for the first time. As I sat at the open window of my room in the station hotel listening in rapture to their song from an adjoining grove of trees, there came the crash of a train entering the station, which drowned out their music. Yet it still rang in my ears, and, after the clatter was over and the train had gone, and I could actually hear the nightingales again, they were exactly at the point which they should have reached by then. They had been singing all the time!

That is the way with learning. You may not be able to sense it for political propaganda and economic effervescence, but it goes right on. The University of Paris has managed to keep going ever since the twelfth century, through all sorts of change and development in government, except for a few years when it was snuffed out by the French Revolution. The University of Bologna went on for a like period regardless of communal movements and papal temporal claims. The Universities of Oxford and Cambridge for almost the same length of time survived most varied constitutional and religious vicissitudes. The University of Wisconsin has come safely through a century of state legislatures and popular elections. And one strongly suspects that the chief factor enabling the universities to do this was what we have already indicated—

minding their own business and attending to higher learning.

An old fragment from a lost play of Euripides runs: "Kill! Kill the all-wise, the nightingale, the unoffending Muse." Yes, you can kill one nightingale, or maybe more, but nightingales will still go on singing. And so we have faith that higher learning can stand on its own feet, and that, to borrow a metaphor from Boethius, its head reaches above the stars. In any case, I desire once more to congratulate this university upon celebrating its centennial by this symposium, devoted not to citizenship or public service, but to science and civilization!

Some recent university presidents seem to believe that a main function and chief concern of an institution of higher learning is defense of democracy and of our American mode of life. But some of us still cling to the old-fashioned notion that an institution of higher learning should attend to its own objective, which is—higher learning. And that not merely in a sense of advanced specialization but of a cast of thought, a temper of mind, and a method in research raised high above present conditions and even present exigencies. Alexander Pope, it is true, wrote:

> Learning and Rome alike in empire grew
> And arts still followed where her eagles flew.
> From the same foes, at last, both felt their doom,
> And the same age saw learning fall and Rome.
> With tyranny, then superstition joined,
> As that the body, this enslaved the mind.

I certainly would not tie up learning with tyranny. But neither would I tie it up with Roman or with any other form of government. I would not mingle it with superstition—or with any other ideology, or anti-ideology. Learning, pure and undefiled—that is all!

Lynn Thorndike

Columbia University
January 13, 1949

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Aristotle and the Origins of Science in the West

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When Aristotle examines the scientific work of his predecessors before formulating a problem or forming hypotheses for its solution, he frequently finds an abundance of data and speculation but seldom an adequate conception of the problem or an understanding of its implications. The history of science, as Aristotle recounts it, is in one sense long and in another sense short. Aristotle finds anticipations of fundamental scientific truths in myths and in poetry, in common opinion as reflected in popular sayings and the ordinary significance of words, in the arts of husbandmen and artisans, and in the doctrines of the barbarians. Yet he could discover no consideration of even the most basic problems of method prior to the reflections of Socrates and Plato and no use of scientific method in the investigation of nature prior to the work of Leucippus and Democritus. Most Greeks who speculated on the origins of intellectual history found the beginnings of philosophy among the Greeks, for they argued that they had transformed whatever they borrowed from barbarian sources, even in mathematics and astronomy, giving theoretic universality for the first time to rule-of-thumb operations and partial empirical generalizations; the very word "philosophy," as Diogenes Laërtius restated the argument, could not be translated into other languages. Later historians have learned from Aristotle to trace the history of science from the atomism of Democritus or the dialectic of Plato, while holding suspect his report of the longer past and tempering or ignoring his own claim to improvement of method or novelty of conclusion.

The effort to judge Aristotle's contribution to the advancement of science in the West faces the same difficulties today that Aristotle encountered in his estimation and use of the work of his predecessors. The history of science is still short in modern versions for much the same reason that persuaded Aristotle that it was no older than the generation before his own work. The scientist who is at work on a particular problem or on a specific subject can seldom find relevant data, appropriate methods, or plausible theories further back than the previous generation. Yet as hypotheses are adjusted and the general scheme of concepts shifts its form and coherence, vistas open in all directions, down the history of earlier inquiries as well as across subject matters to speculation in other fields. The history of science is involved in two basic dialectical dilemmas, one in the oppositions found in its subject matter, the other in the oppositions found in theories concerning "science" and "history." In the first place, the history of science is the account either of the sequence of approximations and anticipations of later truths or of the evolution of truth from error: in the process by which inquiry approximates through the ages to the more adequate formulations and and resolutions of problems that constitute the present stage of scientific advance, the contribution of earlier workers is found either in their statement of portions of truths or in the stimulation their errors afforded to the discovery of truth. In the second place, since the "present" stage, whenever the historian works, is never a final accumulation of absolute truths, the history of earlier stages is determined by a theory concerning the nature of science, usually in contradiction with other contemporary theories or with theories later verified. The numerous and radical differences in expert judgments concerning the adequacy of Aristotle's observations of fact and theoretical constructions and concerning the stimulating or inhibiting character of his insights or errors reflect differences in conceptions of history and of science which affect not only the interpretation of the processes of past developments in science but also the formulation of present theories. The influence of Aristotle on the development of methods and the formulations of doctrines in science cannot be understood apart from an appreciation of his influence on the method of interpreting the development of scientific procedures and theories.

The two lines of scientific development which Aristotle found in the work of his predecessors led to opposed errors, according to his account of them. The one line culminated in Socrates and Plato, who in their effort to explore the method of arguments placed an excessive reliance on formal causes. The other line culminated in Leucippus and Democritus, who in their effort to save phenomena gave an excessive importance to material causes. Aristotle says that Socrates was the first to raise the problem of inductive arguments and universal definitions, and he frequently acknowledges, even when, as in the case of "space," he disagrees with the definition and the method by which it is established, that Plato alone of his predecessors tried to say what things are. Yet Plato's concern with definitions led him, according to Aristotle, to try to satisfy the requirements of definition by setting up Ideas as eternal things intelligible to reason and separate from the changing things of sense and, in so doing, to reduce all sciences to mathematics and all methods to dialectic. Aristotle says that Leucippus tried to construct a theory in harmony with sense perception and consistent with motion and the multiplicity of things and that Democritus alone can be excepted from criticism for lack of method in analyzing the problems of generation and corruption. Yet Democritus, like all other philosophers, "lazily" omitted consideration of the causes of motion; according to Aristotle, he therefore failed to account for change and his concern with phenomena reduced all method to the determination of the interrelations of particles in motion and all sciences to mathematics. Where Plato had made things into Ideas in order to separate in being what was changeless in knowledge, Democritus reduced the intellect to the soul which could be explained, like all other phenomena, by the motions underlying the data of sense perception.

Aristotle sought a scientific method which would account, as the methods of his predecessors had not, for the processes of change. Such a method would take into consideration the efficient causes of motion, which Aristotle thought his predecessors had barely suspected, and it would therefore avoid the reduction of things either to eternal Ideas or to matter in motion. It would be a logic of arguments and causes which would seek the principles of change in the processes of nature rather than either a dialectic of ideas and arguments whose principles are eternal patterns unsuited to explain the beginnings of physical and biological motions or a methodology of configurations and motions whose principles are patterns of motions unsuited to supply any cause of motion other than the existence of prior motions. Finally, his method would be diversified to the diversity of scientific problems, and the sciences would not be reduced to a single science either by a mathematics of dialectical search for a single principle or by a mathematics of composition of mechanical motions.[1]

Aristotle's account of the oppositions of the doctrines of his predecessors serves as a dialectical basis for the exposition of his own conception of scientific method. It is not an adequate historical account of the position of either Democritus or Plato; yet despite the abundant evidence of its inadequacy in the dialogues, Plato is often read as Aristotle presents him and, despite difficulties suggested by the fragments, Democritus is defended in the terms in which Aristotle refuted him. The distinctions which he constructed to indicate what Democritus and Plato might be said to have contributed to his own conception of sci-

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[1] See Richard McKeon, "Aristotle's Conception of the Development and Nature of Scientific Method," *Journal of the History of Ideas* (1947), 8:3–44.

ence have been used as the grounds of opposed arguments turned against Aristotle more frequently than as preparations for a third middle position, and the history of science has been presented in terms of either pole of the opposition he formulated more frequently than in terms of the resolution he sought to establish. His metaphysics undertook an analysis of causes to obviate the simple reduction of things to eternal ideas or to matter in motion; his scientific method was adapted to seek causes of change rather than changeless patterns perceived by the mind or patterns of change perceived by the senses; his logic depended on the use of causes as principles and middle terms of demonstrations rather than on the use of principles of division to analyze classes or bodies. The manner and the pervasiveness of his influence is illustrated not only in the doctrines of followers, but also in the prevalence of histories which present the development of science and philosophy in terms of an opposition between materialism and idealism, empiricism and rationalism, mathematics and mechanics, and which treat Aristotle's scientific inquiries as an imperfect approximation to, or an erroneous deviation from, one or the other part of the opposition. When doubts are raised concerning whether his methods and theories contributed to the advance of science or inhibited it by enslaving men's minds to an unproductive dogmatism, the resolution of the question as well as the form in which it is posed are borrowed either from what Aristotle called the physical method of Democritus or his version of the dialectical method of Plato. Leibniz, who was learned in the history of philosophy, includes the third term in his account of the choice he made with careful deliberation, early in his career as a philosopher, between Aristotle and Democritus, between substantial forms and atomism, between dynamism and mechanism: he decided for mechanism, which led him to the study of mathematics and thence to metaphysics and the monad. Scientists and historians less learned in the history of thought than Leibniz have criticized Aristotle for opposed reasons. His logic is too formal for pertinent application to scientific problems, yet it is insufficiently abstracted from subject matter to achieve perfect generality; the theoretic sciences are separated too sharply from the practical, yet they are vitiated by the intrusion of final causes. His sciences are constructed on the model of art and nevertheless suffer from neglect of the resources of the mechanical arts, while art is improperly conceived as an imitation of nature and also as an intellectual virtue. But most important of all, his scientific method is directed to classification rather than to measurement, and yet it proceeds by analogies and proportions rather than by observation of facts. In either form his influence served to cut short the fruitful application of mathematics to phenomena and set inquirers classifying for centuries, until the influence of Plato and Democritus in the Renaissance and the seventeenth century set them measuring again. The errors of Aristotle in science have been derived from his doctrine of forms which concealed from him the advantages of the mathematical analysis of matter in motion made possible by the atomic theory, or from his misunderstanding of the Platonic forms which turned him from the mathematical method developed in the Academy.

The influence of Aristotle's distinctions on the interpretation of the history of science must be understood before a sound judgment can be formed of his contribution to science, for the meaning no less than the validity attributed to his doctrines depends on the conception of science and scientific method assumed in the interpretation. Aristotle's conception of science depends on rejecting the unique determination of scientific method by reference either to an order attributed to facts or to an order discovered in ideas. His criticism of Democritus pointed out the impossibility of adherence to facts without examination of reasons and sources of order; his criticism of Plato expressed his opposition to speculation inadequately controlled by the requirements of facts. He recognized that Democritus had made excellent though limited use of method, since fidelity to facts will resolve any problem except the problem of how such fidelity is achieved; and he recognized that Plato had made his method general though uncontrolled, since a theory may be elaborated dialectically to clarify any problem except the problem of how the theory applies to facts. He therefore set himself the task of relating theories to facts, testing the adequacy of causes to explain processes and grouping facts and processes according to causes. The differentiation of kinds of subject matter and the interrelation of theories bearing on common subject matters led him to distinguish the sciences from each other. He argued that mathematics could be applied to the measurement of physical motion precisely because the surfaces of bodies have other properties than those of mathematical planes which are known by abstraction from physical surfaces, and that the parts of bodies can be counted precisely because they are not numbers. The analysis of local motion requires a conception of physical place which has other properties than relative position, and the reduction of physics to mathematics would lead to the identification of physical place either with extension or with matter. He argued that the theoretic sciences are distinct from the practical sciences, in spite of the fact that science is an intellectual virtue, because the requirements of truth are distinct from the requirements of effective action or moral habituation, and he differentiated science from art, in spite of the analogies he was fond of drawing between the object of art and natural objects and between the constructions of the artist and the proofs of the scientist, because the exploration of natural things is governed by other conditions than those which determine the successful production of artificial things. Finally, the differentiation of sciences according to subject matter, principles, and methods, which resulted from the investigation and differentiation of phenomena, led him in turn to the differentiation of the logic of inquiry and proof applicable to all arguments from the scientific methods of the various sciences.

This elaborate series of distinctions, far from involving him in a bifurcation of nature, was designed to avoid the partial explanation it exposed in the doctrines of his predecessors, and his rejection of the method of any science, including mathematics, as the unique method of all science, did not face him with the dilemma of choosing between measurement and classification. Even casual reading in Aristotle's scientific works is sufficient to expose the fiction that his scientific theories presuppose a universe of genera and species arranged in neat hierarchical order. The distinctions of logic are devices of inquiry for Aristotle, not metaphysical categories, and even in the biological sciences, from which he is sometimes supposed to have derived his notion of a classification of nature, he sets up no classification of animals, in spite of his elaborate distinctions of kinds, parts, and functions. D'Arcy Thompson, who has made very valuable contributions to our understanding of Aristotle's biological sciences, leaves little ground for doubting that Aristotle's biological writings do not depend on such a classification:

> Many commentators have sought for Aristotle's "classification of animals"; for my part I have never found it, and, in our sense of the word, I am certain it is not there. An unbending, unchanging classification of animals would have been something foreign to all his logic; it is all very well, it becomes practically necessary, when we have to arrange our animals on the shelves of a museum or in the arid pages of a "systematic catalogue"; and it takes a new complexion when, or if, we can attain to a real historical classification, following lines of actual descent and based on proven facts of historical evolution. But Aristotle (as it seems to me) neither was bound to a museum catalogue

nor indulged in visions either of a complete *scala naturae* or of an hypothetical phylogeny.[2]

Aristotle's conception of science requires the differentiation of functions and causes in order to permit the isolation and separate examination of phenomena which, though intricately interrelated in fact, must be treated according to proper principles to differentiate essential from adventitious traits. According to this conception of science he distinguished substance from accident, but the classes and kinds of subject matter by which the sciences were distinguished from each other are determined not by simple genera of substances but by kinds of function and cause, each of which determines classes relative to processes. Such functional differentiations necessarily cut across classes and determine no single classification of natural objects, or plants, or animals. In his collections of materials in the natural histories, in his examination of the theories of other men and his adjustments of theories to facts, in his exploration of the intricate interrelations among the applications of the different sciences to the same things, and in his formulations of the devices of methodological and logical analysis, Aristotle is engaged in a connected series of continual experiments in the relation of facts and theories.

Aristotle occupies a prominent and important place in the history of science, therefore, when the history of science

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[2] D'Arcy Wentworth Thompson, "Natural Science: Aristotle," *The Legacy of Greece*, ed. R. W. Livingstone (Oxford, 1922), 158.

is an account of the interrelations established between the theories constructed to resolve problems and the facts they are adduced to explain. His contribution to science is slight when the history of science is a record of either the successive stages in the development of the atomic theory and the application of sensational empiricism or of the progressive elaboration of devices for the universal application of mathematics and the imaginative construction of mathematical models. Thus, when Whitehead identifies the scientific mentality as "a vehement and passionate interest in the relation of general principles to irreducible and stubborn facts,"[3] he finds the beginnings of the short history of this attitude of mind in the seventeenth century. Whitehead is convinced that, with very few exceptions, the Greeks had not attained to the complete scientific mentality: they had genius, but "their genius was not so apt for the state of imaginative muddled suspense which precedes successful inductive generalization."[4] Aristotle is one of the exceptions, and Whitehead adds that it is a large exception, in spite of his conviction that "in Plato the forms of thought are more fluid than in Aristotle" and therefore more valuable.[5] Whether or not one agrees with Whitehead's version of the historical development of ideas in the West—with his characterization of Greek cosmology as dramatic or with his account of the emergence of modern science as an anti-intellectual reaction, based on the

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[3] Alfred North Whitehead, *Science and the Modern World* (New York, 1925), 3. [4] *Ibid.*, 10.

[5] Whitehead, *The Concept of Nature* (Cambridge, 1926), 17.

weakest side of the philosophy of Aristotle's successors, to the medieval confidence in reason—his vigorous presentation of the interplay of theory and fact, faith in rational order, and interest in natural occurrences for their own sake serves to isolate the basic problem of science which Aristotle sought in opposition to Democritus and Plato and to explain the sense of familiarity which his procedures and analyses afford even when his conclusions depart from those certified by modern scientific methods.

The long history of violently conflicting estimates of the value of Aristotle's scientific doctrines and of the importance of his influence, then, reflects distinctions which Aristotle made among methods in preparation for the statement of his own, and the judgments pronounced on the adequacy of his methods in determining facts or constructing theories are significant in terms of that debate concerning the opposed theories of science. In the biological sciences, where the effort to reduce phenomena to formulae or to matter has been least successful, and where the description and explanation of functions still seem to require the joint use of what Aristotle would have called material and final cause, or necessity and purpose, scientists have continued longest and most generally to appeal to Aristotle's principles and to praise his methods and results. Cuvier expressed amazement at the accuracy and system of his observations: "In Aristotle everything is prodigious; everything is colossal. He lived but sixty-two years, and he was able to make thousands of observations of extreme delicacy, the accuracy of which the most rigorous criticism has never been able to impeach."[6] The *History of Animals* is the object of Cuvier's particular praise: "I cannot read this work without being ravished with astonishment. Indeed, it is impossible to conceive how a single man was able to collect and compare the multitude of particular facts implied in the numerous general rules and aphorisms contained in this work, and of which his predecessors never had any idea. . . . The *History of Animals* is not properly a zoology, that is to say, a series of descriptions of various animals; it is rather a sort of philosophic anatomy, in which the author treats of the generalities of organization presented by various animals, in which he explains their differences and resemblances, founded on a comparison of their organs, and in which he lays the bases of grand classifications irreproachable in accuracy."[7] Charles Darwin is scarcely less extravagant in his estimate of Aristotle's achievements: "Linnaeus and Cuvier have been my two gods, though in very different ways, but they were mere schoolboys to old Aristotle."[8] D'Arcy Thompson not only praises the number and accuracy of his observations and his recognition of the great problems which are still the problems of biology today, but also identifies in the works of Aris-

[6] Baron Georges Cuvier, *Histoire des sciences naturelles* (Paris, 1841), I:132. Quoted by G. H. Lewes, *Aristotle: A Chapter from the History of Science, including Analyses of Aristotle's Scientific Writings* (London, 1864), 154.

[7] Cuvier, *Histoire des sciences naturelles,* 146; Lewes, *Aristotle,* 154.

[8] Charles Darwin, *Life and Letters* (London, 1888), 3:252.

totle fishes that have only recently been rediscovered, structures that have only lately been reinvestigated, and habits only of late made known.[9] Yet even in the biological sciences George Henry Lewes, who regards final causes as a lingering unscientific remnant of the subjective method, which draws all explanations of external phenomena from premises directly suggested by consciousness, sees Aristotle's biological investigations as vitiated by his failure to employ the objective method. He grants that Aristotle may properly be called the father of the Inductive Philosophy, since he first announced its leading principles and in contrast to Plato appealed to sense perception rather than to intuition. But Aristotle failed to employ the method he taught, and Lewes can find little accurate knowledge in the whole sweep of his survey of biological phenomena—no single anatomical description of the least value, no indication even of knowledge of the muscles, the nervous system, or the difference between veins and arteries, many statements wholly without foundation, sometimes even without the superficial appearance of it.[10]

In the physical sciences, where the establishment and extension of classical mechanics was based on concepts radically opposed to those of Aristotle's analysis of motion, there has appeared, conversely, a tendency to see in contemporary physics a partial return to Aristotle's doctrines, as when Whitehead detects a vague intimation of

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[9] D'Arcy Wentworth Thompson, "On Aristotle as a Biologist," Herbert Spencer Lecture (Oxford, 1913).

[10] Lewes, *Aristotle*, 108–114, 156–159.

the Aristotelian substance in the modern scientific concept of matter,[11] and a disposition to appeal to Aristotle's concepts to clarify the paradoxes of quantum mechanics, as when Riezler explores the implications of the modern concepts of motion and organism.[12] The place of Aristotle in the history of the development of the practical sciences undergoes similar shifts, which reflect differences in the interpretation of Aristotle's method. Zeller, who considers Aristotle's philosophy and methods continuations in all essentials of the lines which Socrates and Plato opened out, pronounces Aristotle's *Politics* "the richest treasure that has come down to us from antiquity, and, if we take into account the differences of the times . . . the greatest contribution to the field of political science that we possess." A. E. Taylor, who argues that Aristotle's inferiority in mathematics and his dislike for mechanical ways of explaining facts puts him at a disadvantage in astronomy and physics, as compared with Plato, and who finds that he shows even in the biological sciences an unfortunate proneness to disregard established fact when it conflicts with the theories for which he has a personal liking, concludes that "no Aristotelian work is quite so commonplace

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[11] Whitehead, *The Concept of Nature*, 19: "Matter, in its modern scientific sense, is a return to the Ionian effort to find in space and time some stuff which composes nature. It has a more refined significance than the early guesses at earth and water by reason of a certain vague association with the Aristotelian idea of substance."

[12] Kurt Riezler, *Physics and Reality: Lectures of Aristotle on Modern Physics* (New Haven, 1940), 35–39.

in its handling of a vast subject as the *Politics*" and that Aristotle's "interest in these social questions is not of the deepest."[13] Historians of logic have disagreed with Kant's judgment that it has been unnecessary to retrace a single step in the establishment of a secure method since Aristotle, not merely because of increase in historical information about logic, but because of the influence of modern theories of induction and the assimilation of logic to the methods of mathematics. Like variations traceable to like causes can be found in the place given to Aristotle's doctrines and the interpretation of their meanings in histories of psychology, mathematics, and other particular branches of science. Moreover, historians are sometimes led by these changing judgments to differentiate Aristotle's skill and influence in the different sciences or to separate the fate of his doctrines and method from the continuing acceptance of his principles when his doctrines and method have been discarded. Boutroux thus distinguishes Aristotle's influence in the moral sciences (where Boutroux finds Aristotle's ideas more than ever operative) from his influence in the mathematical and physical sciences (where modern developments seem to have little in common with the natural philosophy of Aristotle), yet he attributes some importance to Aristotle's work even in the latter fields, not only because Aristotle contributed the point of departure for many scientific inquiries in those fields, but also be-

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[13] Eduard Zeller, *Aristotle and the Earlier Peripatetics*, translated by B. F. C. Costolloe and J. H. Muirhead (London, 1897), 2:288; A. E. Taylor, *Aristotle* (2nd ed., London, 1919), 70–71, 118.

cause "many of his principles are still perfectly recognizable in the spirit of contemporary science."[14]

These differences of opinion, when they are not the result of simple ignorance or prejudice on the part of the historian, indicate peculiarities in the development of scientific knowledge which are particularly important in treating a controversial figure like Aristotle who not only took part in the controversy but influenced the statement of its oppositions. The value of a contribution to the cumulative progress of science is not to be found wholly in the discovery of facts which continue to be acknowledged or the statement of theories which are long or widely accepted. Even the momentary success of a theory, however dynamic its concepts and suggestive its formulations, has the effect of putting a stop to inquiry and speculation. The value of the statement of facts and the formulation of problems depends also on their fruitfulness in suggesting further inquiry; yet the discovery of related or more fundamental facts will often lessen the accuracy of, or wholly discredit, the original statements, and the establishment of a new theory will often overturn the theory that prepared the way or make it at best an approximation.

When the history of science is traced in terms of data pertinent to specific problems it is extremely short, and ancient scientists have little or no relevance to the problems of modern science. They did not observe the characteristics crucial to modern forms of the problems which are

[14] Emile Boutroux, *Etudes d'histoire de la philosophie* (4th ed., Paris, 1913), 205–206.

concerned with phenomena with which they were familiar; they did not have the instruments indispensable for knowledge of other phenomena central to modern science; they had not developed the fundamental laws or the mathematical analyses essential to inquiry in many problems; and, in particular, they were without the resources of technological developments which have transformed experimental procedures and incredibly increased available data and information. The few cases in which the facts they observed are still relevant or important, such as some of Aristotle's biological observations, are the results either of renewed study of the ancient documents or rediscovery of the natural phenomena. When, on the other hand, the development of science is traced in terms of the sources of ideas and the recurrent reinterpretations of basic concepts—time, space, and matter; mechanism, order, and organism; structure, pattern, and function—its history is long, for many of the puzzles of the later stage of scientific advance are recognizable variants of the paradoxes of earlier scientists, the suggestion of new theories often comes from forgotten analyses in another branch of inquiry, and new hypotheses sometimes employ the terms and relations discarded in the refutation of an earlier hypothesis.

These recurrences of basic problems and echoes of fundamental theory take the form of either a return to outlines of an earlier analogy or a new use of earlier distinctions. The analogical sweep of Plato's dialectic, moving from art to science, from virtue to knowledge, from becoming to being, from material to structure by way of biological function and psychological process to creative act, approximates in the eyes of many scientists and philosophers the formulae of later theories without anticipating their factual content. The richness in Plato's ideas thus constitutes their scientific value for Whitehead: "Plato's guesses read much more fantastically than does Aristotle's systematic analysis; but in some ways they are more valuable. The main outline of his ideas is comparable with that of modern science. It embodies concepts which any theory of natural philosophy must retain and in some sense must explain."[15] The indivisible atoms of Democritus and the suggestion that he employed an infinitesimal method permits like extensions and analogies to later forms of atomism which are based on data Democritus never envisaged and developed on assumptions inconsist-

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[15] Whitehead, *The Concept of Nature*, 18. See *Process and Reality* (New York, 1929), 142–143: "The *Timaeus* of Plato, and the *Scholium* of Newton . . . are the two statements of cosmological theory which have had the chief influence on Western thought. To the modern reader, the *Timaeus*, considered as a statement of scientific details, is in comparison with the *Scholium* simply foolish. But what it lacks in superficial detail, it makes up for by its philosophic depth . . . The full sweep of the modern doctrine of evolution would have confused the Newton of the *Scholium*, but would have enlightened the Plato of the *Timaeus*." See *ibid.*, 145: "Newton would have accepted a molecular theory as easily as Plato, but there is this difference between them: Newton would have been surprised at the modern quantum theory and at the dissolution of quanta into vibrations; Plato would have expected it."

ent with his formulation of the basic doctrine.[16] Aristotle's conception of science is developed in explicit opposition to these two views, both of which seemed to him to involve an illicit reduction of all sciences to one: his own analysis achieves universality and fluidity by a multiplicity of systematically interrelated distinctions. The value of his work in science is to be found in the effects of that analysis: precision of statement, richness of factual content, and suggestiveness of distinction in application to new data. The vulnerability of the Aristotelian sciences to criticism is found in the same traits, for, whereas it is difficult to contradict a metaphorical or mythical statement of fact or to deny the analogy of one process to another or to refute the reduction of processes to more fundamental motions, it is easy to indicate the error of a literal statement when it is false or to dismiss it as unimaginative when it is true.

The peculiar importance of Aristotle's contribution to science lies in the scope and ingenuity of this effort, made early in the history of science, to find literal distinctions among terms, phenomena, functions, processes of inquiry, grounds of proof, and sciences. The signs of this effort are on every page of Aristotle's scientific writings: he pauses repeatedly to enumerate the meanings of a term before choosing the sense in which he will employ it; he differ-

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[16] See Whitehead, *Process and Reality*, 471: "Mathematical physics translates the saying of Heraclitus, 'All things flow,' into its own language. It then becomes, All things are vectors. Mathematical physics also accepts the atomistic doctrine of Democritus. It translates it into the phrase, All flow of energy obeys 'quantum' conditions."

entiates aspects and situations of things, functions of organisms, and causes of processes before treating the problem he has isolated; he analogizes and distinguishes the problems and methods of the various sciences that might have bearing on a related set of facts, or the properties in the same set, or the problems that might differentiate the principles and methods of related sciences. The whole of learned speech shows the marks of this effort. The terms of the sciences—even when they are applied to subject matters and problems concerning which the Greeks had no intimation—and the fundamental distinctions by which new terms are defined, refining or cutting across old distinctions, still reflect the influence of the Aristotelian schematism by reassertion as frequently as by denial. Substance, accident, substrate, essence; form, matter, actuality, potentiality, act, power, process, function, change, motion; generation, alteration, increase, locomotion; element, principle, cause; category, proposition, syllogism; induction, deduction, definition, proof; disposition, habit, virtue, character; imitation, practice, learning; experience, art, science—Aristotle did not invent the set of terms of which these are a brief selection nor did he in all cases first select the properties by which they are defined or first observe the phenomena to which they are applied. On the contrary he meticulously searches out earlier uses for his terms and saddles his predecessors with definitions and distinctions, claiming for himself only the accomplishment of having completed the list or systematized the schemata. Nor did he succeed in imposing the meanings he chose or the relations he constructed on the terms used by later scientists. New basic terms have been defined, frequently by denying the distinction, or the utility of distinguishing, between substance and accident, subject and predicate, form and matter, the kinds of causes, or motions, or faculties. The denial of a distinction has wider significance if the distinction has been made clearly and if its implications have been carefully elaborated. Moreover, distinctions are avoided usually by analogies or reductions which presuppose distinctions among the subjects in which the similarity or underlying identity is found, and which in turn are usually extensions of qualities found in a stricter sense in one of the groups which is brought into a larger whole by the analogy or reduction. The interrelations of the distinctions which Aristotle drew are so variegated and so close that the discussion of them moves from the terms used in construction of theory and application to fact to the interrelations among the sciences involving distinctions between theoretic and practical, between the sciences and the arts, and between the sciences and the methods of inquiry and proof, until it centers in the conception of science which Aristotle constructed by distinguishing it from two opposed conceptions so suggestively that both have been used in a succession of dialectics and mechanics as the theoretic grounds for rearranging or denying his distinctions.

The vitality of these distinctions as sources of positive knowledge and as starting points of inquiry, rather than the adequacy of Aristotle's scientific theories or the accuracy of his observations and the comprehensiveness of his accumulations of data, constitutes his important contribution to the development of science. For his distinctions are not simple classifications of facts, or of ideas, or of terms; they depend rather on the varieties of interrelation of ascertained facts and established principles—the relations of part and function; of definition and demonstration; of experience, art, and knowledge; of construction, action, and thought; and, in general, of all the lines that cut across the development of science, its subject matter, its ideas, its statements, and its uses. This peculiarity of Aristotle's scientific analysis keeps the problem of the relation of general principles to irreducible and stubborn facts prominent in the discussion of scientific problems wherever his influence has been felt. Platonism may degenerate into pregnant mysticism and theory uncontrolled by reference to fact. Kant repeats Aristotle's criticism of this tendency: "The light dove, piercing in her easy flight the air and perceiving its resistance, imagines that flight would be easier still in empty space. It was thus that Plato left the world of sense, as opposing so many hindrances to our understanding, and ventured beyond on the wings of his ideas into the empty space of pure understanding. He did not perceive that he was making no progress by these endeavours, because he had no resistance as a fulcrum on which to rest or to apply his powers, in order to cause the understanding to advance."[17] The exploration

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[17] Immanuel Kant, *Critique of Pure Reason*, translated by F. Max Müller (New York, 1919), Introduction, 4.

of nature as revealed in sense perceptions may, on the contrary, become so involved in the processes of tracing natural phenomena and explaining them that it loses insight into the nature of its own inquiries and comes to fancy them constituent parts of the data of experience. It is the virtue of the Aristotelian analysis, whether applied to the data of the sciences or to the nature of the sciences themselves, to identify and relate theory, which may seem to account for everything but which accounts for nothing precisely unless brought to specific application, and facts, which may seem to force their proper character on the attention of the inquirer without need of theory but which are unintelligible without insight into the reasons that govern proposed explanations. The effect of the distinctions which Aristotle introduced in elaborate system into the discussions of science and philosophy is to bring periodically to the forefront of attention the character of the abstractions to which we have grown accustomed in our explanations, the peculiarities of their interrelations, and the possibilities of systematic changes in them. In this fashion precise formulations have contributed impulse and many of the elements to that ferment of thought in which new systems of explanation have been suggested, often by revealing the application of dialectic to experience or by developing the theory of empirical investigation.

The contribution of Aristotle to the origins of science in the West must therefore be stated on three levels. In the first place, and in the proper sense of his contribution to science, he discovered facts and he constructed theories that had profound influence on later scientists. But his facts are now familiar or forgotten or false, for so far as they have been influential they prepared the way for better observations, after which they are crude and superficial, and his theories have either been superseded by other theories, suggested by them or constructed in opposition to them, or they continue as generalizations in which only the sympathetic critic will see a likeness to the subtler formulation required in application to data later made available. In the second place, his terms and ideas, his distinctions and theories recur and are influential, in a vague or derived way, in later investigation and discussion. The Platonic dialectic is likewise broadly suggestive of later mathematical analysis and the Democritean atomism lays general lines still regulative of inquiries into the structure of matter. But the manner in which the ideas of Plato and Democritus still influence or awaken echoes in modern thought is opposite to the manner in which remnants of Aristotelian terminology and theory are influential or buried in our inquiries. For the ideas of Plato and Democritus were seminal—inclusive theories, suggestive analogies, sweeping reductions—whereas the peculiar contribution of Aristotle is the integrated system of distinctions on which his science is constructed, which laid basic principles and differentiations frequently employed in later inquiry, experimentation, speculation, and dispute. In the third place, Aristotle's philosophic generalizations force on the inquirer the realization that progress in science is achieved not merely by the meticulous observation and measurement of the facts or by the elaboration of theories and the application of principles, but also by the substitution of theories which interpret the facts differently and the comparison of schemes of explanation established in ordering our various types of experience. This influence of Aristotle assists, however acrimonious the disputes it has inspired, in returning philosophy to what Whitehead describes as its proper role "of harmonizing the various abstractions of methodological thought,"[18] and it is this instrument for purifying ideas and correcting their abstractness by recurrence to concrete experience, rather than any discovery he is thought to have made or any classification he may have imposed, that constitutes Aristotle's lasting contribution to science.

[18] Whitehead, *Science and the Modern World,* 25.

Some Unfamiliar Aspects of Medieval Science

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The theme of this paper is *Some Unfamiliar Aspects of Medieval Science*—or perhaps I should say "less familiar" rather than "unfamiliar." In either case the chief emphasis is upon the word "aspects." And I am thinking of that term especially in its astrological connotation, where it is used to express the varying relationships of the planets to one another. Those heavenly bodies, which Aristotle and medieval science thought of as ungenerated, incorruptible, and eternal, do not change, nor are their regular courses through the heavens subject to any deviation or alteration. But as they weave their ceaseless pattern against the firmament, their relative positions in the belt of the zodiac keep changing. If two planets are two signs apart, they are in sextile aspect; if three signs separate them, their aspect is quartile; if they are four signs or a third of the zodiac distant from each other, the aspect is trine. Similarly, most of the events and persons and writings with which we shall now concern ourselves are not new or even less familiar, but we may succeed in placing them in a relationship somewhat different from the traditional and customary line-up, or at least focus our attention upon them from a less familiar point of view.

Recently I have been engaged in the task of revising my *History of Medieval Europe*, and have found myself both captivated and puzzled by the old problem how two successive periods of history dovetail into each other, and that other question whether, and if so how, there can at the same time be marked progress in one field of human endeavor and apparent stagnation, or even retrogression, in another. And, if this too be so, whether this state of affairs is normal and to be expected, or whether it would be more usual to have parallel developments, all reacting upon and encouraging one another, in, say, art, literature, philosophy, education, and science.

Such problems are difficult to solve, such questions are hard to answer, because our knowledge of the past so often hangs on a single thread and depends upon what may be the purely accidental survival of a solitary manuscript. Discovery and publication of the Edwin Smith papyrus led to drastic revision of our estimate of ancient Egyptian medicine and surgery. How altered would be our picture of early Anglo-Saxon literature and early Teutonic life were it not for the poem *Beowulf,* extant in a single manuscript. There is only one manuscript of the work of John the Scot on predestination, only one—from the fourteenth century—of the official catalogue of the works of Thomas Aquinas, only one of the thirteenth century at Copenhagen of the *Causae et curae* of Hildegard of Bingen, only one of the *Chronicle* of Sulpicius Severus as against hundreds of manuscript copies of his *Life of St. Martin of Tours,* only one, and that in a defective and shortened form, of the very influential sixth-century Byzantine chronicle of John Malalas, only one of the *Summulae logicales* by William of Shyrwood, whom Roger Bacon preferred to Albertus Magnus. Only one of that treatise of which much has been made by both historians of education and historians of political thought, the *De recuperatione terre sancte* of Pierre Dubois. Only one of the *Defensor minor* of Marsiglio of Padua, which presents his case in such a different light from the *Defensor pacis*, for which it is almost an antidote. Only one of Pegolotti's *Practice of Merchandise;* only one of Simon of Phares' *Survey of the Most Celebrated Astrologers.*

The sole codex of the *Hortus deliciarum* of Herrade of Landsberg, a popular encyclopedia covering 324 leaves of vellum and containing 636 colored pen drawings, was destroyed in 1870 in the bombardment of Strassburg. On the other hand, the only copy of Muhammad ibn Musa al-Khowarizmi's Arabic adaptation of the *Geography* of Ptolemy is, or was until World War II at least, to be found at Strassburg. The fifteenth-century original of the Herbal of Benedetto Rinio, with illustrations by the Venetian painter, Andrea Amadio, is still preserved in the library of St. Mark's—where, too, is the single manuscript of the fifth- or sixth-century Greek dictionary of Hesychius of Alexandria. The recent publication for the first time from the sole manuscript of the late thirteenth-century Herbal of Rufinus has called for an even more drastic revision of previous assumptions as to medieval botany than the Edwin Smith papyrus suggested in the case of ancient Egyptian medicine and surgery.

I might go on and list many more examples of works which have reached us in a single manuscript copy and that sometimes very corrupt. But probably enough has been said to convince you of the possibility of the total disappearance of equally significant writings, whose recovery might revolutionize many an existing historical generalization. When we remember that we know of the great Iconoclastic Movement in the eighth century only from its bitter enemies, it is not implausible to feel that many an important trend in the history of medieval science may have gone unnoticed in our extant records.

With this preliminary caution, let us attempt some consideration of such problems as have already been suggested. Aristotle, I believe it was, who held that there is no corruption without preceding generation and no generation without previous corruption. Even as the old vegetation withers, shrivels and dies, new seeds are beginning to sprout underground and soon push their tender shoots above the surface. Let us apply this viewpoint to the transition from declining ancient and classical civilization to the early middle ages. Even as the structure of the world and of society seemed tumbling about men's ears, as the Roman Empire was going to pieces and barbarians were everywhere running amuck, new developments were already in process and taking form in varied fields of human activity. We may first briefly note the new development in art, in history and legend, in education and the study and use of the Latin language, and then ask whether there was not a corresponding and concomitant scientific activity. Perhaps, instead of Pope's facile generalization that "the same age saw learning fall and Rome," we shall find hints that learning, like art, was already laying new and deep foundations, even as the political structure was toppling to the ground and while statesmen and politicians were still clinging desperately to old ideals and adhering to outworn methods.

I shall never forget my astonishment, on the occasion of my first visit to Europe and to the Roman forum in the summer of 1909, to find that the most impressive remains within that historic area were not the ruined buildings of the early empire but the Arch of Constantine and the Basilica of Constantine (or his predecessor Maxentius). Though only the three vaults of one side-aisle had been left standing by the earthquake of 1348, how they still towered in their grandeur over all the rest of the buildings in the Forum! That same summer I had a similar experience at Rheims, where I was simply enchanted by the exquisite proportions and striking originality of the Porte de Mars, a structure dating from the late fourth or fifth century A.D. Are these monuments to be dismissed as isolated instances, the swan songs of expiring Roman art? If so, we may turn to the wonderful buildings preserved at Ravenna from the fifth and early sixth centuries, or to the great church of St. Sophia which has been characterized as "perhaps the boldest instance of a sudden change in almost every respect, whether of plan, elevation, or detail which is known to architecture." It is the chief of the very few edifices left from Byzantine Constantinople, and without it a great chasm would yawn in the world history of architecture. May some correspondingly significant scientific creation have been destroyed and remain unknown? May the history of medieval science be revised as the history of Romanesque art has been, so that crosses in England at Bewcastle and Hexham which used to be regarded as twelfth-century work have been dated from inscriptions in A.D. 670 and 740, and Kingsley Porter assures us that "from the tombs of Venasque, Bobbio and Pavia we learn that the seventh and eighth centuries, instead of being an age of the utmost artistic degeneration, were capable of producing subtle and thoughtful carved decoration in stone of the finest execution."

A contemporary of Constantine, the Christian author, Eusebius of Caesarea, was the father not only of church history but of universal or world history. The ancient Greek and Roman historians had tended to follow the example of Homer's *Iliad* in making some particular war the center of their narrative, or to write concerning their own city or state from a local, patriotic or national point of view, and further to regard the writing of history as an art, or as a branch of literature, rather than as an independent subject and social science. Eusebius for the first time viewed the past as a unit in its entirety and was guided in his arrangement of historical events by one central idea, the development through the ages of a divine plan and the progress of the kingdom of God—an idea to be re-emphasized a century later by St. Augustine in *The City of God*. Eusebius further, by the device of parallel columns of dates, endeavored to correlate the chronologies of various peoples and cultures, and to bring them into close association with Biblical and Christian history. So that it was the judgment of the modern French authority on historiography, Monod, that "we owe the development of chronological science as well as the first scientific conception of history to the Church."

If we turn from history to the realm of legend and of literature, we may observe that the oldest Latin version of the legendary story of Alexander the Great bears the name of Julius Valerius, who appears to have flourished in the fourth century of our era.

In the field of education the same century saw the composition of new Latin textbooks which were to be conned by schoolboys for the next thousand years and more, namely, the moral *Distichs of Cato* and the elementary grammar of Donatus, which was presently to be supplemented by the more advanced grammar of Priscian, composed during the barbarian invasions about 500. In the *Margarita philosophica* of Gregorius Reisch, written in 1495 and first printed in 1503, a picture of the tower of knowledge shows the Donatists on the ground floor, the students of Priscian in the mezzanine, and more advanced subjects in the upper stories. The long-continued currency of the *Distichs of Cato*[1] is attested by a passage in the fourteenth-century *Vision of Piers the Ploughman,* in which the sin of Sloth, personified as a member of the clergy, confesses his ignorance of Latin in these words:

[1] The *Distichs of Cato* were published in Madison in 1922 in the University of Wisconsin Studies in the Social Sciences and History, with an introduction and English translation by Wayland Chase.

I have been priest and parson passing thirty winters . . .
But I cannot construe Cato, nor speak clerically.

St. Jerome, who had continued the universal history of Eusebius from the year 329 to 378, later completed his own Latin translation of the Bible known as the *Vulgate,* which was accepted as standard by the Roman Catholic Church and which exerted a great influence upon the future development of medieval Latin.

We have next to ask ourselves: Was there any shift in science in the fourth century or thereabouts to match these notable new departures in the fields of art, history, education, and language? The ancient Romans are not especially noted for scientific achievement, and the fathers of the Church, if not so hostile to science as they have sometimes been represented, were certainly more intent upon other matters. Yet the fourth century was not without indication of interest in the world of nature and of activity in both pure and applied science. The Eastern audiences who listened to Basil's sermons in Greek on the six days of creation displayed great curiosity concerning natural phenomena. The Latin paraphrase of these sermons by Ambrose suggests a like interest in the West. To Julius Firmicus Maternus, composing an astrological manual in Latin, the stars seemed still to promise a coming crop of scientists and inventive intellects. He predicts the birth of intellectual pioneers three times; that of inventors, once; those absorbed in the secrets of all arts, once; geometers, thrice; other mathematicians, six times; astronomers and astrologers, fourteen times; medical men, eleven times; surgeons, once; and botanists, twice. The great astronomical work of Ptolemy was made the subject of commentaries by Pappus, Theon, and Hypatia at Alexandria in the fourth century, and by Proclus in the fifth century. Aristotle was commented on by Themistius in the fourth and by Philoponus and Simplicius in the first half of the sixth century.

The writings of Greek alchemists do not begin with the age of Pericles, nor with that of the Museum at Alexandria, nor even in the century of Galen and Ptolemy, but start with Zosimus of Panopolis in the third century of our era and extend to Olympiodorus in the early fifth century. Their somewhat fantastic reveries were paralleled in early medieval Latin by practical manuals devoted to technical processes in metals, glass, mosaic, painting, gilding, and the preparation of parchment, such as the *Compositiones ad tingenda,* extant in a manuscript of the later eighth century, in which are found for the first time such words as "bronze" and "vitriol" and a mention of the making of cinnabar from mercury and sulphur.

The invention of valves for bellows, which made an advance in iron-working possible, has been attributed to the fourth century, and the first casting of church bells to Paulinus, bishop of Nola in Campania, places after which bells were called *nolae* and *campanae,* and the bell tower, *campanile.* Paulinus lived from A.D. 354 to 431. The first public mill run by water power at Rome was situated at the foot of the Janiculum hill and dates from A.D.

398. Later on, in the year 536, when the Goths were besieging Belisarius in Rome, they destroyed the water courses which turned the millstones in that city. But Belisarius set up floating ship mills in the midstream of the Tiber and, by stretching chains across the river, protected them from the logs and corpses which the Goths floated downstream against them.

Belisarius' master, the great Emperor Justinian, tried to prevent further legal interpretation, thought, writing, and development by stereotyping the Roman law in the form of his *Digest, Code,* and other lawbooks. But medicine continued its independent development in the works of Aëtius of Amida in Mesopotamia about the year 500, Alexander of Tralles in Asia Minor in the sixth century, and Paul of Aegina in Greece proper in the seventh century. Over two hundred years ago Friend and Milward protested against regarding these men as mere compilers —as Oribasius, the friend and physician of the last pagan emperor, Julian the Apostate, had perhaps been earlier— and maintained that rather they "were really men of great learning and experience" who "described distempers which were omitted before; taught a new method of treating old ones"; gave "an account of new medicines, both simple and compound; and made large additions to the practice of surgery." Alexander of Tralles was the son of a physician; his brother Anthemius was the architect of the church of Saint Sophia; another brother was a noted grammarian. Alexander visited Italy, Gaul, and Spain as well as all parts of Greece before settling down in old age, when he could no longer engage in active medical practice, to compose his *magnum opus* in twelve books. He often disagreed with previous authorities, and recorded his own observations and experiences. Friend said that his method was "extremely rational and just, and, after all our discoveries and improvements" in medicine, "scarce anything can be added to it." More recently Puschmann praised Alexander as an original thinker and pathologist: his arrangement was concise and more orderly than that of previous medical writers. He was the first to open the jugular vein and to mention rhubarb and tapeworms. He displayed his ingenuity in checking nosebleed by blowing fuzz or down into the nostrils, and he dislodged foreign objects from the ear by having the patient sneeze with mouth and nose stopped up—this a thousand years before the announcement of the discovery of the Eustachian tube.

These medical authors, and also the commentators on Aristotle and Ptolemy whom we have mentioned, composed their works in the Greek language and were connected, primarily at least, with the Byzantine Empire or eastern half of the Mediterranean world. But it was not merely the Bible, the church fathers, and the legend of Alexander that were translated into Latin for the West. We shall have more to say presently of the significance from the standpoint of the history of science of the translation of the *Timaeus* of Plato which Chalcidius made in the fourth century. Such translations were not confined to that century but went on through the entire period of barbarian invasion. Of the twenty-five extant books from the original seventy-two of Oribasius' great medical compendium, a Latin version, made perhaps in the sixth century, is preserved in manuscripts of the seventh, ninth, and twelfth centuries. Heeg, in his *Studien* of 1913 on the pseudo-Democritus, dated in the fifth or sixth century a Latin translation of the Greek medical *Prognostica Democriti* which utilized the *Synopsis* of Oribasius. A pseudo-Galenic commentary on the Hippocratic treatise, *De septimanis,* was published in 1914 by Bergsträsser in an Arabic version. The Greek text is lost, but there is another early translation in barbarous Latin.[2] An early Latin translation of the work of Alexander of Tralles exists in manuscripts of the ninth century, and it was from this Latin translation and not the Greek original that the Hebrew and Syriac versions were made. It also was much cited by later medieval Latin writers like Constantinus Africanus, Gariopontus, and Gilbert of England. By the sixth century there was a complete literal Latin translation of the work of Dioscorides on materia medica, and also apparently more than one partial version.

Sometimes a Greek or oriental work has survived only in Latin translation. A rather extreme illustration may be given of this point. Priscian of Lydia was one of the Greek professors who found a refuge at the Persian court when Justinian closed the schools of philosophy at Athens. His answers to nine questions which were put to him by the Persian monarch, Chosroës, in that distant oriental re-

[2] See Charles Singer, "Biology before Aristotle," in *The Legacy of Greece*, ed. R. W. Livingstone (Oxford, 1922), 170n.

treat are extant neither in Persian nor Greek nor Syriac nor Arabic but only in Latin translation in a single manuscript of the ninth century. Similarly the treatise of Maslama of Cordova on the astrolabe is preserved only in the Latin rendition by John of Seville. It is far from being the sole Arabic work that is preserved only in Latin form.

Incidentally, in regard to the closing of the schools at Athens by Justinian, it may be remarked that a student of *computus* (i.e., the ecclesiastical calendar) from Armenia found teachers of philosophy again at Athens in the next century. But in general the later history of Byzantine education is very obscure, and there would seem to have been more progress in medical science than in any other form of science in the Byzantine Empire. However, from the fact that palimpsests of scientific manuscripts in Greek are seldom found, Heiberg inferred that such manuscripts continued to be read and used in all periods of Byzantine history.[3] The three instances of such palimpsests known to him may be worth noting: (1) a ninth-century manuscript in the British Museum in which fragments of Euclid, together with fragments of the *Iliad* and the New Testament, had been effaced to make way for a text in Syriac; (2) a manuscript at Bobbio from which a treatise on burning mirrors and an opusculum by Ptolemy were erased in the eighth century; (3) a unique copy of the *Methodology* of Archimedes which in the thirteenth century gave way to a prayer book preserved in the con-

[3] J. L. Heiberg, "Les sciences grecques, et leur transmission," in *Scientia* (1922), 31:101.

vent of the Holy Sepulcher at Jerusalem. On the other hand, in a manuscript of the twelfth century which is now in the Bibliothèque Nationale of Paris, the *Elements* of Euclid were written over an obliterated text of the *Septuagint.* But Duhem contrasted unfavorably the meager and faulty science of John of Damascus, who had at his disposal the rich libraries of the Orient, with the naïve curiosity as to nature of the Venerable Bede, writing in Latin in far-off Britain. Even so, the unscientific doctrine of John of Damascus that comets were *ad hoc* divine creations was well known in the Latin West.

Until Valentin Rose discovered and published in the first volume of the periodical, *Hermes,* in 1866 the prologues of the Sicilian translator from the Greek, Aristippus, to the *Meno* and *Phaedo* of Plato, it used to be generally believed and continually asserted that the *Timaeus* was the only work by Plato of which the medieval West had direct knowledge, and that in the form of the fourth-century Latin translation and commentary by Chalcidius. Indeed, Rose's discovery made no deep or widespread impression upon the English-speaking world until the well-known medievalist, Charles Homer Haskins, reaffirmed it in 1910 and subsequently. Moreover, even Haskins still held that "until the translation of the *Meno* and *Phaedo* ca. 1156, the only work of Plato directly known to the western Europe of the Middle Ages was the *Timaeus,* or rather the first fifty-three chapters as translated and commented upon by Chalcidius in the fourth century." Haskins further implied that no other works of Plato appeared in Latin until the humanist translations of the fifteenth century. So far as I know, to date no new manuscripts have been found to contradict these conclusions, although it is conceivable that one or more may turn up. Let us, however, agree for the sake of argument that the *Timaeus* alone held the field in the West from the fourth century to 1156, and, except for a few manuscripts of the *Meno* and *Phaedo,* dominated it for three more centuries after 1156. "This in itself is a curious fact," remarked Haskins, "for 'of all the writings of Plato,' says Jowett, 'the *Timaeus* is the most obscure and repulsive to the modern reader, and has nevertheless had the greatest influence over the ancient and medieval world.' Accordingly," continued Haskins, "medieval Platonism was largely concerned with the vague and mystic cosmogony of this dialogue."

But is the fact really so curious? It is a matter of common knowledge that the *Timaeus* is the only work by Plato which concerns itself with the world of nature, with cosmology, and a feeble attempt at mathematical explanation. And yet no one, so far as I know, has ever drawn the obvious and well-nigh inevitable inference that the Middle Ages read the *Timaeus* almost alone of all the dialogues of Plato because the Middle Ages were more interested in natural science than they were in the other subjects treated by Plato. It was not because it was vague and mystic that they read it, but because, vague and mystic as it was, it was the best that Plato had to offer them, almost the only thing that he had to offer them that made any approach to natural science. Possibly one might prefer the converse inference that the Middle Ages became interested in natural science because the *Timaeus* was the only work of Plato directly known to them. But that will not explain why Chalcidius chose it to translate and comment on, and why his version of it survived.

If we continue to peruse the passage by Haskins from which we have already quoted, we shall find further, albeit unwitting, support for the conclusion which we have just drawn. Haskins says:

> The other principal source of [medieval] Platonism was the fifth-century commentary of Macrobius on the *Somnium Scipionis* of Cicero. Revived in the ninth century, this contained a considerable amount of ancient astronomy and geography; and it served as the vehicle for transmitting an important fragment of non-Platonic astronomy, the hypothesis respecting the movement of Venus and Mercury about the sun which is commonly ascribed to Heraclides of Pontus. . . . There are also bits of Platonism in the astronomical part of Martianus Capella, from which an extract beginning, "Mundus igitur ex quatuor elementis . . ." is sometimes found in manuscripts of the period.

So far Haskins.[4] In point of fact, as Duhem[5] has shown, both Chalcidius and Capella, as well as Macrobius, transmitted the Heraclidean hypothesis.

[4] Charles Homer Haskins, *Studies in the History of Mediaeval Science* (Cambridge, 1924), 88–89.

[5] Pierre M. M. Duhem, *Le système du monde* (5 vols., Paris 1913–1917), 3:47–52.

But not only was natural science the *leit motif* in the one or chief genuine work of Plato directly known to the Latin Middle Ages and in such indirectly transmitted Platonism as was found in the writings of Macrobius and Martianus Capella. The several supposititious works which passed under the name of Plato tell a like story of medieval interest in alchemy and in natural magic.[6]

From the data regarding the transmission of Platonism to the Middle Ages one more deduction may be made, and that is the reaffirmation in the cases of Chalcidius and Macrobius and Martianus Capella of the importance of writers of the fourth and fifth centuries in the history of science.

Henry Osborn Taylor, in his book *The Classical Heritage of the Middle Ages,* although describing the work of Capella as "perhaps the most widely used school book of the Middle Ages," and again as "the 'standard' school book of the Middle Ages," and as "pabulum for coming generations," and although devoting two whole pages to its two mythological and allegorical introductory books, instead of comparing the contents of the other seven books on the seven liberal arts with previous Latin schoolbooks, if any, or with subsequent manuals and tracing their medieval influence, incontinently dismissed them as being very

[6] See the paper by Dorothea Waley Singer, "Alchemical Texts Bearing the Name of Plato," *Ambix* (1946), 2:115–28; and the accounts of *Liber quartus, Liber tegimenti, De tredecim clavibus,* and *Liber vaccae* or *anguemis* or *institutionum activorum* in my *History of Magic and Experimental Science,* Vol. II (consult bibliographical index under "Plato, spurious").

dry and "strictly instructive"—whereas his reproduction of them gave no information—and as being "sapless as the rods of medieval schoolmasters" and their author as "a desiccated person."[7] Thus on the one hand poor Capella was blamed for being dry in the main part of his work where he gave a digest of contemporary knowledge—in which Henry Osborn Taylor was apparently not at all interested and of which he gave no adequate account—while on the other hand fun was poked at Capella for trying to interest the reader in the work by mythological trimmings and allegorical window-dressing—although this was apparently the only portion of the work that Taylor himself derived any amusement and satisfaction from. Thus, whereas one would naturally have expected the chief emphasis to be laid upon Capella's selection of material and subject matter, his method of presentation and instruction, and on the degree to which he reflected and preserved classical science and learning and foreshadowed and influenced medieval learning and science, with not much more than passing reference to his allegorical and mythological introduction, Taylor took the exactly oppo-

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[7] In this connection it is interesting to note that the two printed Italian translations of Capella in 1578 and 1629 were limited to the first two mythological books of his work, as was an early eleventh-century translation in High German preserved in a manuscript at St. Gall. See E. Narducci, "Intorno ad un comento inedito di Remigio d'Auxerre al *Satyricon* di Marziano Capella e ad altri comenti al medesimo *Satyricon*," *Bullettino di Bibliografia e di Storia delle Scienze Matematiche e Fisiche* (1882), 15:505–565, at pp. 507–509. For commentaries limited to the first two books, *ibid.*, 529–530.

site course and treated a work which was primarily a textbook and so intended—although hardly as universally used as he suggested—as if it were a work of pure literature and belles lettres. Perhaps he was a little aggrieved to discover that *The Marriage of Philology and Mercury* was not a novel or love story but a manual of learning, and so decried it as being dry, much in the manner of Pope, who at the age of twenty in his *Essay on Criticism* dismissed the entire medieval period with the couplet:

> Much was believed, but little understood,
> And to be dull was construed to be good.

The historian of thought and science, on the contrary, tries to tell what was believed and to be sure that he himself understands it, and not to approach and appraise a work of science or learning from a purely literary and pleasure-giving standpoint. Furthermore, he tries to show what the men and minds of the past *were* interested in rather than what he is *not* interested in. Taylor should have paid a little less heed to what Capella said of the marriage of the god Mercury and given a little more attention to what he said of the movement of the planet Mercury. For Capella declared flatly that "Venus and Mercury do not move about the earth . . . it is the sun that they take for the center of their respective circles."

So far I have been seeking to suggest that there was interest in science and some scientific activity not only in this declining Roman Empire but even in the early Latin Middle Ages. Some time ago it was customary to set forth the scientific achievements of the medieval Arabic speaking and writing world in glowing colors, and to make disparaging comparison between their activity in various fields and the supposed stagnation of the Latin West. Thus Chasles could assert in his "Aperçu historique sur l'origine et le développement des méthodes en géometrie," which appeared in 1837 in the publications of the Belgian Royal Academy at Brussels: "From the eighth century to the thirteenth Europe remained sunk in profound ignorance. Love and knowledge of the sciences were preserved during that long interval by a single people, the Arabs of Bagdad and Cordova."[8] I did not myself exhume this tidbit, which by this time, like Lazarus, stinketh, but found it repeated with apparent approval and at greater length by a leading modern Catalan historian of science, J. Millás Vallicrosa, in his work of 1931 on the history of physical and mathematical ideas in medieval Catalonia from the sixth to twelfth century. Yet it not only does grave injustice to the prevalence of science and learning in Christian Europe, especially in the twelfth and thirteenth centuries, but also disregards the fact that but a small fraction of the populations of Bagdad and Cordova and a still smaller fraction of the scientists in those places were Arabs, most scientific writings in Arabic having been the work of persons who were not Arabs and sometimes were not even Moslems.

This dictum of Chasles is now out-of-date, because with the more intensive study of western medieval science, in

[8] *Mémoires couronnés de l'Académie Royale des Sciences et Belles Lettres de Bruxelles* (Brussels, 1837), 1:480–521.

late years the balances have appreciably shifted. Long series of publications by many investigators have greatly increased our knowledge and heightened our estimate of the value of medieval Latin science. On the other hand, much less that is new and important has been brought out by recent research into Arabic writings, where the law of diminishing returns seems to be operating. One or two illustrations of this may be adduced.

Al-Jahiz, who died A.D. 869, composed a book on animals which was briefly characterized by Sarton as "a very discursive compilation, the purpose of which is theological and folkloric rather than scientific."[9] It is on the basis of a passage in the chapter on the ostrich in this book on animals that Fräulein Rudi Paret, in an article covering six pages in the periodical *Der Islam* on "An-Nazzam als Experimentator," has hailed the philosopher An-Nazzam, who died in 845 and was Al-Jahiz's master, as an exponent of experimental method superior even to Albertus Magnus.[10] After asserting that the ostrich will swallow and digest not only pebbles but live coals, Al-Jahiz continues:

> It was reported to me by . . . An-Nazzam (and we doubted his report not, if he told what he himself had heard or seen) that he was present when Muhammad ibn Abdallah placed stones on the fire and, after they had become as hot as coals, threw them to the ostrich which gobbled them up as it gobbles up coals.

[9] George Sarton, *Introduction to the History of Science* (3 vols. in 5 parts, Baltimore, 1927–1948), 1:597.

[10] *Der Islam* (1939), 25:228–235.

I [i.e., An-Nazzam] had said to him, "Coals are weak and quickly extinguished, if they come in contact with moisture. And if one covers them with anything that cuts off the fresh air, they go out. Stones hold ever fast the warmth that has penetrated them. Won't you heat some stones (and throw them to the ostrich)?"

So he heated some and threw them to him. Then he swallowed the first, but I was in doubt whether he would go on. But when he did the same with the second and the third, I marvelled much.

I thereupon said to Muhammad, "Won't you heat up a few ounces of iron, some pieces of a quarter or half a pound?"

He did it and the bird swallowed this too.

Then said I, "That is more remarkable than the first (eating coals) or the second (eating hot stones). Now there is one thing more for us to do, namely, to see whether he digests the iron as well as the stones."

However, a stupid jackass made it impossible for us to continue this. For I had had the intention to kill the bird after a day or two and examine its gizzard and crop, and perhaps have found that the iron had remained there without dissolving and going through. But one of Muhammad's companions took a knife, heated it in the fire and threw it to the bird who gobbled it up. But it went no farther than its throat, which the point pierced and the ostrich fell dead.

This solitary instance of reported experimentation, based upon a secondary source and sounding rather like a cock-and-bull story, is preferred by Fräulein Paret to the passage in which Albertus Magnus briefly states that he had heard it said that the ostrich would eat and digest iron, but that the many ostriches to whom he had offered the metal had consistently declined it, although they would eat bones and stones readily. But there are many other instances in the works of Albertus of trust in, resort to, and test by observation and experience, whereas it would appear that Fräulein Paret's example is the sole extant record of experimentation by An-Nazzam, to whom Sarton devoted only five lines and these "chiefly because of his theory of creation." Even if we assume that every detail of the story is true, it seems to have been an isolated instance both in the career of An-Nazzam and in Al-Jahiz's book on animals. Furthermore we are moved to ask why Muhammad and An-Nazzam did not repeat their experiment with other ostriches; Albertus did not stop with a complete experiment with one bird but offered iron vainly to many ostriches.

However, an article in an Egyptian periodical on the scientific method of Al-Jahiz as shown in his Book of Animals,[11] which preceded that of Fräulein Paret by some eight years but was unmentioned by and apparently unknown to her, affirms that "the modern methods of close personal observation, experimentation and drawing logical conclusions were all used by this Moslem scientist long

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[11] Shafiq Jabri, "Tabqiq al jahiz," in *Majallat al-Majma al-Ilmi al-Arabi,* for August, September, and October, 1931, at pp. 468–483, 548–556, and 557–564. I have not seen the original article but use the English summary of it by Philip K. Hitti in *Social Science Abstracts* (1932), Vol. 4, No. 7, abstract 11064, found by a member of my seminar, Vito W. Caporale.

before they were discovered in Europe." But since the writer of the article goes on to say of Al-Jahiz, "Informed that a male ostrich could swallow a live coal without injury to itself, he tried it and killed the animal," a statement which is not in accord with the account already quoted from the Book of Animals by Fräulein Paret, one hesitates to place much reliance upon his other specific examples or his general conclusion.

We pass on to one more specimen of recent publication concerning Arabic science, this time of the eleventh century. It too is a bird, as Aristophanes would say. The prevailing theory among the Arabic physicians of that time was that the young of flying birds were hotter in temperament than the chicks of the domestic hen. Ibn Butlan, who was a Christian from Bagdad, upon arriving in Cairo in 1049, heard that at the palace of the vizier there was another Christian, a Jacobite from Damascus, who maintained the opposite view and who had baffled the medical savants of Egypt by the new argument that the chick, unlike the young of flying birds, was able to run about and pick up its own food as soon as it was hatched. Ibn Butlan thereupon offered to supply yet other arguments on the side of the chick, but he did this purely as an exhibition of his intellectual resourcefulness and dialectical skill, since at heart he still adhered to the traditional doctrine. He thereby became involved in an interchange of no fewer than five treatises with a Muslim of Cairo, named Ibn Ridwan, in which they vied in a display of their erudition in Greek medicine and philosophy, and than which in 1937 at Cairo

Joseph Schacht and Max Meyerhof found nothing better to publish in a volume called *The Medico-Philosophical Controversy Between Ibn Butlan and Ibn Ridwan of Cairo. A Contribution to the History of Greek Learning Among the Arabs.* Talk about your Western scholasticism and its disputation of idle questions!

What used to be considered the normal process of the transit of ancient Greek science to the medieval Latin West, namely, by Arabic translation from the Greek and then by Latin translation from the Arabic, we have seen was often preceded or accompanied by direct translation from Greek into Latin. We even find an Arabic work translated first into Byzantine Greek and then from it into Latin. Thus an astrological tract by Albumasar on the revolution of nativities was first translated into Byzantine Greek and then from it into Latin not later than the thirteenth century, as Professor Haskins showed from a Latin manuscript of that century in the Bibliothèque Nationale.[12]

One more sidelight upon the relationship between medieval Latin and Arabic science I repeat substantially from

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[12] *Studies in Mediaeval Science*, 221–22.

I mention this fact for another reason. In the voluminous *Census of Medieval and Renaissance Manuscripts in the United States and Canada*, drawn up by Seymour de Ricci and William Jerome Wilson, there is a single entry for the state of Mississippi. But this lone codex in the public library of Greenville, Mississippi, not only contains the treatise of which we were just speaking, but, although itself written in 1480, gives a definite date, A.D. 1262, for the translation from Greek into Latin which was lacking in the two manuscripts listed by Haskins and in two others noted in my *Catalogue of Incipits of Mediaeval Scientific Writings in Latin.*

the introduction to my recent volume, *The Sphere of Sacrobosco and Its Commentators.*[13] The *Sphere* of Johannes de Sacrobosco held its place as the leading textbook in elementary astronomy from the time of its composition early in the thirteenth century down into the seventeenth century. It has been described as "slavishly derived from al-Farghani and al-Battani,"[14] but I believe that this accusation is greatly overdrawn and cannot be substantiated. This is not to deny that Sacrobosco made use of those authors, or at least of Alfraganus, but he did so in his own way, and they were far from being his sole sources. If we compare the *Sphere* with Alfraganus' *Rudimenta, Differentiae scientia astrorum,* or *Liber de aggregationibus,* as it has been variously styled, we find that the work of Sacrobosco has a well-constructed plan and order which is much superior to the sprawling presentation by Alfraganus in thirty chapters. The first chapter of Alfraganus on Arabic and other months and eras is not paralleled in the *Sphere,* although Sacrobosco cites it in his *Computus.* The arguments, in Alfraganus' second chapter, that the sky is a sphere are repeated by Sacrobosco, who cites Alfraganus for them expressly. But first he gives three reasons from likeness, convenience, and necessity which are not drawn from Alfraganus. And before these are the six opening paragraphs of the *Sphere,* with citation of Euclid, Theodosius, and so forth, which in so far as they are paralleled in Alfraganus

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[13] Lynn Thorndike, *The Sphere of Sacrobosco and Its Commentators* (Chicago, 1949), 15–21.

[14] Sarton, *History of Science* (1931), Vol. 2, Part 2, p. 617.

at all correspond to his fifth chapter on the two movements of the heavens and to other passages which occur still later in his work. His third chapter, giving proofs that the earth is spherical, is repeated more tersely and effectively by Sacrobosco, who adds a proof that the water also is spherical, namely, that a signal on shore can be seen from the masthead of a ship after it has disappeared from the view of a person standing at the foot of the mast. There is resemblance again between the *Sphere* and the fourth chapter of Alfraganus, in that both show that the earth is at the center of the heavens.

But the resemblances to Alfraganus thus far noted may be accounted for by the explanation that both Alfraganus and Sacrobosco are following and summarizing the *Almagest* of Ptolemy, and that Sacrobosco does so independently. For example, six successive chapters of the *Almagest* argue that the heavens are moved spherically, that the earth too is a sphere, that it is in the middle of the heavens, that it has no local motion of its own, and that there are two different movements in the heavens. Sacrobosco, as we have seen, considers the last point first. Then he takes up the other five precisely in the order of the *Almagest*, whereas Alfraganus omits that on the immobility of the earth. Furthermore, the sphericity of the surface of the sea, which Alfraganus also omits, follows the sphericity of the earth in the *Almagest* as it does in the *Sphere*, although Ptolemy gives the different illustration of seeing mountains rise gradually out of the sea as we approach them.

Sacrobosco followed Eratosthenes' estimate of a degree as 700 stades and the circumference of the earth as 252,000 stades rather than Ptolemy's erroneous figure of 500 stades per degree or Alfraganus' estimate of 20,400 miles for the earth's circumference, based upon the measurement of a degree under Al-Ma'mun as fifty-six and two-thirds miles.

The ten circles of the heavens do not stand out together in either Ptolemy or Alfraganus as they do in the *Sphere* but are mentioned separately in connection with other topics.

Sacrobosco seems indebted to the discussion by Alfraganus of habitable places, diversity of days and nights, summer and winter, and the seven climes. Especially in the case of the climes, where he gives identical figures in miles for the breadth of each clime without seeming to realize that this involves acceptance of Alfraganus' estimate of 20,400 miles for the earth's circumference, which he failed to adopt before, Sacrobosco's treatment appears at first sight a mere repetition of the text of Alfraganus. Yet in this instance he fails to cite Alfraganus, and he gives the names of the climes as Alfraganus does not. He makes no use of Alfraganus' long ninth chapter on the regions and cities in each clime and little if any use of Chapters 12 to 30. Nor is the treatment of the ascensions of the signs in Alfraganus' tenth chapter followed at all closely. In brief, Sacrobosco has omitted a great deal, has taken only what suited his purpose, has condensed it and usually stated it in different words, and has rearranged it according to a more effective plan under topics which are more mutually exclusive. He has not followed Alfraganus "slavishly." Still less is any close connection apparent between the *Sphere* and the more elaborate, advanced, and specialized treatise of Al-Battani.

It is true that some commentators on the *Sphere* rather support the notion of the superiority of Alfraganus and the indebtedness of Sacrobosco to him. The commentary ascribed to Michael Scot and another which perhaps copies it speak of "Ptolemy and Alfraganus, from whom this treatise is drawn." By a third commentator, Ptolemy in the *Almagest* and other writers, especially Alfraganus, are said to have treated the subject by way of demonstration, Sacrobosco by way of introduction only. A fourth commentator, however, concedes the demonstrative method to Ptolemy, Geber, and Thebit, but puts Alfraganus with Martianus Capella and "great John of Sacrobosco, the compiler of this book," as employing the narrative method. I do not know that any commentator represented Alfraganus as Sacrobosco's chief or sole authority.

Allowance should be made, in estimating the relative originality, scientific ability, importance, and influence of two such authors as Alfraganus and Sacrobosco, for the common tendency of medieval writers to roll off their tongues with oft-repeated unction the names of ancient authorities and celebrated Arabic authors but seldom to mention by individual name their Latin contemporaries, to whom they prefer to allude by some vague and generic term like *moderni, recentes,* or *novitii.* A case very much in point is presented by Rufinus, who lived near Genoa and composed his herbal soon after 1287. In a long astronomical passage he cites Alfraganus more than once as his chief authority, but his whole presentation of the subject strongly suggests the *Sphere* of Sacrobosco, whom he never deigns to name. It may even be that he was not directly acquainted with the text of Alfraganus but derived that magic name from Sacrobosco's fairly frequent citation thereof. Similarly Pedro Cirvelo, in opening his commentary on the *Sphere* toward the close of the fifteenth century, described it as collected from the books of Ptolemy, Thebit, Alfraganus, and others, whereas it is obvious that Sacrobosco was unacquainted with, or at least totally omitted, Thebit's theory of the motion of the eighth sphere.

Moreover, much of the *Sphere* seems to be derived neither from Arabic sources nor directly from the *Almagest* but through a channel which is unmentioned either by the medieval commentators on the *Sphere* or, in this connection, by modern historians of science. Much of it came from a previous medieval Latin tradition, which might be traced back through such authors as William of Conches in the early twelfth century, Helpericus in the ninth or tenth, and Macrobius in the fifth. Take, for example, the commentary by Macrobius on the *Dream of Scipio*, a work found frequently in early medieval manuscripts. Macrobius there discusses the ten circles—equinoctial, zodiac, colures, horizon, meridian, Tropics of Cancer and Capricorn, and the Arctic and Antarctic circles—much as Sacrobosco does. From Macrobius, also, might have been taken the estimate of the earth's circumference as 252,000 stades and the discussion of the numerical relation between circumference and diameter, or the explanation of the sense in which a planet may be said to be "in" a sign. Macrobius, too, treats of the nine spheres and the movement peculiar to the planets. He notes the five zones and holds that the southern temperate zone is habitable. He antecedes Sacrobosco in five quotations from Vergil and one from Lucan, to which Sacrobosco added others from Lucan and Ovid. But the indications that he used Macrobius seem quite as impressive as do the resemblances to Alfraganus.

It may seem strange that the *Sphere* should cite Alfraganus a number of times by name and hardly mention Macrobius. But perhaps Sacrobosco, too, enjoyed smacking his lips over the name Alfraganus, as so many other Latins seem to have done. Perhaps without it his readers would not have been satisfied. Perhaps they were "tired and sick" of Macrobius. He had gone out of fashion, along with the old-fashioned presentation of astronomy such as we find in William of Conches. Alfraganus and Arabic astronomy had become the rage! Sacrobosco apparently wrote his manual at just the right time to make a skillful combination of, and compromise between, the old literary astronomy of the early Middle Ages and the new scientific astronomy of the twelfth-century translators from the Arabic. He made a mixture of Macrobius and Ptolemy and frosted it over with Alfraganus, and his book stayed in style for five centuries.

And here with Sacrobosco at the threshold of the thirteenth, "greatest of centuries," we terminate our paper, leaving untouched its other great scientific achievements as well as those of the fourteenth century and the ensuing humanistic reaction and falling-off in science which marked the fifteenth century.

The Definition of Scientific Method

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My object in this paper is to clarify some of the problems involved in attempting to define scientific method. I shall argue that most writers who have tried to define scientific method have been working with a notion of definition which is too narrow for the task. I shall try to outline a pattern of definition which would be more adequate and discuss some criteria which any satisfactory definition of scientific method must satisfy. The purpose will therefore be to clear away some of the difficulties which have impeded the search for a satisfactory definition, rather than to provide a definition of my own.

Let us begin by considering some of the motives which lead us, as it has led so many philosophers and scientists in the past, to search for a definition of scientific method—or for what is nearly the same thing, science itself.

The laziest answer will invoke that "intellectual curiosity" which so conveniently explains an interest in truth for its own sake. No doubt a thinker of precise intellectual habits will find it distasteful to be constantly using a term like "science" without having an explicit analysis of its connotation; the problem of definition may challenge his ingenuity as a chess problem might, and its solution will provide a satisfaction similar in kind. Disinterested philosophical lexicography is a harmless pursuit not to be sneered at in a world in which so few occupations are innocent. Nevertheless, we shall misconstrue the nature of our problem if we treat the definition of scientific method as a mere intellectual exercise.

When a term has a relatively well-determined denotation or application, the analysis of its connotation will, it is true, have little effect upon practice. Carpenters will continue to make tables, in happy ignorance of the epistemologist's inability to define the term "table." The case is different when the term to be defined has controversial or problematic application. To define such a term as "justice" is to engage in a hazardous occupation, as Socrates long ago discovered. Men are firmly convinced that justice is an excellent thing, while agreeing neither in the application of the term nor on the criteria which ought to determine its use. In these circumstances, the practice of those who use the term "justice" is likely to be as inconsistent as their thought is confused; and a good philosophical analysis runs the risk of bringing such inconsistencies to public notice. To anybody who continues to believe in justice, any shift in the term's application, induced by philosophical definition, threatens to bring about a redirection of his interests. And in general, *any* definition removing inconsistencies or involving a redistribution of emphasis will redirect the interests of those who use the term, provided they can understand the definition, and have sufficient intelligence to be moved by rational considerations.

The importance of such "persuasive definitions," as they have come to be called,[1] is being increasingly recognized.

[1] "A 'persuasive' definition is one which gives a new conceptual

We can see today, more clearly than in the past, that definition of difficult terms is usually a process not regulated simply by the character of the concept to be defined.[2] The adequacy of a persuasive definition has also to be judged in relation to the soundness of the interests which it is designed to serve; the criticism of persuasive definitions is a proceeding partly normative in character, involving considerations of an ethical as well as of a methodological and logical character. This view, if correct, raises some difficult questions of procedure, some of which must be considered later in this paper.

Now I wish to maintain that the attempt to define scientific method or to analyze science is a search for a *persuasive* definition. I hold this to be true because I believe that the term "science" has no definite and unambiguous application. No doubt we should all agree that physics is a science par excellence, and that the atomic physicists use scientific method, whatever scientific method may prove to be. But we shall hardly agree with the remark attributed to Lord Rutherford that science consists of "physics and

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meaning to a familiar word without substantially changing its emotive meaning, and which is used with the conscious or unconscious purpose of changing, by this means the direction of people's interests." (C. L. Stevenson in *Mind* (1938), 47:331.) I would want to change this definition of "persuasive definition" in some respects, however. The reference to "emotive meaning," for instance, commits users of the term "persuasive" to a controversial and, in my judgment, mistaken analysis of meaning.

[2] Strictly speaking there is no determinate "concept" in such a case.

stamp collecting"—if this is taken to imply that nothing but physics is a science in the strict sense. Is chemistry science? Of course. But is psychology a science or the mere hope of a science? Is history a science? Or mathematics? Or ethics? Or sociology? Such questions have no answer *because they have no clear sense;* and they are asked, paradoxically enough, just *because* clear sense is lacking. For the term "science" is eulogistic; whatever science may prove to be after analysis; and these requests for classification are also clamorous demands for the recognition and material rewards which await the application of the honorific label.

If this view of the situation is correct and we are looking for a persuasive definition, the search for a definition of scientific method will require the following combination of descriptive and normative procedures.

Instances of modes of investigation provisionally identified as eminently "scientific" will be collated and compared with the hope of determining common characteristics. The instances must be such as will not be seriously disputed—and this is perhaps why a few stock instances like Kepler's investigations into planetary motion reappear so often in textbook discussions of scientific method. So long as agreement about the scientific character of the instructive examples can be preserved, the process of comparison and analysis can be treated as non-normative and descriptive; the collation of undisputed instances of scientific method is, in principle, as "objective" as taxonomy. Unfortunately, the generalizations resulting from the examination of such undisputed instances are too general to be of much use;

the definitions of scientific method produced at this level are little better than banalities, and just because they codify what is commonly accepted, such definitions help not at all in resolving the burning questions of the applicability of scientific method to disputed cases. To be told that "perhaps science is after all only organized common sense, preferably derived from experiment and preferably organized on a quantitative basis"[3] helps not at all to decide whether psychical research is scientific or ethics extrascientific. The formula elicits general agreement by virtue of its deliberate vagueness; and the vagueness covers a multitude of omissions. It is not unfair to say that the more those who write about scientific method agree, the less there is *about which* they agree.

Once we leave the area of universal agreement, we find ourselves compelled to choose criteria which are not clearly exemplified in acceptable instances: the instances are as problematic as the criteria to be employed. Our choice has to be made in the light of an interest we find to be good and is thus determined by normative considerations. It cannot be otherwise, since there is in this area of wider but uncertain application no definite denotation of the term to be analyzed. Let me make this plainer by an extreme illustration. Suppose my interest in science were to be confined exclusively to its chances of making me some financial profit (an attitude which is not altogether unheard of); in

[3] James Bryant Conant, "The Advancement of Learning in the United States in the Post-War World," *Science* (February 4, 1944), 99:91.

that case I might define as scientific only those investigations which, while conforming to the vague specifications achieved at our first level of analysis, *also* showed prospects of profitable exploitation by myself. The choice of my own financial interest as my *summum bonum* would be normative; but the implications of that choice would be nonnormative or, in the language I have been using, descriptive. If the instance seems grotesque, it is so because we know in advance that we should refuse to accept the profit motive of a single person as determining our own interests in science. But the emphasis of theorists, from Bacon onward, on science as yielding power and mastery is not so remote from the grotesque hypothesis I have just considered. Those who single out technology for special emphasis in the definition of scientific method are committed to regarding technological advance, in that context, as a supreme good. My point is that there must *be* a choice if the definition is to be worth having; it will be valuable *because* it is controversial. (The question of how such choices and value commitments are to be validated raises some of the most difficult problems of philosophy and cannot be discussed here.)

The regularizing of their own procedures is among the enduring interests of scientists. There is, in science, as in other creative human activities, a continual tension between the conservative demands of the tradition and the revolutionary activities of those who transform the tradition by revolting against it. There is something lawless in the creative process itself, and the scientists whom scientists have most wished to honor have made their discoveries by means as mysterious to themselves as to their contemporaries. But if their results are to be useful they must be communicable to those who are not themselves geniuses. Thus what begins as a brilliant discovery, as incoherent as it is dazzling, is eventually converted into a routine which the mere artisan of science can master and apply. In this way the new tradition is created but for which the later pioneers would have nothing to rebel against.

In some ways the progress of science, as here depicted, smacks too much of the marvelous and the unpredictable for comfort; and the hope has never been abandoned of reducing the process of discovery itself to a routine that can be communicated and taught. This hope has inspired investigators of scientific method from Aristotle to Descartes and from Bacon to Eddington. In one version of the legend of the Holy Grail those who sought it hoped to find a "self-acting, food-providing, talisman"[4] and this is precisely what such men as Bacon have hoped to discover. We can write off such a project as illusory and no more likely to succeed than the quest of the Grail itself; but it would be rash to assume that there are no principles relevant to the practice of research. However much we stress the final mystery of the art of creation, we have to admit that even the genius learns; and all learning is, necessarily, the learning of something general, reproducible, and, in theory at least, communicable. There are principles which assist the

[4] *Encyclopaedia Britannica,* 11th ed., 12:320.

process of discovery, however insufficient in themselves to yield novel results; and if even this is not conceded, it will perhaps be granted that there are erroneous principles which constrict and hinder scientific progress. The study of scientific method may help at least to remove some of the obstacles to the development and extension of scientific thought. This alone would be sufficient justification for the most careful attempts to provide satisfactory analyses of scientific method.

I hope I have said enough to indicate something of the motives leading men to formulate definitions of scientific method; I have explained my interpretation of the procedure in which they were engaged. Now anybody who is persuaded of the importance of this enterprise may well feel some disappointment upon examining the analyses and definitions which invite his acceptance. Consider, for instance, the ancient tradition which identifies what is "really" or "pre-eminently" or "essentially" scientific with what is mathematical. This Pythagorean attitude recurs constantly in the philosophy of science; it has deep roots in Platonic metaphysics, was strong in Kepler and Galileo, Leibniz and Descartes; it was stated with unequivocal definiteness by Kant and is a living force today. Such a view can certainly not be accused of triteness or banality; it appears rather as a wild paradox which only "a fool or an advanced thinker" would seriously defend. A position which regards mathematics as the queen of the sciences, relegating the fact-finding activities of the observer and the experimenter to the role of "mere" auxiliaries, is certainly in need of a good deal of argument to render it plausible. Yet the curious inquirer, naïvely wondering at the boldness of the abstraction involved, will search in vain for such defense. What he is likely to discover instead is a claim that science is *essentially* mathematical, in spite of all appearance to the contrary.

I suppose that very few who use such language would admit that they were in search of an *essence,* in some Aristotelian sense. Yet I think it plausible that some of the defects of any definition as abstract as this are due to the use of a pattern of definition which is Aristotelian in origin. For until recent times nearly all textbooks of logic have echoed Aristotle's doctrine of definition.[5] Those who would shudder at professing Aristotelian or Thomist metaphysics continue to look for definitions *per genus et differentiam* as if no other mode of definition were conceivable.

I shall not try to make much of the point that modern generalizations of traditional logic show definition by division to be only one among many conceivable forms of definition. For this is of not much importance here, except as helping to encourage an attitude of sensible irreverence in

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[5] "The traditional theory of definition is based upon the theory of the predicables. It can be summed up in the rule: definition should be *per genus et differentiam* (i.e., by assigning the genus and the distinguishing characteristic). This rule expresses Aristotle's view that definition states the essence of what is defined. . . . Everything, it is assumed, has a determinate essence and there is one and only one definition appropriate to it, viz., that which expresses the essence." (Lizzie Susan Stebbing, *A Modern Introduction to Logic* (1st ed., New York, 1930), 432.

respect of any claims of finality for Aristotelian logical doctrine.

What is more important to stress is that definition by genus and differentia is always definition of a *determinate* and *immutable species.* What we define in this manner is a *kind* of thing, capable of having repeated instances alike in character; and the kind of thing that we define must have precise and constant boundaries. So long as this type of definition is used, it is impossible to define the name of a unique entity, say "Napoleon"; nor can we define a general term such as "bald" which, being vague, admits of a fringe of borderline cases; nor a term such as "music" whose criteria change in time. Definition is of something generic, determinate, and unchanging; and it follows that definitions are final, in the sense of never calling for revision. We may make a mistake in defining "science" or "scientific method," but if we find the correct definition it will stand for all time; to characterize a definition as "provisional" or "approximate" is to talk nonsense. Again, if "science" is a vague term, the boundaries of which are *not* precisely determined, it is insofar recalcitrant to this kind of definition: the best we can do is to substitute for the vague term some more precise substitute which *can* be defined. If the progress of science is in some respects a unique historical phenomenon, definition is impossible; if the "nature" of science is not constant, there is nothing that we can properly define.

These presuppositions of the generality, definiteness, and constancy of the object of definition will seem to most people too obvious to be questioned; and we may as well grant that definition is most easily accomplished where the presuppositions are justified. I wish to urge, however, that when the object of definition is "science" or "scientific method" we are not justified in postulating generality, definiteness, and constancy. We ought at least to consider seriously the possibility that the "scientific method" which is worth defining is in some respects historically unique, is continuous with its contraries, and is appreciably variable with time.

If serious account is taken of the uniqueness, indeterminateness, and variability of scientific method in the course of its analysis, it will be a matter of relative unimportance whether the process of analysis is called "definition." For those who conform most faithfully to the Aristotelian canons of definition will permit *some* kind of investigation into the connotation of individual, indeterminate, or variable terms. Rather than talk of definition in such cases they may prefer to say that the individual can be *described,* the indeterminate can be rendered determinate (by elimination of vagueness), and the variable can be subsumed under unchanging laws of change. Such ways of describing the task to be performed are not to be recommended; for they blur the important point that what we have to do is not so much to describe an object or to invent a new notation *as to clarify the language we now have and the thoughts we express by means of it.* I see no good reason not to call this "definition." Whether it can be effectively practiced remains to be seen.

My quarrel with the traditional mode of definition is, in short, that it takes for granted certain conditions of generality, definiteness, and fixity which are not always, or always completely, satisfied; that rigid adherence to these presuppositions narrowly limits the range of what can be "properly" defined; and entails that "scientific method," *qua* unique, indeterminate, and subject to change, is indefinable.

So far, you may complain, no shred of evidence has been given of the contention that "science" and "scientific method" are recalcitrant to definition in the Aristotelian mode. I can think of no better means of persuasion than to invite you to contemplate in imagination the totality of the activities involved in and relevant to what we call "science."

Consider, if you will, the vast variety of activities of a scientific character which have occurred in the day now nearly past—the glass blowing and the dissecting, the manipulation of rulers, stop watches, test tubes, bunsen burners, cyclotrons, questionnaires; men fishing in swamps, solving differential equations, polishing lenses, composing manuscripts, developing photographic plates, writing a polemic against vitalism, modifying an axiom system; handling, manufacturing, observing, experimenting, calculating, theorizing, speculating. Is not the resulting impression one of the extreme diversity, not to say heterogeneity, of the activities which we are naturally inclined to regard as scientific? Yet there is something more than a mere aggregate here; we know that this apparent mesh of activities of observation, manipulation, experimentation, explanation, calculation, prediction, speculation is unified by an extremely fine network of relationships. There *is* a pattern, but an extremely complex one.

The activities I have distinguished are not conducted independently and in isolation; the prosperity of one depends upon the success of all: calculation is performed for the sake of experimental and observational test; experiment is conducted in the service of generalization, which in turn uses theory, which provokes speculation, which invites systematization, which is controlled by experiment . . . and so on, without end, in a maze of cross-connections and mutual dependencies. Science is an organic system of activities; and the pattern of its development is also organic.

We have imagined ourselves taking the latest cross section of scientific activity. To do justice to our subject we must extend our survey in imagination to cover the history and development of scientific activity no less than its present condition. We shall then see that this vast symphony of activities displays superordinate rhythms of development and change; there will be brought vividly to our notice the striking variety of motives and circumstances which have fostered or hindered the progress of science, the changes in instruments, modes of calculation, theories, underlying methodologies, and philosophies. For sheer complexity of texture and incident, science is like life itself and as little to be reduced to formula.

Some scientists regard an interest in the history of their subject as mere antiquarianism, and it may be that the very remote past consists largely of mistakes to be avoided. But it deserves to be remembered that the history of any scientific discipline intimately determines the current modes of investigation. The frames of reference which appear eligible at any given epoch, the instruments accepted as respectable, and the types of "fact" taken to have evidential value are historically conditioned. To pretend otherwise is to claim for human reason, as manifested in scientific progress, a universality and fixity it has never manifested. We may justly call the pattern of development organic, since the causal pattern is not analyzable into a set of independent causal strands; there is constant interaction along the temporal dimension.

A lively awareness of the complexity of science, regarded as a historical phenomenon, will make it seem unlikely that we can discern a relatively simple and immutable essence underlying the confusing procession of accidents. We seem to have not a coherent nexus of well-defined and fully cognizable universals, but rather a concentration and overlapping of characteristics of variable degree. None of the characters which we recognize in the scientific process are independently necessary or sufficient, but all supporting and jointly reinforcing one another give rise to the unique historical phenomenon.

Neither observation, nor generalization, nor the hypothetic-deductive use of assumptions, nor measurement, nor the use of instruments, nor mathematical construction —nor all of them together—can be regarded as essential to science. For branches of science can easily be found where any one of these criteria is either absent or has so little influence as to be negligible. Astronomy makes no experiments, mathematics uses no observation, geography is mainly descriptive, archaeology hardly uses measurement, much taxonomy frames no abstract generalizations, and biology is hardly beginning to use mathematical idealization and formalization. The characters mentioned are neither necessary nor sufficient, but they may be present in higher or lower degree and they contribute to what we recognize as science. Their diminution removes from an activity the features we apprehend as scientific; their joint presence in high degree creates conditions recognized as pre-eminently scientific.

This line of thought will lead us to abandon the search for a timeless and immutable essence in favor of the identification of a system of overlapping and interacting criteria. I propose to call this "definition of concrescence." By a concrescence I understand the simultaneous actualization of a number of mutually reinforcing characters, all of them capable of variations in degree.[6] I am proposing, in fact, that we take seriously the organic and historical aspects of science. I propose that we treat "scientific method" as a historical expression meaning, among other things, "those procedures which, as a matter of historical fact, have proved most fruitful in the acquisition of systematic and comprehensive knowledge." On this approach,

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[6] I am not using "concrescence" in the sense in which Whitehead did, though there is some relation between the two usages. The term is too useful for the Whiteheadians to be allowed to pre-empt it.

the methodological problems involved in the definition of scientific method closely parallel those arising in an attempt to define Napoleon, the industrial revolution, slavery, or any other person or institution having historical actuality. In each such case, what we recognize as the idiosyncracy of the unique historical phenomenon is constituted by a growing together, a concrescence, of variable factors, interacting to produce the degree of unification and contrast with an environment which leads us to recognize a distinct entity.

The technical problems which arise in definitions of concrescence are similar in character to those encountered in the specification of biological or psychological "types." And logicians have already begun to consider the methods of formalization appropriate.[7] To provide a satisfactory definition by concrescence we shall need (a) a description of the main factors engaged in the concrescence, (b) determination of their relative "weight" or importance, and (c) an account of their mode of interaction.

In trying to carry out such a program as this the pervasive difficulty will be that of choosing a proper level of abstraction. The greater formal complexity of definition of concrescence will not exempt it automatically from the danger of overabstraction; the result of our labors may still prove to be a sterile formula, unable, for all its complexity, to influence practice. There is, however, a kind of formulation of principle which is able to avoid this danger.

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[7] See C. G. Hempel and P. Oppenheim, *Der Typusbegriff im Lichte der neuen Logik* (Leyden, 1936).

I shall try to show how this happens by considering an illuminating illustration of the formulation of principles of scientific method.

Claude Bernard's *Introduction to the Study of Experimental Medicine,* first published in 1865,[8] is a classic of the philosophy of science which deserves to be better known in the English-speaking countries. Its title may have misled readers into expecting a technical treatise on physiology; it is in fact an essay on method not unworthy to be classed with that of Descartes. We shall not find here the pretensions to system, arrangement, and thoroughness of more elaborate treatises on scientific method. Here everything is said directly, simply, without pretentiousness or pseudo-profundity; we can almost hear the harpsichord playing in the background. But there is nothing forced or contrived in this elegance; every page is informed with the judgment and educated memory of a superb experimenter. We seem to be always in the presence of a *person,* meditating upon a lifetime's experience of creative research.

The reflections of such a man deserve great respect. I shall select for special attention two aspects of his doctrine which are directly relevant to my present purpose. First, Bernard's fallibilism[9] with respect to scientific theory —a doctrine held by many but never, to my knowledge,

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[8] I shall quote from H. D. Greene's translation, published by Macmillan in 1927.

[9] The term is Peirce's. See *Collected Papers of Charles Sanders Peirce,* ed. Charles Hartshorne and Paul Weiss (6 vols., Cambridge, 1931–35), 1:13, 141–152.

stated better—will point the way for a more radical fallibilism with respect to the principles of scientific method. Second, Bernard's use of determinism as an instrument of criticism and discovery may throw light upon the manner in which principles of method can, in favorable cases, contribute to the progress and extension of science.

Bernard's views about the uncertainty of all scientific theory arise from his analysis of the distinction between experiment and observation and of the part played in both by hypothesis. Experiment, he says, differs from observation in demanding artificially induced variation for the sake of comparison and reasoning.[10] Experiment is relatively active, observation relatively passive; but the most elaborately artificial experiment terminates in simple observation, a submission to the verdict of experience. It is important to notice that because the experimenter *interrogates* nature, every experiment is based upon a "preconceived idea," a hypothesis to be tested.[11] But once the

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[10] "We give the name observer to the man who applies methods of investigation, whether simple or complex, to the study of phenomena which he does not vary and which he therefore gathers as nature offers them. We give the name experimenter to the man who applies methods of investigation, whether simple or complex, so as to make natural phenomena vary, or so as to alter them with some purpose or other, and to make them present themselves in circumstances or conditions in which nature does not show them." Bernard, *Experimental Medicine*, 15.

[11] "It is impossible to devise an experiment without a preconceived idea; devising an experiment, we said, is putting a question; we never conceive a question without an idea which invites an answer." *Ibid.*, 23.

experimental conditions have been set up, the scientists must turn passive again. "Observers, then, must be photographers of phenomena; their observations must accurately represent nature. We must observe without any preconceived idea; the observer's mind must be passive, must hold its peace; it listens to nature and writes at its dictation."[12] Without hypotheses or "preconceived ideas" we should never discover the observations—so that even an erroneous theory is better than none at all.[13] But we must always "be ready to abandon, to alter or to supplant"[14] the hypothesis in the light of the decisive judgment of the observations by which it is tested.

To a generation successfully weaned from Baconian empiricism, Bernard's insistence upon the primacy of fact and the need for theoretical "preconceptions" in research may seem too elementary to call for praise. But a successful marriage of rationalism and empiricism is hard to arrange; and it is of great interest to see how Bernard manages to reconcile his great respect for fact with a thorough belief in the intelligibility of phenomena. He is a rationalist to the finger tips; observation, he says, is *always* made for the sake of generalization and explanation; the disproof of a theory must always be an incentive

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[12] *Ibid.*, 22. See 192; "We must never go beyond facts and must be, as it were, photographers of nature."

[13] "Even mistaken hypotheses and theories are of use in leading to discoveries. . . . It seems, indeed a necessary weakness of our mind to be able to reach truth only across a multitude of errors and obstacles." *Ibid.*, 170.

[14] *Ibid.*, 23.

to theoretical explanation of the discrepancy—"negative facts when considered alone never teach us anything."[15] So it is that, in spite of his confidence in the senses,[16] he does not hesitate to outlaw a fact, if necessary. He says, of a particular instance, "The irrationality of the fact, therefore, led me to see *a priori* that it must be false, and that it could not be used as a basis for scientific reasoning."[17] Yet faith in the ultimate intelligibility and rationality of the universe is controlled and moderated in him by an abiding scepticism concerning *particular* explanations and reasons. Because every theory is an imperfect, and partly arbitrary, extrapolation from observation we must never place unqualified trust in *any* theory. "When we propound a general theory in our sciences, we are sure only that, literally speaking, all such theories are false."[18] "Even when we have a theory that seems sound, it is never more than relatively sound, and it always includes a certain proportion of the unknown."[19] This paradoxical attitude of active scepticism, of an undogmatic and corrigible faith, Bernard sums up in a striking phrase: "We must have robust faith," he says, "and not believe."[20] We must not believe

[15] *Ibid.*, 174.
[16] *Ibid.*, 177.
[17] *Ibid.*, 179.
[18] *Ibid.*, 36.
[19] *Ibid.*, 162.
[20] *Ibid.*, 168. We may be reminded here of Huxley's notion of "tätige skepsis" that he adapted from Goethe. For a similar more recent statement see Morris R. Cohen, *Studies in Philosophy and Science* (New York, 1949), 50: "We may define science as a self-

wholeheartedly in any body of scientific theory; the object of our robust faith is determinism. Determinism, he says, "is the absolute principle of science"[21] and to abandon it is to renounce the hope of being scientific. "In science we must firmly believe in principles, but must question formulae; on the one hand, indeed, we are sure that determinism exists, but we are never certain we have attained it."[22]

It might be supposed that a determinism such as Bernard espoused would be too pliable to serve as a guide and a stimulant to research. It has often been pointed out that determinism, construed as the principle that laws exist of no matter what degree of complexity, is irrefutable in principle. Faced with no matter how much confusion and irregularity of appearance, a determinist can always continue to *look* for laws. Is Bernard's major principle anything more than an expression of his determination to continue to look and to theorize? Does his determinism do any work, or is it merely a regrettable lapse into metaphysics?

Now I am inclined to think that Bernard's determinism, *as he used it*, can be shown to have been an active instru-

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corrective system . . . *science invites doubt*. It can grow or make progress, not only because it is fragmentary, but also because no one proposition in it is in itself absolutely certain, and so the process of correction can operate when we find more adequate evidence. But note, however, that the doubt and the correction are always in accordance with the canons of the scientific method, so that the latter is the bond of continuity."

[21] Bernard, *Experimental Medicine*, 39.

[22] *Ibid.*, 168.

ment of research and criticism. His practice repeatedly shows his faith in ultimate rationality lighting the path to original discovery. When the physicians of his day invoked the tact and intuition of the medical practitioner, Bernard counters with a stubborn search for causal sequences; when other theorists hid their ignorance of biological laws by appealing to some mysterious "vitality" or vital force, he condemns them for irrationality and continues to experiment; and in more specific contexts where less clearheaded experimenters blunder, Bernard's robust faith in a deterministic order of nature fortifies him in a laborious but successful search for rational explanation. Belief in determinism for him was a light for the darkness, not a mere obeisance to an imputed order of nature.

Such an interpretation might easily be wrong; and it is conceivable, though I find myself unable to believe it, that Bernard read his philosophical principles back into earlier researches which at the time of their performance required no better support than the luck and cunning of the investigator. Those who prefer to think that the experimenter is guided mainly by intuition may do so if they can persuade themselves that the term is something better than a disguise for our own ignorance of the creative process. But the general point is sound, and could be supported, if time allowed, by ample illustrations from the history of science. As the individual experiment itself would be nothing without a preconceived idea, however crude and faulty in detail, so would the whole course of research be a mere random succession of fumbles but for the co-ordination of leading ideas. It seems implausible to me that any regulative principle as abstract as determinism in its most general formulation can have decisive influences upon research; and I think it demonstrable that in any epoch of experimental research the leading ideas take more specific forms, varying with the climate of philosophical opinion and the earlier fortunes of the science.[23]

I have been citing Claude Bernard's philosophy of science in support of my contention that cautious extrapolation of methodological principles may be a valuable guide to scientific research. There is, however, much in Bernard's doctrines themselves that would seem open to serious objection.

His conception of the fact of observation as simply given, the observer himself acting as a passive photographer of nature, is hardly adequate. The image itself betrays the intention; for a photograph is not an identical reproduction of the scene it portrays and must itself be correctly interpreted if it is to be a suitable datum for inference. The insistence upon the primacy of what is presented is valuable, but it must be reconciled with the principle, of opposite tendency, to the effect that there is no observation without interpretation; pure observation is a myth. What shall *count* as a fact in any well-developed

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[23] I take this to explain in part Bernard's success in using determinism. Given the special context of physiological experiment in the last century I think he had robust faith, not in general determinism, but rather in the existence of laws and explanations of *a certain kind*.

science is already largely determined by theory embodied in the disposition of the scientific instruments, the selection of "competent" scientists, and the postures of "correct" observation. That Bernard needed to take no account of such complications is explained by the simplicity of his observations.

We cannot share, today, his unqualified faith in determinism. If we are to continue to search for laws we must be prepared to find them complex beyond all the expectations of earlier scientists. Bernard's distrust of statistical generalization—thoroughly justified in his own context—must seem old-fashioned to a generation educated in statistical physics. We must be prepared to mix a little more scepticism with our faith than Bernard was prepared for.

One of the major contributions of this century's philosophy of science has been the clarification and analysis of the symbolic aspects of science. We are beginning to understand how very far from being a literal generalization about observable features of observable phenomena the theories of any advanced science must be taken to be. The more advanced the science, the greater the part played in its theories by unobservables; and the more urgent the task of elucidating the deductive paths connecting such abtruse and recondite entities with the experiences to which, however indirectly, they refer. A well-developed philosophy of science will have much to say in this chapter of its investigations.

We can now begin to see some hope of solving our problem about the choice of overlapping criteria for a definition of science. It will be remembered that I claimed that science, as a historical process, resembled an organism in being a manifestation of variable, interacting factors. Now we have to add that analysis of the character of this process, as it shows itself in the formulation of an explicit methodology, may itself become a factor in the further development. Principles abstracted from the past history may be put to work to help determine its future and themselves become subjected in this way to continuing experimental test. And the same holds for the goals, standards, and ideals which determine the choice, acceptance, and rejection of principles in the light of continuing experimental tests. Looking back at the history of science we choose such principles of method as will seem to answer best to the search for knowledge of the world—as in the light of that examination we feel constrained to envisage what we now call the world and knowledge of it. If our choice is wise, the principles can be set to work to assist in the acquisition of more knowledge; and if the application, however successful, changes our conceptions of what should be the right method—indeed, of what we should regard as "nature," "the world," "fact," "evidence," and the other terms of our philosophical discourse—that is no more than we should be wise to expect.

I am advocating, as you see, an attitude of active scepticism, of faith without belief, toward the very principles of investigation themselves. No doubt, to renounce the support of determinism or any other immutable theoretical certainty is to call for an attitude of mind difficult to sustain. There will be some to object that in the absence of some such underpinning the program is condemned to a futile relativism and, if it be wholehearted, a complete scepticism. "What," it will be said, "in the last analysis, justifies your choice of standards, ideals, criteria, and principles? If the justification lies in the future, then your choice of a scientific method is no better *now* than a blind guess. To be rational *now*, it must itself be based on rational first principles necessarily incapable of justification empirically. You want the advantage of a metaphysics while shirking the labor required to establish it."

The question is too large to be argued fully here. Let me be content to affirm that in the last analysis there *is no* last analysis. If the search for a definition of scientific method is more than an exercise in platitudinous verity or epigrammatic falsity, it is a serious attempt to clarify the relation of our culture to its past in order to bring into sharper focus our commitments to its future. This means starting from the standpoint not of a detached rational being exempt from the influence of his history but from our own standpoint in space and time. We start with given notions, preconceptions, and prejudices about knowledge, evidence, method, and science which it is mere folly to pretend to ignore. What we can do is to render these philosophical preconceptions more rational by testing them against past history and future experience. This is as far removed from blind guessing as the calculation of the meteorologist from the casual weather predictions of the man in the street. But the philosopher of science may be sufficiently humble to expect no higher ratio of success than the weather forecaster; his one advantage in the long run over the advocate of intuition or guesswork is that he may hope to learn from his mistakes.

Since we have introduced the salutary virtue of humility, let me repeat that at best we have here the mere outline of a program. The detailed execution of the program, by which its merits must eventually be judged, is by far the harder undertaking. When we compare our present conception of scientific method with those of Herschel, Whewell, Jevons, Bernard, John Stuart Mill, and the other nineteenth-century writers we may take pride in having made marked advances. Certainly we have a much sounder grasp of the character and importance of the symbolic aspects of scientific method; our conceptions of statistical method, the nature of mathematics, of measurement, and the use of nonobservable theoretical entities have advanced in a way which would astound our not so distant predecessors. It would be self-deception, however, to pretend that light shines everywhere in those intensely cultivated regions, and it is safe to predict that theorists who venture already to lay down definite prescriptions for research in economics, psychology, or the still embryonic social sciences will seem entertainingly doctrinaire to their not so distant successors. The wisest philosophers of science have shown their wisdom not least in shrouding their first principles in protective ambiguity. The best we can hope for is some useful second principles that will prove to be not so vague as to be exempt from refutation by experience still to come, or so hopelessly wrong as to deserve outright rejection when science has suffered its next benign convulsion.

So much ground has been covered in this paper that a summary may be welcome. I began by characterizing definitions of "scientific method" as "persuasive," in a technical sense of that term. "Scientific method," it was contended, is a term of such controversial application that a definition universally acceptable can be expected to be platitudinous. A useful definition will be a controversial one, determined by a choice made, more or less wisely, in the hope of codifying and influencing scientific procedures. It is too much to expect infallible recipes for conducting research, but most definitions in the literature fail to satisfy the more modest demand of helping to determine the development and extension of scientific method. A common defect is excessive abstraction; it was suggested that this arises from conformity to a pattern of definition Aristotelian in origin. The search for an immutable and determinate essence underlying the plenitude of the historical process can result only in epigrammatic paradox. We may do better, I urged, to think of science as a concrescence, a growing together of variable, interacting, mutually reinforcing factors contributing to a development organic in character. The type of definition appropriate takes the form of a description of the constitutive factors, together with an indication of their relative weight or importance and their mutual relationships.

For further light upon the kind of definition that would be satisfactory I turned to Claude Bernard's philosophy of science. He was found to be advocating a blend of rationalism and empiricism, marked by submission to the authority of the results of observation, and an unflagging confidence in the causal structure of the universe. His scepticism with regard to the finality of scientific theory was congenial to the more radical fallibilism advocated in this paper. Though his robust faith in determinism cannot be shared without reservation, the use he made of it as an instrument of criticism encouraged us to hope that methodological principles might have a useful regulative function.

My own contention was that the very principles of scientific method are themselves to be regarded as provisional and subject to later correction, so that a definition of "scientific method" would be verifiable, in some wide sense of the term. To the degree that the definition is framed in the light of our best reflection about past knowledge-seeking activities, with the intention that it shall guide our further pursuit of knowledge in the future, we can properly claim that the procedure is rational. For to be rational is to be always in a position to learn more from experience.

The Meaning of Reduction in the Natural Sciences

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The science of mechanics was the first branch of mathematical physics to assume the form of a comprehensive theory. The success of that theory in explaining and bringing into systematic relation a large variety of phenomena was for a long time unprecedented; and the belief entertained by many eminent scientists and philosophers, sometimes supported by a priori arguments, that all the processes of nature would eventually fall within the scope of its principles was repeatedly confirmed by the absorption of several sectors of physics into mechanics. However, it is now common knowledge that classical mechanics no longer occupies the position of the "universal" physical science once claimed for it; for since the latter part of the nineteenth century the difficulties facing the extension of mechanics to various further domains of physical inquiry have come to be acknowledged as insuperable, and rival candidates for the office of a universal physical science have been proposed. Moreover, with some exceptions, no serious students today believe that some particular physical theory can be established on a priori grounds as the universal or fundamental theory of natural processes; and to many thinkers it is even an open question whether the ideal of a comprehensive theory which would thoroughly integrate all domains of natural science is realizable. Nevertheless, the phenomenon of a relatively autonomous branch of science becoming absorbed by, or "reduced" to, some other discipline is an undeniable and recurrent feature of the history of modern science, and there is no reason to suppose that such reduction will not continue to take place in the future.

It is with this phenomenon that the present paper is concerned. The successful reduction of one science to another, as well as the failures in effecting such a reduction in a number of notable cases, have been occasions, exploited by both practicing scientists and professional as well as lay philosophers, for far-reaching reinterpretations of the nature and limits of knowledge, science, and the allegedly ultimate constitution of things in general. These interpretations take various forms. Discoveries concerning the physics and physiology of perception have been frequently used to support the conclusion that the findings of physics are incompatible with so-called common sense or naïve realism (the belief that things encountered in normal experience do possess the traits which are manifest to controlled observation); and elaborate epistemologies have been proposed for resolving the paradox that, in spite of this presumed incompatibility, science takes its point of departure from, and finds its evidence in, such common-sense knowledge. The successful reduction of thermodynamics to statistical mechanics in the nineteenth century, and the more recent expansion of electrical theories of matter, have been taken to show that spatial displacements are the only form of intelligible and genuine change; that the qualitative and behavioral diversities noted in ordinary experience are "unreal" and illusory; or, conversely, that the "mysterious world" discovered by microscopic physics is but an insubstantial symbol which expresses a pervasive spiritual reality not alien to human values. On the other hand, the failure to explain electrodynamical phenomena in terms of the principles of mechanics, and the general decline of mechanics as the universal science of nature in contemporary physics, has been hailed as evidence for the bankruptcy of classical science, for the necessity of instituting an "organismic" point of view and "organismic" categories of explanation in the study of all natural phenomena, and for a variety of metaphysical doctrines concerning levels of being, emergence, and creative novelty.

I do not believe that these speculative interpretations of the assumed facts of science are warranted by the evidence. On the contrary, I believe that the problems to which they are addressed are generated by misconstruing the statements of the natural sciences and reading them in senses not in accordance with the meanings that actual usage in scientific contexts establishes for those statements. However, it is not my present aim to examine the detailed arguments which lead to the adoption of views such as those just briefly indicated. I wish instead to consider what is done when one science is reduced to another, and to suggest that an important source of much dubious commentary on the nature and the interrelations of the sciences lies in the failure to recognize the conditions which must be fulfilled when such a reduction is effected. It is a commonplace that linguistic expressions, associated with established habits or rules of usage in one set of homogeneous contexts, frequently come to be used in other contexts on the assumption of definite analogies or continuities between the several domains. But judging from the practice of many philosophers and scientists, it is still not a commonplace that when the range of application of expressions is thus extended, these expressions may undergo critical changes in meaning, and that unless care is exercised in interpreting them so that specific contexts of relevant usage are noted, serious misunderstandings and spurious problems are bound to arise. In any event, misconceptions having their basis in just such careless handling of language seem to me to accompany much traditional and current discussion of the significance of scientific reduction. The present essay is an attempt to indicate some quite familiar and yet frequently neglected distinctions that are pertinent to the analysis of this recurrent phenomenon in the development of the natural sciences.

Before turning to my actual theme, it will be useful to distinguish a type of reduction in the history of science which generally, though certainly not always, is unaccompanied by serious misapprehensions. I have in mind the normal expansion of some body of theory, initially proposed for a certain extensive domain of phenomena, so that laws which previously may have been found to hold in a narrow sector of that domain, or in some other domain homogenous in a readily identifiable sense with the first, are shown to be derivable from that theory when suitably specialized. For example, Galileo's *Two New Sciences* was a contribution to the physics of freely falling terrestrial bodies; but when Newton showed that his own general theory of mechanics and gravitation, when supplemented by appropriate boundary conditions, entailed Galileo's laws, the latter were incorporated into the Newtonian theory as a special case. Were we to regard this branch of inquiry cultivated by Galileo as a distinctive science, the subsequent facts could be described by saying that Galileo's special discipline was reduced to the science of Newton. However, although it is possible to distinguish the subject matters of the Newtonian and the (initially distinct) Galilean sciences (for example, the latter was concerned solely with terrestrial phenomena, while the former included celestial ones), these subject matters are in an obvious sense homogenous and continuous; for it is the motions of bodies and the determinants of such motions that are under investigation in each case, and in each case inquiry is directed toward discovering relations between physical traits that are the common concern of both disciplines. Stated more formally, the point is that no descriptive terms appear in the formulations of the Galilean science which do not occur essentially and with approximately the same meanings in the statements of Newtonian mechanics. The history of science is replete with illustrations of reductions of this type, but I shall ignore them in what follows, because the logical issues involved in them do not appear to generate typical forms of philosophic puzzlement or to stimulate fundamental reinterpretations of the nature of knowledge.

The situation seems to be quite different, however, in those cases of reduction in which a subject matter possessing certain distinctive properties is apparently assimilated with another that supposedly does not manifest those traits; and acute intellectual discomfort is often experienced in those instances of reduction in which the science that suffers reduction is concerned with so-called "macroscopic" or "molar" phenomena, while the science to which the reduction is effected employs a theory that postulates some "microscopic" structure for molar physical systems. Thus, consider the following example. Most adults, if provided with ordinary mercury thermometers, are able to determine with reasonable accuracy the temperatures of various bodies, and understand what is meant by such statements as that the temperature of a glass of milk is 10° C. Accordingly, such individuals know how to use the word "temperature," at any rate within a broad context, though doubtless a large fraction of them would be incapable of stating adequately the tacit rules governing such usage, or of explicating the meaning of the word to the satisfaction of someone schooled in thermodynamics. However, if such an individual were to use the word so that its application was always associated with the behavior of a mercury column in a glass tube when the latter was placed in proximity to the body whose temperature was in question, he might be at a loss to construe the sense of such a statement as that the temperature of a certain substance at its melting point is several thousand degrees high; and he might protest that since at such alleged "high temperatures" ordinary thermometers would be vaporized, the statement had no definite meaning for him. But a slight study of physics would readily remove this source of puzzlement. The puzzled individual would discover that the word "temperature" is associated with a more inclusive set of rules of usage than he had originally supposed, and that in its extended usage it refers to a physical state of a body, which may be manifested in other ways than in the volume expansion of a mercury column—for example, in changes in electrical resistance, or in the generation of electric currents. Accordingly, once the laws are understood which connect the behavior of ordinary thermometers with the behavior of bolometers, pyrometers, and other overtly identifiable recording instruments, the grounds for the more inclusive usage of the term "temperature" become intelligible. This wider use of the word, then, rarely appears to cover a mystery, any more than does the extension of the word from its uses in contexts of direct experience of hot and cold to contexts in which the mercury thermometer replaces the human organism as a test body.

Suppose, however, that the layman for whom the word "temperature" thus acquires a more generalized meaning than he originally associated with it now pursues his study of physics into the kinetic theory of matter. Here he discovers that the temperature of a body is simply the mean kinetic energy of the molecules constituting the body. But this bit of information usually produces renewed perplexity, and, indeed, in an especially acute form. For the layman is assured by the best authorities that while on the one hand individual molecules possess no temperatures, nevertheless the meaning of the word "temperature" must by definition be taken as identical with the meaning of such expressions as "energy of molecular motions." And questions that are typical of a familiar philosophical tradition now seem both relevant and inescapable. If the meaning of "temperature" is the same as that of "kinetic energy of molecular motion," what are we talking about when milk is said to have a temperature of 10° C.? Surely not the kinetic energies of the molecular constituents of the liquid, for the uninstructed layman is able to understand what is thus said without possessing any notions about the molecular composition of milk. Perhaps, then, the familiar distinctions between hot and cold, between various temperatures as specified in terms of the behavior of identifiable instruments, are distinctions which refer to a domain of illusion. Perhaps, also, the temperatures that are measured in ordinary experience as well as in laboratories are merely indications of some fundamental underlying reality which is inherently incapable of being characterized by such expressions as "temperature" understood in its customary sense. Or should we perhaps regard temperature as an emergent trait, not present on lower levels of physical reality? But if this is the correct way of viewing the matter, does a theory that is about such lower levels ever really explain emergent traits such as temperature? It would be easy to enlarge the list of such queries, but those cited suffice to suggest the general character of the instances of reduction which provoke them. To avoid repeated circumlocution, and for lack of better labels, let me refer to a science to which another is reduced as the "primary science," and to the science which suffers such reduction as the "secondary science." Philosophical problems of the sort indicated, then, seem to be generated when the subject matter of the primary science is "qualitatively discontinuous" or "in-homogenous" with the subject matter of the secondary science—or, to put the matter perhaps more clearly, when the statements of the secondary science contain descriptive terms that do not occur in the theories of the primary science.

It is reductions of this type that I wish to consider. And since the reduction of thermodynamics to mechanics, more exactly, to statistical mechanics and the kinetic theory of matter, is both a typical and a relatively familiar and simple example of this type, I propose to center my discussion around this illustration.

I will first briefly recall some well-known historical facts. The study of thermal phenomena goes back in modern times to Galileo and his circle, and during the subsequent three centuries a large number of laws were established dealing with special phases of the thermal behavior of bodies—laws which were eventually exhibited as systematically interrelated on the basis of a small number of general principles. Thermodynamics, as this science came to be called, employed concepts, distinctions, and general laws which were also used in mechanics—for example, the notions of volume, weight, mass, and pressure, and laws such as the principle of the lever and Hooke's Law. Nevertheless, it was regarded as a science relatively autonomous with respect to mechanics, because it made use of such distinctive notions as temperature, heat, and entropy, and because it assumed laws and principles which were not corollaries of the fundamental assumptions of mechanics. Accordingly, though many propositions of mechanics were constantly employed in the exploration of thermal phenomena, thermodynamics was generally assumed for a long time to be a special discipline, plainly distinguishable from mechanics and not simply a chapter of it. In this respect, the relation of thermodynamics to mechanics was considered analogous to the relation between mechanics and physical geometry: mechanics was held to be distinguishable from physical geometry, even though geometrical propositions were employed in the formulation of mechanical laws and in the construction of instruments used to test these laws. Indeed, thermodynamics is still frequently expounded as a physical theory that is autonomous in the indicated sense with respect to mechanics; and in such expositions the findings of the science are presented in such a manner that the propositions asserted can be understood and verified in terms of explanations and procedures which do not assume the reducibility of thermodynamics to some other theory. However, experimental work early in the nineteenth century on the mechanical equivalent of heat stimulated theoretical inquiry to find a more intimate connection between thermal and mechanical phenomena than the bare facts seemed to assert. And when Maxwell and Boltzmann were finally able to "derive" the Boyle-Charles Law from assumptions apparently statable in terms of mechanics concerning the molecular constitution of ideal gases, and especially when the entropy principle was shown to be capable of interpretation as a statistical law concerning the aggregate mechanical behavior of molecules, thermodynamics was widely believed to have lost its autonomy and to have been reduced to mechanics.

Just how is this reduction effected, and what is the argument which apparently makes possible the derivation of statements containing such terms as "temperature," "heat," and "entropy" from a set of theoretical assumptions that do not use or mention them? It is not possible, without producing a treatise on the subject, to exhibit the complete argument. I shall therefore fix my attention on a small fragment of the complicated analysis, the derivation of the Boyle-Charles Law for ideal gases from the assumptions of the kinetic theory of matter.

Suppressing most of the details that do not contribute directly to the clarification of the main issues, a simplified form of the derivation is in outline as follows. Assume an ideal gas to occupy a volume V. The gas is taken to be composed of a large number of molecules possessing equal mass and size, each perfectly elastic and with dimensions that are negligible when compared with the average distance between them. The molecules are further supposed to be in constant relative motion, and subject only to forces of impact between themselves and the walls of the containing volume, also taken to be perfectly elastic. Accordingly, the motions of the molecules are assumed to be analyzable in terms of the principles of Newtonian mechanics. The problem now is to determine the relation of the pressure which the molecules exert on the walls of their container to other aspects of their motion.

However, since the instantaneous co-ordinates of state of the individual molecules are not actually ascertainable, the usual mathematical procedure of classical mechanics cannot be applied; and in order to make headway with the problem, a further assumption must be introduced—an assumption which is a statistical one concerning the positions and momenta of the molecules. This statistical assumption takes the following form. Suppose that the volume V of the gas is subdivided into a very large number of smaller volumes whose dimensions are equal but nevertheless are large compared with the diameters of the molecules; suppose also that the maximum range of velocity of the molecules is divided into a large number of equal intervals of velocity; and associate with each small volume all possible velocity intervals, calling each complex obtained by associating a volume with a velocity interval a "phase-cell." The statistical assumption then is that the probability of a molecule's occupying an assigned phase-cell is the same for all molecules and phase-cells, and that (subject to certain qualifications which need not be mentioned here) the probabilities that any pair of molecules will occupy the same phase-cell are independent. From this set of assumptions it is now possible to deduce that the pressure p which the molecules exert on the walls of the container is related in a definite way to the mean kinetic energy E of the molecules of the gas, and that in fact $p = 2E/3V$, or $pV = 2E/3$. But a comparison of this equation with the Boyle-Charles Law (according to which $pV = kT$, where k is constant for a given mass of gas and T its absolute temperature), suggests that the latter could be deduced from the assumptions mentioned, *if* temperature were "identified" with the mean kinetic energy of molecular motions. Accordingly, let us adopt this "identification" in the form of the hypothesis that $2E/3 = kT$ (i.e., that the absolute temperature of an ideal gas is proportional to the mean kinetic energy of the molecules which are assumed to constitute it). The Boyle-Charles Law is then a logical consequence of the general principles of mechanics, when these are supplemented by a statistical postulate on the motions of molecules constituting a gas, a hypothesis on the connection between temperature and kinetic energy, and various further assumptions that have been indicated.

If the derivation of the Boyle-Charles Law is used as a basis for generalization, what are the essential requirements for reducing one science to another? The following comments fall into two groups, the first dealing with matters that are primarily of a formal nature, the second with questions of an empirical character.

1. In the first place, the derivation requires that all the assertions, postulates, or hypotheses of each of the sciences involved in the reduction are available in the form of explicit statements, whose meanings are assumed to be fixed in terms of procedures and rules of usage appropriate to each discipline. Moreover, the statements within each science fall into a number of fairly distinct groups when a classification is introduced on the basis of the logical functions the statements possess in the discipline. The following schematic list, though not exhaustive, indicates what I believe to be the more important groupings.

(a) In a highly developed science such as mechanics there usually is a class T of statements which constitute the fundamental theory of the discipline and thus serve as principles of explanation and as partial premises in most deductions undertaken in the science, e.g., the principles of Newtonian mechanics. In a given exposition of the science, these statements are logically primitive, in the sense that they are not derived from any other class of statements in the science. Whether this class of statements is best conceived as a set of leading principles, empirical rules of inference, or methodological rules of analysis, rather than as premises in the usual sense of the word, is a question that can be ignored here.

(b) A science which contains a fundamental theory will also contain a class of statements or theorems which are logically derivable from T. These theorems in all but trivial cases are usually of a conditional form, and their consequents are derivable from *T* only if the latter is supplemented by various special assumptions which appear as the antecedents in the theorems. Two subdivisions of this class of special assumptions may be distinguished. (i) There is the group of assumptions which serve as general hypotheses concerning a variety of conditions to which the fundamental theory may be applied. Thus, one such assumption in the application of Newtonian principles to the study of gases is that of a physical system composed of a large number of point-masses, with forces of impact as the only forces present. An alternative assumption might be that of a physical system consisting of bodies with non-negligible diameters subject to gravitational forces. (ii) And there is also the group of assumptions which specify the detailed boundary or initial conditions for the application of the theory. Thus, in the above example the initial conditions are stated as a statistical assumption concerning the position and velocities of gas molecules.

(c) Finally, every positive science will contain a large class of singular statements which formulate procedures and the outcome of observations relevant for the conduct of inquiry in the science; and it will usually also make use of general laws which its fundamental theory does not pretend to explain but which are simply borrowed from some other special discipline. Call the first group of these statements "observation statements," and the second group "borrowed statements." Observation statements may on occasion serve as specifications of the initial conditions for the application of the theory, or they may state the predicted consequences of the theory when other such statements are used to supplement the latter as initial conditions. Accordingly, observation statements will normally have members in common with the class of statements of boundary and initial conditions, though in general these two classes will not coincide. Indeed, many observation statements will describe instruments required for testing general assumptions of the science, and in doing so may make use of general laws and hence of expressions referring to distinctions that fall within the province of some other specialized discipline. For example, if Newtonian assumptions are employed in the study of celestial phenomena, telescopes may be required to test these assumptions; but the description of telescopes, and the interpretation of the observations that are obtained through their use, generally involves the use of expressions that refer to distinctions studied primarily in theoretical optics rather than in Newtonian mechanics.

2. This brings me to my second formal observation. The statements of a science, to whichever of the above classes they may belong, can be analyzed as linguistic structures compounded out of more elementary expressions in accordance with tacit or explicit rules of construction. These elementary expressions *E* are of various sorts, but they may be assumed to have fairly definite meanings fixed by habit or explicit rules of usage. Some of them are the familiar expressions of logic, arithmetic, and perhaps

higher mathematics; but most of them will usually be so-called "descriptive" terms or combinations of terms which signify allegedly empirical objects, traits, and processes.

Though there may be serious difficulties both theoretical and practical in distinguishing descriptive expressions from others, let us suppose that the distinction can be carried through in some fashion, and let us consider the class of descriptive expressions in *E*. Many of the descriptive expressions of a science are taken over from the language of ordinary affairs and retain their customary, everyday meanings; others, however, may be specific to the science, and may, moreover, have meanings which preclude their application to matters of familiar experience. Thus the statements constituting the fundamental theory of a science, as well as many of the special assumptions which are used to supplement the theory in various ways, normally contain several descriptive expressions of this latter sort.

Now it is generally possible to explicate the meanings of many descriptive expressions in *E* with the help of other such expressions, though of course logical expressions will play a role in the explication. Let us refer to those descriptive expressions with the help of which the meanings of all other such expressions may be explicated —whether the explication is given in the form of conventional explicit definition or through the use of different and more complicated logical techniques—as the "primitive expressions" of the science. (Expressions that are primitive in this sense may be primitive only in some specific context of analysis and not in another. But this point, though not without importance for a general theory of definition, does not affect the present discussion.)

However, the explication of the meaning of an expression may have either of two objectives, and accordingly it is useful to distinguish between two classes of primitive expression. (a) The explication may aim at specifying the meaning of an expression in terms familiar from everyday usage; and in consequence, the primitives employed may be restricted to those expressions which refer to matters of common observation, laboratory procedure, and other forms of overt behavior. Call such primitives "experiential primitives," even if no sharp line may be drawn between expressions that are experiential and those that are not. For example, the meaning of the word "temperature" is often specified by means of statements describing the volume expansion of liquids and gases, or the behavior of other readily observable bodies; and in this instance the primitives employed in the explication are experiential ones.

(b) On the other hand, an explication may aim at specifying the meaning of an expression by exhibiting its relation to the meanings of expressions used in formulating the fundamental theory or the various supplementary assumptions of the science. And in consequence, the primitives employed may in fact contain no expression which refers to matters accessible to direct observation. Call such primitives the "theoretical primitives" of the science. For example, the meaning of the word "temperature" is sometimes specified with the help of statements describing the Carnot cycle of heat transformations, statements which contain expressions like "perfect nonconductor," "infinite heat-reservoir," and "infinitely slow volume expansion," that have no manifest reference to anything that is observable. Again, the explication of the expression "center of mass," as customarily given in treatises on mechanics, involves the use of other expressions that are basic in formulating the principles of mechanics, though they do not all refer to directly observable characteristics of bodies.

It is not necessary to decide, for the purpose of the present discussion, whether the meanings of all theoretical primitives of a science are explicable with the help of its experiential primitives. And though the class of theoretical primitives of a discipline and the class of its experiential primitives may have expressions in common, the two do not in general coincide.

3. I come to my third comment of a formal nature. A comparison of the statements belonging to the primary science involved in a reduction with those belonging to the secondary science shows that in general the two sciences share a number of common statements and expressions, the fixed meanings of these expressions being the same for both sciences. Statements certifiable in logic and demonstrative mathematics are obvious examples of such common expressions, but, in addition to them, the two sciences will frequently share statements and other expressions which have a descriptive or empirical content. For example, many

propositions that fall within the field of mechanics, such as the law of the lever, also enjoy important uses in thermodynamics, as one of the borrowed statements of the latter science; and thermodynamics also employs such expressions as "volume," "weight," and "pressure" in senses which coincide with the meanings of these words in mechanics. On the other hand, the secondary science prior to its reduction generally contains statements and expressions not occurring in the primary science, except possibly as members of the class of observation and borrowed statements. For example, theoretical mechanics in its classical form contains neither the Boyle-Charles Law nor the word "temperature," though both of these occur in thermodynamics, and though the word may on occasion be employed in statements which describe the conditions of application of the first principles of mechanics.

Now it is of the utmost importance to observe that expressions peculiar to a science will possess meanings that are fixed by its *own* procedures, and that are therefore intelligible in terms of its own rules of usage, whether or not the science has been or will be reduced to some other discipline. In many cases, to be sure, the meanings of some expressions in a science can be explicated with the help of those occurring in another, and, indeed, even with the help of the theoretical primitives of the latter. For example, it is usually assumed that an analytical equivalence can be exhibited between the word "pressure" as employed in thermodynamics and other expressions belonging to the class of theoretical primitives in the science of mechanics. But it obviously does *not* follow that every expression used in a sense that is specified in a given science must or need be explicable in terms of the primitives, whether theoretical or experiential, of another discipline.

Let us finally consider what is formally required for the reduction of one science to another. The objective of the reduction is to show that the laws or general principles of the secondary science are simply logical consequences of the assumptions of the primary science. However, if these laws contain expressions that do not occur in the assumptions of the primary science, a logical derivation is clearly impossible. Accordingly, a necessary condition for the derivation is the explicit formulation of suitable relations between such expressions in the secondary science and expressions occurring in the premises of the primary discipline.

Now it may be possible to explicate the meaning of an expression occurring in a law of the secondary science in terms of the experiential primitives of the primary one, especially if, as is perhaps normally the case, the experiential predicates of the two sciences are the same. But this possibility is not in general sufficient for the purposes of reduction, since the problem here is to establish a certain kind of connection between expressions that occur in the secondary science but not in the premises of the primary discipline and expressions that do appear in these premises, especially those expressions of the latter class in terms of which the fundamental theory of the primary science is formulated. For though the uses of each of two expressions may be specifiable with the help of a common set of experiential primitives, it by no means follows that one of the expressions must be definable in terms of the other. The words "uncle" and "grandfather," for instance, are each definable in terms of "male" and "parent," but "uncle" is not definable in terms of "grandfather." Accordingly, a crucial step in reduction consists in establishing a proper kind of relation—that is, one which will make possible the indicated logical derivation—between expressions occurring in the laws of the secondary science and the theoretical primitives of the primary science.

There appear to be just two general ways of doing this. One is to show that an expression in question is logically related, either by synonymity or entailment, to some expression in the premises of the primary science. In consequence, the meaning of the expression in the secondary science, as fixed by the usage established in this discipline, must be explicable in terms of the theoretical primitives of the primary science. The other way is to adopt a material or physical hypothesis according to which the occurrence of the properties designated by some expression in the premises of the primary science is a sufficient, or a necessary and sufficient, condition for the occurrence of the properties designated by the expression of the secondary discipline. But in this case the meaning of the expression in the secondary science, as fixed by the established usages of the latter, is not declared to be analytically related to the established meaning of the corresponding expression in the primary science. In consequence, the indicated hypothesis cannot be asserted on the strength of purely logical considerations, but is at best a contingent truth requiring support from empirical data.

Let us now assume that the word "temperature" is the only expression that occurs in the Boyle-Charles Law which does not also occur in the various premises of mechanics and the kinetic theory of gases from which the law is to be derived. Accordingly, if the deduction is to be possible, an additional assumption must be introduced—the assumption that temperature is proportional to the mean kinetic energy of the gas molecules. How is this assumption to be understood, and in particular what sort of considerations support the indicated connection between the word "temperature" and the expression "mean kinetic energy"? But it is clear that in the sense in which "temperature" is used in thermodynamics, the word is neither synonymous with "mean kinetic energy" nor is its meaning entailed by the meaning of the latter expression. For it is surely not by analyzing the meaning of "temperature," in its thermodynamical sense, that the additional assumption required for deducing the Boyle-Charles Law from the premises of mechanics can be established. This additional assumption is evidently an empirical hypothesis, which postulates a determinate factual connection between two properties of physical systems that are in principle independently identifiable—between temperature as specified in thermodynamics on the one hand and the state of having a certain mean kinetic energy on the other; and if the hypothesis is true, it is at best only contingently true.

One objection to this last claim must be briefly considered. It is well known that though an expression may possess a certain fixed meaning at one stage in the development of inquiry, the redefinition of expressions is a recurrent feature in the history of the sciences. Accordingly, so the objection runs, while in an earlier usage the word "temperature" possessed a meaning which was specified by the procedures of thermometry and classical thermodynamics, it is now so used that temperature is "identical by definition" with molecular motion. The deduction of the Boyle-Charles Law does not therefore require that the premises of mechanics be supplemented with a contingent physical hypothesis but simply makes use of this definitional identity. This objection seems to me to illustrate the curious double talk of which highly competent scientists are sometimes guilty, to the detriment of essential clarity. It is obviously possible to so redefine the word "temperature" that it becomes synonymous with "mean kinetic energy of molecules." But it should be no less obvious that on this redefined usage, the word has a different meaning from the one associated with it on the basis of the usage customary in thermometry and thermodynamics, and in consequence a different meaning from the one associated with it in the Boyle-Charles Law. If, then, thermodynamics is to be reduced to mechanics, it is temperature in the sense specified in the former science which must be shown to be connected with mean kinetic energy. Accordingly, if the word "temperature" is redefined as proposed, the hypothesis must be adopted that the state of bodies described by the word "temperature" in its thermodynamical meaning is also correctly characterized by the word "temperature" in its redefined and different sense. But then this hypothesis is one which does not hold simply by definition. And unless it is adopted, it is not the Boyle-Charles Law which is derived from the premises of mechanics; what is derived is a sentence with a physical and syntactical structure similar to the law, but with a sense that is entirely different from what the law asserts.

I now turn to my second set of comments, those concerned with matters that are not primarily formal.

1. Thus far, I have been arguing the doubtless obvious point that the reduction of one science to another is not possible unless the various expressions occurring in the laws of the former also appear in the premises of the latter. But it is perhaps equally evident that these premises must satisfy further conditions if a proposed reduction is to count as an important scientific achievement. For if the premises of an alleged primary science could be selected quite arbitrarily, subject only to the formal requirements that have been mentioned thus far, the logical deduction of the laws of a secondary science from such premises selected *ad hoc* would in most cases represent only a trivial scientific accomplishment. And in point of fact, an essential condition that is normally imposed upon the assumptions of the primary science is that they be supported by empirical evidence possessing some measure of adequacy. The issues raised by this requirement, and especially the problems

connected with the notion of adequate evidence, cannot be discussed in the present paper, and in any case are not pertinent exclusively to the analysis of reduction. However, a few brief reminders bearing on this requirement that are especially relevant to the reduction of thermodynamics to mechanics may contribute something to the present analysis.

It is well known that the general assumption according to which physical bodies in different states of aggregation are systems of molecules is confirmed by a large number of well-established experimental facts of chemistry and of molar physics, facts which are not primarily about thermal properties of bodies. Accordingly, the adoption of this hypothesis for the special task of accounting for the thermal behavior of gases is in line with the normal strategy of the natural sciences to extend the use of ideas fruitful in one set of inquiries into related domains. Similarly, the fundamental principles of mechanics, which serve as partial premises in the reduction of thermodynamics to mechanics, are supported by evidence drawn from many fields of study distinct from the study of gases. The assumption that these principles characterize the behavior of the hypothetical molecular constituents of a gas thus involves what is essentially the extrapolation of a theory from domains in which the theory has been well confirmed to another domain whose relevant features are postulated to be homogenous with those of the former domains. But in addition to all this, it is especially noteworthy that the combined set of assumptions employed in the reduction of thermodynamics to mechanics, including the special hypothesis on the connection of temperature and kinetic energy, make it possible to bring into systematic relations a large number of propositions on the behavior of gases as well as of other bodies, propositions whose factual dependence on one another might otherwise not have become evident. Many of these propositions were known to be in approximate agreement with experimental facts long before the reduction was effected, but some of them, certainly, were discovered only subsequently to the reduction, and partly as a consequence of the stimulus to inquiry which the reduction supplied.

This last point needs to be stressed. It is fairly safe to maintain that the mere deduction of the Boyle-Charles Law from the assumptions of mechanics does not provide critical evidence for those assumptions, and especially for the assumption on the connection between temperature and kinetic energy, for prior to the reduction this law was already known to hold, at least approximately, for most gases far removed from their points of liquefaction. And though the adoption of those assumptions does effect, in consequence of the mere deduction of the law, a unification of physical knowledge, the unification is obtained on the basis of what to many practicing scientists seems an *ad hoc* postulation. The crucial evidence for those assumptions, and therefore for the scientific importance of the reduction, appears to come from two related lines of inquiry: the deduction from these assumptions of hitherto unknown connections between observable phenomena, or of propositions which are in better agreement with experimental findings than any that had been previously accepted; and secondly, the evaluation, from data of observation, of various constants or parameters that appear in the assumptions, with the proviso that there is good agreement between the values of a constant calculated from data obtained from independent lines of inquiry. For example, though the Boyle-Charles Law holds approximately for ideal gases, most gases under all but exceptional circumstances do not behave in accordance with it. On the other hand, if some of the assumptions used in the deduction of the law from mechanics are modified in a manner not radically altering their main features—specifically, if molecules are assumed to have diameters that are not negligible in comparison with the mean distances separating them, and if cohesive forces between molecules are also postulated—the proposition known as Van der Waal's equation can be derived, which is in much closer approximation to the actual behavior of most gases than is the Boyle-Charles Law. Again, to illustrate the second type of evidence generally accepted as critical for the importance of the reduction of thermodynamics to mechanics, one of the assumptions involved in that reduction is that under conditions of standard pressure and temperature equal volumes of a gas contain an equal number of molecules, quite irrespective of the chemical nature of the gas. Now the number of molecules contained in a liter of a gas (Avogadro's number) can be calculated on the basis of data obtained from observations, though to be sure only if these data are interpreted in a specified manner; and it turns out that alternative ways of calculating this number yield estimates that are in good agreement with one another, even when the measurements which serve as the basis of the calculations are obtained from the study of quite different materials—e.g., Brownian movements and crystal structure, as well as thermal phenomena.

2. These admittedly sketchy remarks on the character of the empirical evidence which supports the assumptions of a primary science merely hint at the complex considerations that are actually involved in judging whether a proposed reduction of one science to another is a significant advance in the organization of knowledge or whether it is simply a formal logical exercise. However, these remarks will perhaps help make plain that even though a science continues to be distinguished from other branches of inquiry on the basis of the general character of its fundamental theory, it may with the progress of inquiry modify or supplement the details of many of its subordinate and yet still quite general assumptions.

And this brings me to my next comment. For if this last point is well taken, it is clear that the question whether a given science is reducible to another needs to be made more explicit by the introduction of a definite date. No practicing physicist will take seriously the claim that, say, electrodynamics is reducible to mechanics—even if the claim were accompanied by a formal deduction of the equations of electrodynamics from a set of assumptions that by common consent are taken to fall within mechanics—unless these assumptions are warranted by independent evidence available at the time the claim is made. It is thus one thing to say that thermodynamics is reducible to mechanics when the latter includes among its assumptions certain hypotheses on the behavior of molecules, and quite a different thing to claim that the reduction is possible to a science of mechanics that does not countenance such hypotheses. More specifically, thermodynamics can be reduced to a mechanics that postdates 1866, but it is not reducible to a mechanics as this science was conceived in 1700. Similarly, a certain part of chemistry is reducible to a post-1925 physical theory, though not to the physical theory of a hundred years ago.

In consequence, much traditional and recent controversy over the interrelations of the various special sciences and concerning the supposed limits of the explanatory power of physical theory can be regarded as a debate over what at a given time is the most promising line of research and scientific advance. Thus, biologists who insist upon the importance of an "organismic" theory of biological behavior and who reject "machine-theories" of living structures may be construed as maintaining, though by no means always clearly, that in the present state of physical and biological theory it is advantageous to conduct their inquiries without abandoning distinctions peculiar to biology in favor of modes of analysis typical of modern physics. On the other hand, the mechanists in biology can be understood as recommending, though often in the language of a dogmatically held ultimate philosophy, a general line of attack on biological problems which in their opinion would advance the solution of these problems and at the same time hasten the assimilation of biology to physics—even if the physics to which biology may eventually be reduced may differ from the present science of physics in important though unspecified respects. However this may be, if the controversy over the scope of physics is conceived in this manner, no major philosophical or logical issue appears to be raised by it, though subsidiary questions involved in the controversy may require logical clarification. If one takes sides in the debate, one is primarily venturing a prediction, on what are often only highly conjectural grounds, as to what will be the most fertile avenue of exploration in a given subject matter at a given stage of the development of several sciences. On the other hand, when such controversies overlook the fact that the reduction of one science to another involves a tacit reference to a date, they assume the character of typically irresoluble debates over what are alleged to be metaphysical ultimates; and differences and similarities between departments of inquiry that may possess only a temporary autonomy with respect to one another come to be cited as evidence for some immutably final account of the inherent nature of things.

3. These last remarks have prepared the way for my final comment. Unlike the present discussion, which views the reduction of one science to another in terms of the logical connections between certain empirically confirmed statements of the two sciences, analyses of reduction and of the relations between sciences in general frequently approach these questions in terms of the possibility or im-

possibility of deducing the properties of one subject matter from the properties of another. Thus, a contemporary writer argues that because "a headache is not an arrangement or rearrangement of particles in one's cranium" and our sensation of violet is not a change in the optic nerve, psychology is demonstrably an autonomous discipline; and accordingly, though the mind is said to be connected with physical processes, "it cannot be reduced to those processes, nor can it be explained by the laws of those processes." Another recent writer, in presenting the case for the occurrence of "genuine novelties" in the inorganic realm, warns that "it is an error to assume that *all* the properties of a compound can be deduced solely from the nature of its elements." And a third influential contemporary author asserts that the characteristic behavior of a chemical whole or compound, such as water, "*could* not, even in theory, be deduced from the most complete knowledge of the behavior of its components, taken separately or in other combinations, and of their properties and arrangements in this whole."

Such an approach to the question almost invariably transforms what is eminently a logical and empirical problem, capable in principle of being resolved with the help of familiar scientific methods and techniques, into a speculative issue that becomes the concern of an obscure and inconclusive dialectic. And in any case, formulations such as those just cited are highly misleading, in so far as they imply that the reduction of one science to another deprives any properties known to occur of a status in existence, or in so far as they suggest that the reducibility of one science to another can be asserted or denied without reference to the specific theories actually employed in a primary science for specifying the so-called "natures" of its ostensible elements.

It is clearly a slipshod formulation, and at best an elliptic one, which talks about the "deduction" of properties from one another—as if in the reduction of one science to another one were engaged in the black magic of extracting one set of phenomena from others incommensurably different from the first. Once such an image is associated with the facts of scientific reduction, the temptation is perhaps irresistible to read these facts as if in consequence some characters of things were "unreal" and the number of "genuine" properties in existence were being diminished. And it is simply naïveté to suppose that the natures of the various hypothetical objects assumed in physics and chemistry can be ascertained once and for all and by way of a direct inspection of those objects, so that in consequence it is possible to establish for all time what can or cannot be deduced from those natures. To the extent that one bases one's account of these matters on the study of scientific procedure, rather than on the frequently loose talk of scientists, it is plain that just as the fundamental nature of electricity is stated by Maxwell's equation, so the natures of molecules and atoms and of the properties of these postulated objects are always specified by a more or less explicitly articulated theory or set of general statements.

It follows that whether a given set of properties or behavioral traits of macroscopic objects can be explained by or reduced to the properties and behavioral traits of atoms and molecules is in part a function of the theory that is adopted for specifying the natures of the latter. Accordingly, while the deduction of the properties studied by one science from those of another may not be possible if the latter discipline postulates certain properties for its elements in terms of one theory, the reduction may be quite feasible when a different theory is adopted for specifying the natures of the elements of the primary science. Thus, to repeat in the present context a point already made, if the nature of molecules is stipulated in terms of the theoretical primitives and assumptions of classical mechanics, the reduction of thermodynamics to mechanics is possible only if an additional hypothesis is introduced connecting temperature and kinetic energy. But as has been seen, the impossibility of the reduction without some such special hypothesis follows from purely formal considerations, and not from some alleged ontological hiatus between the microscopic and the macroscopic, the mechanical and the thermodynamical. Laplace was thus demonstrably in error when he imagined a divine intelligence that could foretell the future in every detail on the basis of knowing simply the instantaneous positions and momenta of all material particles as well as the magnitudes and directions of the forces acting between them. At any rate, Laplace was in error if his divine intelligence is assumed to draw inferences in accordance with the canons of logic, and is therefore assumed to be incapable of the blunder of asserting a statement as a conclusion if it contains expressions not occurring in the premises.

The question whether genuine novelties occur in nature when elements combine to form complex structures is clearly ambiguous. It can be construed as asking whether properties may not occur from time to time which have never before appeared anywhere in the cosmos. And it can also be understood as asking whether properties exhibited by various bodies assumed to be complex are in some cases at least different from and irreducible to the properties of their constituents. The question in the first sense clearly raises a problem in history which requires to be resolved with the help of the normal methods of historical inquiry; and the considerations raised in the present paper are not directly relevant to it. But the question in the second sense does call for a brief comment at this place. For the issue whether the properties of complexes are novel, in the nontemporal sense of the word, in relation to the properties of their elements, appears to be identical with the issue whether statements about the former are reducible to a primary science which deals with the latter. And if this is so, then the question whether the reduction is possible—and whether the properties alleged to be novel are indeed as thus described—cannot be discussed without reference to the specific theory which formulates the nature of the elements and of their properties. Failure to observe that novelty is a relational characteristic of properties with respect to a definite theory, and the supposition that on the contrary certain properties of compounds are inherently novel relative to the properties of the elements, irrespective of any theory which may be used to specify these elements and their properties, are among the chief sources for the widespread tendency to convert the analytic truths of logic into the dogmas of a footless ontology.

The chief burden of this paper, accordingly, is that the reducibility or irreducibility of a science is not an absolute characteristic of it. If the laws of chemistry—e.g., the law that under certain specified conditions, hydrogen and oxygen combine to form a stable compound, which in turn exhibits certain modes of behavior in the presence of other chemical substances—cannot be systematically deduced from one theory of atomic structure, they may be deducible from an alternate set of assumptions concerning the natures of chemical elements. Indeed, although not so long ago such a deduction was regarded as impossible—as it indeed was impossible from the then accepted physcal theories of the atom—the reduction of various parts of chemistry to the quantum theory of atomic structure now appears to be making steady if slow headway, and only the stupendous mathematical difficulties involved in making the relevant deductions from the quantum-theoretical assumptions seem to stand in the way of carrying the task through to completion. At the same time, the reduction of chemical law to contemporary physical theory does not wipe out, or transform into a mere appearance, the distinctions and the types of behavior which chemistry recognizes. Similarly, if and when the detailed physical, chemical, and physiological conditions for the occurrence of headaches are ascertained, headaches will not thereby be shown to be nonexistent or illusory. On the contrary, if in consequence of such discoveries a portion of psychology will have been reduced to another science or to a combination of other sciences, all that will have happened is that the occurrence of headaches will have been explained. But the explanation will be of essentially the same sort as those obtainable in other domains of positive science. It will not consist in establishing a logically necessary connection between the occurrence of headaches and the occurrence of traits specified by physics, chemistry, or physiology; nor will it consist in establishing the synonymity of the term "headache" with expressions defined with the help of the theoretical primitives of these disciplines. It will consist, so the history and the procedures of the sciences seem to indicate, in stating the conditions, specified in terms of these primitives, which as a matter of contingent fact do occur when a determinate psychological phenomenon takes place.

Physics as a Cultural Force

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The subject of *Physics as a Cultural Force* raises the preliminary question of what is meant by general culture. I would like to suggest as a working definition that culture is the opposite of specialization. The aim of the professional is to know as much as he can of his chosen field. Since life is short his field, except in the case of a few rare geniuses, must of necessity be narrow. The aim of culture is to obtain a balanced view of man in the universe. The cultivation of this immense field will of necessity be superficial, except again for the rare genius.

To make culture coextensive with the universe sounds ridiculous, I know, but let me illustrate how inadequate a less broad culture can be. Many years ago I happened to be in a Paris department store with a man older than I. His knowledge of French and English literature and of European history was outstanding. He was an excellent pianist, had traveled widely over Europe, and was very competent in painting, architecture, and the decorative arts. Assuredly he was no specialist, but a very cultured person, in a humanistic sense at any rate. From where we were we could see an escalator carrying shoppers to an upper floor, and as I was then taking engineering courses I remarked that that same morning one of our professors had discussed the kind of electric motor suitable for this particular purpose. "Really," said my friend, "is there an electric motor here?" "Why—yes," I answered hesitatingly, not being sure what he had in mind. "Oh," he continued, "I did not think that was necessary, since the escalator is all of one piece. I thought it was going round and round."

I have this incident always in mind during the course in the physical sciences which I give at Harvard. This is one of many courses given under the heading of General Education. They cover three areas—the humanities, social sciences, and natural sciences—last one year each, and are addressed to the non-concentrator exclusively. Some chemists or musicians, left to themselves, might never open the *Odyssey* or *Vanity Fair*, just as my friend had never heard of the conservation of energy. General Education courses have, therefore, the mission of opening windows which would otherwise have remained shut, of creating friendly interest where there was indifference or even antipathy, and of substituting the desire for a total outlook for the satisfied parochialism of the specialist.

You may think that I am mixing up my proposed theme, "physics as a cultural force," with another, "physics in general education." Actually, I believe that at bottom these two subjects are one and the same. True, there is the technique of physics, the discovery and co-ordination of specific physical laws, Newton's law of gravitation, Maxwell's equations, and the like. And there is also the technique of teaching a General Education course—what to say and what to omit, how to motivate, and how to grade. But we are not interested at this moment in either of these techniques. We want rather to discover what physics can contribute to the culture of the common man and when we have found it, we shall want to give it to our non-concentrators. It is, therefore, with conscious reference to the Harvard courses in General Education that I enter into my subject: What has physics to offer, over and beyond the knowledge of physical laws, that is a contribution to the common culture of mankind?

Faced with this question a physicist will probably think first of the intellectual attraction of physics and of its ideological values. Upon these all scientists are agreed, but it seems to me somewhat circular to point out the beauty of physics to one ignorant of science. I would like to suggest another direction, more pragmatic perhaps, sociological if you prefer, in which a knowledge of physics can be made to contribute to our understanding of the world of today—an approach not unrelated to my experience with my escalator friend.

Life in America in 1949 is permeated with the products of industrial technique and this technique is founded on science. If we are not to live as complete strangers or as infants in the midst of our civilization we surely must understand electric light, generation and transmission of electric power, the steam locomotive, the automobile, the electric refrigerator, the radio and the movies, the telephone, and the airplane. I purposely limit this list to a small number of items of which the importance in our lives cannot be questioned.

Now if we want to bring the nonspecialist to a reasonable understanding of these various industrial developments, we shall have to initiate him into the following chapters of physics and chemistry: mechanics, heat, acoustics, light, electromagnetism, electronics, atomic theory, the periodic table of elements, oxidation-reduction, and the metallurgy of iron, copper, and aluminum. Of course, if our student demands that we explain to him the atom bomb, and he surely will, we shall have to go into isotopes, neutron bombardment, and fission. Thus almost all of physics and of general chemistry, plus the chemistry of the more common elements, would seem necessary in order to give our students an understanding of the most common objects in their daily lives.

Next to physics and chemistry there are two other sciences of which the importance should be duly stressed, even if there is not time enough to study them adequately: the sister sciences, astronomy and geology. Astronomy does not have the practical importance today that it had in the days of navigation by the stars, but its cultural importance is greater than it has ever been. In spite of much complacent thinking there is reason to doubt that the average citizen has a correct idea of the place of the earth in the universe. The tremendous intellectual shock of the Copernican scheme to the men of the seventeenth century helped to make every cultured person aware of this theory, but with its universal acceptance oblivion has to some extent crept over it. Surely it is as important today as it ever was to let everyone know the place of the earth in the solar system.

But two new fields of investigation have been opened in astronomy in the last hundred years which are of no lesser cultural importance: the place of the sun in the Milky Way, and the role of the Milky Way as one of the millions of nebulae which the Palomar telescope is just now attempting to count. Here are additions to our knowledge which are nearly as secure as the Copernican view of the sun and planets; and it is hard to see how certain mental attitudes, perfectly adapted to an epoch or a nation which saw itself in the center of the world, can be maintained by someone who sees himself on a speck of dust circling around a middle-class star in an average nebula. I doubt very much whether this perspective of the space around us is present in the minds of most of our contemporaries.

If astronomy is necessary to give us a sense of space, geology is equally necessary to give us a sense of time. The history of the earth comes quite naturally after a presentation of the dynamics of the solar system and its effect on the unprepared listener should be equally enlightening. It has been remarked that if the history of the earth were to be represented by the height of the Empire State Building, the presence of man on this planet would be represented by a nickel on top of the building, and the duration of time recorded in history by the thickness of a piece of tissue paper on top of the nickel. An account of the successive geological ages, terminating with the recent appearance of man, and summed up in the above picture, should bring home to our students the minuteness of our historical time scale. The perspective of space and time afforded by astronomy and geology is one that a "cultured" person can not possibly be without.

It is impossible to introduce geology without bringing in paleontology; for instance, we need to mention the fossil ferns to explain the origin of coal, and the evolution of animal species is the main reason for our interest in the geological time perspective. This is but one of the many points where we realize the logical impossibility of limiting ourselves strictly to the science of inanimate bodies. The sciences of life will keep intruding into our program from time to time. Our students will have made an important cultural acquisition if they have come to realize that any separation between the physical and the biological sciences is entirely artificial, and justified only by the limitations of time. We shall tell them at the end of the course that should they feel that they have received any benefit from a study of the physical sciences, they will receive an equal or greater one from a course in the biological sciences. In the meantime they will have learned that we make no separation between the various sciences of the physical world, the interpenetration of which they will have had ample opportunity to observe, nor between science and industrial technique—a point obvious enough to the designer of a broadcast transmitter or a nylon plant, yet denied by some who have devoted their lives to purely theoretical thought.

If we manage to take our college freshmen, or the members of an adult night class, through some part at least of this ambitious program, we shall hope for some of the following results: First, they should have a better grasp of the facts underlying the economic, social, and political questions of the day—the techniques of production and distribution, the legislation of transport and power, patent law, and many other such problems of economic and legal import. More important still, through an understanding of industrial techniques they should gain an understanding of the conditions of life of the industrial worker. It is easy to observe, by listening to discussions of labor laws for instance, that some of the debaters have only the vaguest idea of the processes of industry and consequently of the actual life of the men who carry them out. Most important of all, the teaching of some branches at least of physics, chemistry, and technology to the prospective or actual historian, newsman, businessman, or lawyer would break down a barrier of misunderstanding and considerably increase self-confidence.

It is all very well to preach the desirability of broad information and a balanced viewpoint to the specialist of one area of human endeavor. But the case of the engineer ignorant of humanities is not the same as that of the man of letters ignorant of science. The engineer can learn an ancient or modern language, enter history or economics on the ground floor if he earnestly wishes to do so. It is quite another matter to inform oneself about power engineering, radio, or metallurgy. There is so much abstract physics or chemistry, not to mention some necessary algebra, essential to the understanding of these technical processes that the intelligent amateur realizes rapidly that he is unable without help to penetrate below the surface of the subject he would like to study. And ignorance of the basic physical sciences and techniques is apt to make one biased in the appreciation of their social effects.

It is generally agreed that our contemporary problems arise, in great part at least, from the inability of mankind to adjust fast enough to rapidly changing conditions. For instance, from the increase in available mechanical power and the simultaneous growth of industrial efficiency and productivity have resulted: a complete change in the situation of woman in society, since brawn has ceased to be a condition of industrial employment; a growing industrialization of agriculture, since the farmer tends to become as much of a technician as the industrial worker; a reduction in the number of hours of work, which at times develops into technological unemployment; and an increase in the percentage of population going to high school or college. Social strains arising from these new conditions (and from others which will keep appearing as long as science progresses) raise problems in collective behavior which, in a democracy, call for the best powers of reasoning and adjustment of millions of citizens. It is obviously difficult to adjust both laws and attitudes to rapid changes in basic relationships.

The situation would be difficult enough to handle if all the conflicting facts could be impartially examined. But someone completely ignorant of science and of the industrial techniques is apt to develop an antipathy toward more recent ways of living and a yearning for past civilizations which he can understand better than his own. This is not a necessary psychological consequence, as we can all testify from public and private experience, yet when the poet tells us that "Miniver loved the days of old, When swords were bright and steeds were prancing," it is safe to guess that Miniver did not know any science. Some knowledge of "physics," in the most general sense of the word, is practically a prerequisite to meeting our social problems without a negative bias.

This, then, is our answer—one of several possible answers—to the question which was put to us: In what sense is physics a cultural force? Physics, we suggest, is necessary to an understanding of our present world; it can contribute considerably to the integration of our society; it can smooth its passage through the long-range revolution which we are witnessing. This revolution was started by Galileo about 1600, when he attacked the simplest problem of the simplest of the physical sciences. It has brought to us modern physics and modern technology; it has barely begun to attack the much more important problems of life. This revolution, we believe, is fundamentally intellectual; it is economic and social only in its effects. To understand its motive power, the scientific method, to realize the irresistible force of its logic and the compound progression of cumulative knowledge, is to gain an insight into our present age which is of incomparable value to every responsible citizen.

Historians have stressed that the greatness of the age of Pericles, of European civilization in the thirteenth century, or of that of China under the Sungs came from a collective appreciation of a common ideal. These happy periods, and half a dozen others, did not have to face our difficulties. Humanity has now literally gotten hold of a magic wand, of a magic formula. Too few understand how and why it works; many others resent the trick played on them and are irked by a mystery which they cannot comprehend. Courses in the broad field of physics, including technology, addressed to nonscientific adults or to college students, are necessary to throw light on our common road and may significantly contribute to the emergence of a common culture.

It is now easy to see that such courses do not have to be all-inclusive, but on the contrary must go reasonably deep into a few sample subjects. What the nonspecialist needs is a conscientious introduction to the world of physics and technology. He must be helped to enter the realm of heat and the steam engine, or of electromagnetism and the electric motor, or of oxidation reduction and the metallurgy of steel. Once he has been made to feel at home in any one of these or similar subjects, his timidity and his antagonism will have disappeared. He will have learned that there is a modern way of thinking and doing which unites the research physicist or chemist, the research engineer, the production engineer, and the industrial worker. It does not matter whether he has learned it through one of the above fields or through that of aviation or of the dye industry. He is no more a stranger to this modern world in which willy-nilly he has been compelled to live. He is now ready to take his part as a responsible citizen in the shaping of the society of tomorrow, to greet with understanding the social changes which flow from the relentless advance of the sciences and the techniques.

Science as a Social Influence

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Under the title *Science as a Social Influence* I shall explore some of the interrelations between science and society which have come to my observation in recent years. I shall discuss them under the following headings: (1) The benefits which society derives from science; (2) the distinction between fundamental research and applied research; (3) the responsibility of society for the development of science; (4) the responsibility of the scientist to society; and (5) the scientist's obligation to find new sources of raw materials and energy.

The scientist was long happy with the ideas and facts obtained in his isolated laboratory. Now, many feel that science must contribute to society in order to prove its worth. If society is to benefit from advances in science, they say, then practical applications of science must be developed and made available to all. In fact, our civilization is distinctive in its development and use of the practical applications of science. Everyone acknowledges the benefits man has derived from the production of fertilizer by the fixation of the nitrogen in the air, from synthetic fabrics, dyes, and plastics, from electronics, from airplanes, and from atomic energy. If these contributions are not always used beneficially, political man should be blamed rather than science.

The practical applications of science and the contributions of new products, ideas, or machines to society usually involve a continuing and complex development. A product such as nylon, for instance, which was developed under ordinary peacetime conditions, requires many years and a host of scientists, engineers, and administrators to advance it on the long and expensive road over which an idea or a laboratory experiment must travel before large-scale production of tangible value to the public can be obtained. First come experiments on a laboratory scale, and they may cost hundreds or thousands of dollars. Then a process that looks promising in the laboratory must be enlarged ten or twenty fold in the building of a pilot plant, preliminary to full-scale production. The expenditure in running such a pilot plant usually runs into the tens of thousands of dollars. Finally come detailed designs and the construction of a full-scale plant, with costs which may run into hundreds of thousands of dollars, or millions of dollars if the process involves large quantities of material or machinery.

In contrast to these standard, peacetime procedures the wartime development of atomic energy was virtually a miracle of swift progress from pure theory to large-scale production. This miracle was made possible by the war emergency and by an expenditure of two billion dollars; and it emphasized dramatically the importance of science in defensive and offensive warfare. The development of atomic energy cut many corners and involved early work on a scale that was gigantic compared to normal procedures in which the scope of operations is increased by ten-fold steps. Although the expense was great and the risks enormous, the exigencies of war impelled the government to proceed in this fashion.

In carrying out a research program which may be of benefit to society we are faced with short-range and long-range considerations. When we are in a hurry, we seek answers to specific problems in the shortest possible time. This was true of our wartime efforts to which everything else was secondary. As a result, we embarked on what is sometimes called organized research, that is, programmatic or empirical research. The director of a laboratory for such research is informed of the specific needs and of the type of performance expected from a new device or a new chemical process. He then calls in the various heads of his departmental units, and several different programs which may lead to the desired objectives are proposed and discussed. Different individuals or teams of scientists are assigned to specific programs, and a friendly, vigorous competition sets in. As soon as the problem is solved with reasonable satisfaction by one team, the other teams are assigned to new tasks and the successful process undergoes further study and expansion, which may lead eventually to a pilot plant and to large-scale operation. Reports are filed on all alternative processes noted in passing, and later these may be reconsidered and developed further. Under this scheme, the scientists are always working under pressure. If they see something interesting which is not relevant to the main line of attack they have to pass it by, no matter how important it may seem. In this game they are permitted to strive only for practical results and to spend only as much time in developing theories as is immediately necessary to solve the specified problem.

This type of applied, or programmatic, research accomplishes wonders in a short-range program and is widely used in industry. It can be successful only so long as a stock of fundamental scientific research data exists. Before World War II this reservoir of fundamental knowledge was stored up largely in universities, research institutions, and in certain government laboratories throughout the world. Much of it came from the European laboratories which suffered so tragically during the war. During that period we used up the capital of fundamental research at an alarming rate and still are not replacing it at prewar speed.

In the long run, the fundamental research method is the more important, and even for the achievement of practical results it would often be chosen if it were not for the pressure of time. Let me illustrate with a specific point. Suppose there is an urgent need to find a solvent which will dissolve a certain organic material. This solvent must possess certain properties with reference to density, temperature, vapor pressure, and stability toward air and moisture. Also it must be cheap and capable of production in carload lots. If the answer must be found immediately the laboratory is turned loose picking bottles off the shelves to find solvents which will best meet the specifications. If more time is permitted, however, the laboratory will ask its theorists to study the general principles of solubility while it asks its experimentalists to determine solubilities of a large number of different compounds which have nothing to do with the particular problem, but which may be helpful in developing a fundamental theory regarding the mechanism of solubility. Perhaps these theories can be reduced to formulas and graphs so that it will be possible to predict the solubility of many substances at different temperatures and under special conditions. This more fundamental approach, then, can give all that the empirical method of research gives, *and more*. Suppose, for example, that after the process is partially developed, the requirements originally set for this particular project are changed, due to some unforeseen complication. A new start must be made. Under the applied method this means that it is necessary to repeat the mad scramble of "cut-and-try" until a new answer is obtained. However, if fundamental knowledge was acquired in the original research, it is an easy matter to determine the optimum conditions for meeting the new requirements.

The supervision of an applied research laboratory is very different from that of a fundamental research laboratory. It calls for careful control in order to keep all effort steered toward a particular objective. The direction of fundamental research, on the other hand, calls for greater wisdom and breadth of vision and consists chiefly in allowing scientists to follow their own bents wherever they may lead. In the long run this results in an accumulation of fundamental knowledge which is one of the greatest assets of the nation and mankind.

The accumulation of the empirical facts and fundamental knowledge just discussed is expensive. Who is going to support this research and how? In order to realize the advantages resulting from the application of science, the research laboratories and the developmental laboratories must be supported in some manner by society. This support may take the form of taxation, of gifts to universities and institutions, or of investment of private capital in old or new industries.

It is interesting to note the extent to which the country at large and the industrial companies themselves are beginning to realize the debt which they owe to past fundamental research, and the extent to which they must draw on new fundamental research for the future. To illustrate: a large industrial company has just announced a gift of $10,000 a year to the department of chemistry at the University of Wisconsin and has made similar gifts to other universities. The only string tied to this gift is the provision that the money shall not be spent for research which, at the time, has any apparent commercial importance.

The development of an idea through the pilot plant and production plant stages is much more costly than the original scientific research. What incentive can be used to encourage the long and expensive developments which are necessary before society can reap the benefits of scientific discovery? When the government takes charge of such a development, obviously it is the taxpayers that support the project. It is proper and fitting that the federal government should support those projects which involve the national defense or the national health and welfare. Inasmuch as government spending is subject to public criticism and this public criticism is intimately bound up with politics, there is a tendency for the government to be conservative in developing new areas. In the development of new processes by private initiative some incentive must be found to encourage the investment of private capital—often risk capital. One important attempt to meet this need has been our patent system.

Many people have attributed a considerable measure of American industrial prosperity to our patent system. There are however abuses, and the patent system has been under occasional criticism. It seems to me that the concept of the function of patents is not entirely clear. Patents are not needed primarily as incentives to inventors and scientists; these men love their work and their inventive minds cannot easily be stopped. Although some merit may attach to the idea that a patent is the inventor's just reward, very frequently many different people have contributed over a long period of time to the perfection of a patentable idea. It is often difficult to assign complete originality, or full patent rights, to any one person. In my opinion a patent system is needed primarily to encourage the investment of sufficient capital to develop scientific experiments and ideas until they make available a given product to the public. Also, in a system of private enterprise some incentive must be found to attract private capital to these venturesome projects in sufficiently large amounts. There are many failures as well as successes in new processes, and risks must be taken. If private capital is to take these risks it has a right to expect a corresponding return. The monopoly which a patent grants for a limited period of time seems to be a practical answer. We cannot ignore the fact that present-day concern with social responsibility has caused more and more people—investors and taxpayers—to regard a monopoly as a public trust. Corporations which obtain monopolies are increasingly expected to discharge not only their obligations to the investors, by paying substantial dividends, but also their obligations to the public, by providing better and cheaper products.

It is much easier to obtain private support for applied research and development than for fundamental research in which there is no prospect of immediate financial return. Fundamental research in the past has been supported by universities and philanthropists but now, when greater demands are being made for fundamental research than ever before, the sources of philanthropic support are decreasing.

Universities are beginning to realize that they have a moral and social obligation toward the practical results of the discoveries which are made in their laboratories. They do not have, and probably should not have, the business organization to develop and administer them. Many universities are following along the lines pioneered by the Wisconsin Alumni Research Foundation and the University of Wisconsin in returning any financial gains to the University for the support of further research. The Foundation, which handles patents and business arrangements, is completely divorced from the University and gives any profits to the University for the continuance of research in certain areas, without strings and without voice in the administration of the funds.

The argument for a National Science Foundation supported by federal taxes seems sound. Grants in aid for fundamental research throughout the country are greatly needed but every safeguard must be utilized to prevent the stifling of research by bureaucratic administration.

In the past the scientist has been so deeply absorbed in his work that he has taken little heed and no responsibility for the results of his researches. If laboratory research produced a polymer which competed with the silkworm and threw large numbers of workers out of employment, that was no concern of the scientists. But the day of the irresponsibility of the scientist for the results of his work has passed. It passed in a dramatic way with the development of the atomic bomb. The atomic scientists were the first to perceive the danger of this new weapon, with its extraordinary destructive power, its lethal radiation, and its potentially annihilative effect upon urbanized society. As soon as the war was over and their lips were unsealed, the atomic scientists set out to accomplish two things. First, they sought to inform the public about the true nature and significance of the bomb, and second, they undertook to make certain that a permanently responsible organization would be created to deal with the various problems of atomic energy and to safeguard the general welfare and defense. The young atomic scientists took leaves of absence from their work and went to Washington to convince the country and Congress of the importance of placing peacetime development of atomic energy under civilian control. Their influence was decisive in bringing about the passage of the Atomic Energy Act of 1946, which set up a five-man commission to control all aspects of this nation's development of atomic energy.

Secrecy restrictions which prevented free discussion of the problems of atomic energy were very proper and were fully accepted during the war period. Now, in peacetime, certain secrecy restrictions are still necessary, but increasingly it is hoped that public opinion will favor less secrecy and more discussion. The people of the country are being called upon—and will certainly be called upon in the future—to make decisions of the greatest importance regarding the use of atomic energy. In a democracy such as ours how can wise decisions be made by the people at large unless they have available the necessary facts? Although we must realize the necessity of withholding information regarding certain aspects of the atomic energy program, such as construction and use of atomic bombs, one may well ask if there are not areas in which keeping information back from the American public does more harm than would the release of such information to a possible enemy. It might even be that potential research programs in many laboratories of the country would suffer so greatly from these secrecy restrictions as to retard seriously the whole development of atomic energy in this country. If the majority of the people think that this is the best program for a democracy and that safety lies not in trying to conceal information but in keeping far ahead of any competition through intensive research, then the public must not receive too hysterically a few statements which some people will interpret as the release of so-called secrets.

After ages of thoughtless exploitation of our natural resources we are now asking ourselves if we are leaving enough raw materials for our descendants. Some of our minerals and soils are being consumed at a rapid rate. At the same time, in certain areas, the populations are increasing at an alarming rate and the world's appetite for industrial power to run factories and communications and to heat and light homes and cities is growing greatly.

What are the sources of power? Water power and wood to burn are both products of the sun's energy, which raises the water to the clouds and grows the trees. Most of our heat and power comes now from fossil fuels, stored at present in the earth, but made millions of years ago by the action of sunlight on growing plants. Coal which can be shoveled, oil which can be carried in tanks, and gas which can be piped—how long will these last?

At this time we have used up 5 per cent of all our coal, oil, and gas—most of it since 1900. The time left before the exhaustion of these resources depends on the increase or decrease of the present alarming rate of consumption, but it has been estimated that the oil and gas cannot last a century, and that a few thousand years will see the end of all our fossil fuels. What will our descendants do after A.D. 4949?

They may use atomic energy, but according to prewar estimates there is a supply of high-grade uranium and thorium ores sufficient to operate the machines of the United States for only a few years. If all the *low-grade* uranium and thorium ores can be utilized, however, the supply will exceed that of our coal reserves. To be more specific, *theoretically* a ton of average rock contains a supply of uranium and thorium which, if separated and fully utilized, could again *theoretically* provide more heat from fission than is given by the burning of a ton of coal. The concentration and recovery of this low-grade uranium and thorium is not practical now, but it is comforting to know that after we have selfishly appropriated for our own use all of the coal and oil of the world, our descendants by pounding up rocks can still keep warm.

There are many difficulties connected with the development of atomic power from uranium and thorium resources, but atomic energy can and will be an important factor in the power of the future. Technical difficulties are due chiefly to the need for new material and to the intense radioactivity which accompanies the processing.

According to some views, there is no hurry, for the United States has been abundantly blessed with coal and oil. Yes, that is true, but what about India and Brazil, which have large supplies of thorium but very little coal or oil, and which desperately need industrial power to raise the economic standard of their people? Certainly we have an obligation to make atomic energy available to them.

In my opinion the development of atomic power around the world could proceed much more rapidly than the present program allows. Probably the chief reason for delay is the fear that atomic piles around the world can be misused and the fuel converted into atomic bombs. Thus the chance to raise the standard of living around the world will have to wait for a happier international situation.

If man cannot be trusted here on earth with atomic power, then perhaps we should turn to atomic power at a safer distance—to the sun. Let us explore the possibilities of utilizing this enormous source of atomic energy.

On the average, in the United States, one square foot of land receives about one kilocalorie of sunlight energy per minute. Adding this up for an acre it amounts to twenty million kilocalories per day, which is more than a thousand times as much energy as is needed by each man, woman, and child per day in the United States for food and fuel of all kinds including that used for heat, light, transportation, and manufacture. The difficulty is, of course, that the sunlight is not suitable for direct utilization as power because its temperature is too low for mechanical use. Nature, however, utilizes this energy by the process of photosynthesis in which growing plants, through the agencies of chlorophyll and sunlight, convert carbon dioxide and water into carbohydrates and other chemicals. Plants have thus an extraordinarily clever means of storing sunlight in forms which we can use as food and as transportable and storable fuel.

Very little financial support has been given to the study of the vital and fundamental process of photosynthesis. The atomic energy development which was so successful during the war was highly centralized and was carried out largely by means of the applied or programmatic research method of which I have spoken, drawing heavily on the capital of fundamental research which had been accumulated over the years. It would be very interesting now to set up a corresponding program to study solar energy from a fundamental standpoint with decentralized administration of research. Important advances will certainly come in the fields of botany, physics, physical chemistry, biochemistry, engineering, and agriculture. I think we would be surprised to see what rapid progress could be made in our utilization of sunlight for food, house heating, and power if adequate financial support were given and research were encouraged in many different laboratories upon a loose, decentralized organizational basis. There is being accumulated rather rapidly now, along several different lines, a considerable stock of fundamental facts which should lead to a rapid unfolding of our understanding of photosynthesis—the solution of which should enable the human race to continue indefinitely into the future.

In conclusion, let me say that the scientists *can* take care of our descendants provided the statesmen and all of us can outgrow war.

Metaphors of Human Biology

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Analogies are not in good usage among modern scientists. And of all analogies the metaphor is almost the worst, for it smacks of rhetoric rather than of sober and factual description of things.[1] We are all prone to compare human life to a candle that slowly burns down, or our bodies to prisons for our souls. But although we may think that such fanciful images are good enough for casual or poetical expressions, we expect the biologists to keep aloof from metaphorical concepts. Thereby, however, we underrate the power of the metaphor. I believe that metaphors have exercised considerable influence over the biologists' thought. For this thesis I propose to give some examples and then to inquire into the reason for this peculiar habit of mind.

Instead of searching for the oldest metaphor for the human organism, let us discard chronology and turn our

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[1] According to Aristotle (*Poetics*, ch. 21, 1457 b.) analogy is but one among several possibilities of forming metaphors. However, it is the metaphor based on analogy which we have in mind here and which according to Alfred Biese, *Die Philosophie des Metaphorischen* (Hamburg-Leipzig, Voss, 1893) has played a fundamental role in nearly all branches of human life. The use of analogy in science goes, of course, beyond the metaphorical. See Agnes Arber, "Analogy in the History of Science," *Studies and Essays in the History of Science and Learning, Offered in Homage to George Sarton on the Occasion of His Sixtieth Birthday*, ed. M. F. Ashley-Montagu (New York, 1947), 219–233.

attention to the term "organism" itself. We are immediately aware that we are here dealing with a concept which suggests a social counterpart. When we speak of an organism we think of a natural object where all parts function so as to maintain the existence of the whole. Now this biological order also seems applicable to human society, as is expressed in the old parable by which Menenius Agrippa is said to have brought back the revolting plebeians from the Mons Sacer, where they had seceded in 494 B.C.[2] Jealous of the stomach that received all the good things for which they had to work, some other organs of the body decided to go on strike. But as a result, they too starved until they finally recognized that the stomach was as important to them as they to the stomach, and that in order to exist the body needed the proper service of each part. The moral of this story was obvious. The stomach is the patrician caste, the other parts are the plebeians, the body as a whole is the Roman state. Disregarding this parable, it is indeed hard to say which side of the comparison between organism and state was the primary one. Yet, once in existence, the comparison served not only the statesman and political thinker but the biologist as well. Moreover, the differences in social organization were reflected in the different pictures of biological organization. Thus Alcmaeon, one of the old pre-Socratic philosophers, defined health as a balance between the various qualities constituting the body. Significantly enough, the Greek word which he used for balance, "isonomia," also connoted

[2] Livy, II, 32, 9.

equality of political rights. In further accord with the political theory of the Greeks, he described disease as a "monarchy" of any one of the qualities.[3] Some six hundred years later the Greek city-state had lost its freedom. The Roman Empire ruled the world not only by its armies but also by its laws expressed in the maxim: To each his own.[4] This did not imply everyone's having equal claims; rather, it meant that everybody ought to share according to his rank. In the second century Galen, the last of the ancient anatomists and experimental physiologists, used this concept of justice again and again to make the anatomy of the human body understandable.[5] The various parts of the body differ in size: this is only just, because Nature has apportioned their size to the usefulness of their functions.[6] Some parts have few nerves: this too is just, for they do not need much sensitiveness.[7] As we shall see later, the comparison with a social organism was not Galen's main biological metaphor. Nevertheless he found the concept of social justice valuable just as he used the simile of the food supply of a city for explaining the function and name

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[3] See John Burnet, *Early Greek Philosophy* (4th ed., London, 1930), 196, who also gives the Greek text of the passage.

[4] *Corpus iuris civilis* (3 vols., Berlin, Weidmann, 1889), "Institutiones," recognovit P. Krueger, I:1: "Iustitia est constans et perpetua voluntas ius suum cuique tribuens."

[5] For a general statement see Galen, *De usu partium*, V, 9 (ed. G. Helmreich, Leipzig, 1907), 1:277 f; see also *ibid.*, I, 17 (1:36), I, 22 (1:59), II, 16 (1:116).

[6] *Ibid.*, III, 10 (1:171).

[7] *Ibid.*, V, 9.

of the veins of the portal system, which carried chyle to the liver just as many routes carried food to the city's bakeries.[8]

In more recent times the metaphor of the state was utilized in a much stricter sense by Virchow in establishing his cellular pathology. As Hirschfeld has shown,[9] there existed a remarkable parallel between Virchow's biological views and his liberal political opinions. The cell, Virchow maintained, was the fundamental unit of life. All plants and animals were sums of these vital units. It was the relationship between the cells that determined the structure and function of the multicellular organism. "Hence it becomes evident that the composition of a larger body, the so-called individual, always amounts to some kind of social institution."[10] This cell state, moreover, was patterned after a republic. There was no special organ, no single cell representing the individual. Individuality as something simple and integral was altogether a subjective phenomenon of our minds without corresponding biological parallel.[11]

We shall find the biological significance of Virchow's metaphor best if we dwell briefly upon the biological controversy which in the fifties raged between the physicists, systematists, and the followers of the cellular theory. In

[8] *Ibid.*, IV, 2 (1:196).

[9] Ernst Hirschfeld, "Virchow," *Kyklos* (1929), 2:106–116.

[10] Rudolf Virchow, *Die Cellularpathologie* (2nd ed., Berlin, 1859), 12.

[11] *Ibid.*, 260. See also Virchow, "Atome und Individuen" in *Vier Reden über Leben und Kranksein* (Berlin, 1862), 73 f.

1852 Carl Ludwig, one of the leaders of the German materialistic school, published the first volume of his textbook of physiology,[12] which quite seriously attempted to erect a physiological system upon the actions of chemical atoms and physical molecules. Characteristically enough, the book began with a chapter on "the physiology of atoms."[13] Since atoms were the elements of all matter, the first task of the physiologist was to find the significance of various atoms and atom groups for the process of life. What this "process of life" was, and how this concept entered into a system that was supposed to recognize nothing but physical forces, Ludwig did not say. But Ludwig remained consistent in not recognizing cells as vital units. The relative unimportance which the cells played in Ludwig's thought was demonstrated by the fact that their discussion was relegated to the second volume in connection with the physiology of nutrition.[14] Both Virchow and the systematists were united in their attack upon this radical form of materialism. Virchow was rather impatient with the "scientific prudishness" which saw in vital processes nothing but a mechanical result of the inherent molecular forces.[15] Even if it were granted that in a remote past life had originated from these forces, today at least it was a

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[12] I have had at my disposal the second edition: Carl Ludwig, *Lehrbuch der Physiologie des Menschen* (2 vols., Leipzig-Heidelberg, 1858–61).

[13] *Ibid.*, 1:16.

[14] *Ibid.*, 2:229.

[15] Rudolf Virchow, "Zellular-Pathologie," *Virchows Archiv* (1855), 8:23.

demonstrable truth that life did not exist outside of cells, that there was no spontaneous generation of cells, and that vital phenomena rested in the cells. But from here on the paths of Virchow and the systematists parted. In the same year, 1855, in which Virchow published his article on "Cellular Pathology," where his famous formula "omnis cellula a cellula" appeared,[16] Reichert criticized this article in a comprehensive and instructive review.[17] He blamed Virchow for having adopted an atomic view of the organism. Virchow, he claimed, rightly admitted the principle of organization for the cell, only to give it up in the explanation of the animal as a whole, and to construct the latter from cells as if they were atoms composing an inorganic body. In this criticism of the cellular theory Reichert coincided with certain views which Thomas Huxley had expressed in 1853.[18] This article of Huxley's has not received the attention it deserves. We here deal with the young Huxley, the follower of von Baer, who still defended the constancy of species. For Huxley as for the embryologists, Wolff in the eighteenth century and von

[16] *Ibid.*

[17] "Bericht über die Fortschritte in der mikroskopischen Anatomie im Jahre 1854," *Archiv für Anatomie, Physiologie und wissenschaftliche Medicin,* ed. J. Müller, 1855, Appendix. See also E. S. Russell, *Form and Function* (London, 1916), 192.

[18] Thomas Henry Huxley, "The Cell-Theory," reprinted in *The Scientific Memoirs of Thomas Henry Huxley,* ed. M. Foster and Ray Lankester (London, 1898), 1:242–278. In a certain sense this article is a continuation of a paper, "Upon Animal Individuality," which Huxley had delivered on April 30, 1852, and which is abstracted, *ibid.,* 1:146–151.

Baer in the early nineteenth, animal individuality did not so much consist in organization as in the course of development that leads from conception to death. This development "is a continually increasing differentiation of that which was at first homogeneous." The cells "are not instruments, but indications" of this development.[19] In other words, the law of the organic individual determines the differentiation into cells.

But for Virchow, as we have seen, the adult organism was not an individual. How then was he to save its existence as an organized whole? His answer was to take refuge in the concept of the cell state. "There is then no danger that we may lose the unity of the living organism by our multiplicity of vital foci. . . . It is a free state of individuals with equal rights though not with equal endowments, which keeps together because the individuals are dependent upon one another and because there are certain centers of organization without whose integrity the single parts cannot receive their necessary supply of healthful nourishing material."[20] In other words, the metaphor of the cell state for Virchow was not a mere manner of speech, but an integral part of his biological theory. It was a means of preserving the unity of the organism which he, as a physician who had to treat human beings, could not possibly give up. At the same time, the metaphor helped him to avoid admitting an objective form for this unity. A society, or even a state, has no objective form; it has no

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[19] *Ibid.*, 277.

[20] Virchow, *op. cit.* (see note 15), 25.

morphology. The metaphor of the organism as a society is, therefore, just as inadequate as the reverse analogy of society as an organism so popular among sociologists of the nineteenth century.[21]

At this point we have to clarify our use of the term "metaphor." Virchow, apparently, was quite serious in comparing organism and society. At any rate he seems not to have been unduly bothered by the problem of borrowing, for biological purposes, a concept belonging to another field. It is only under our critical analysis that the metaphorical nature becomes clearly visible. All the comparisons which we are discussing derive their strength from the belief that they are true and valid biological notions. To refer to them as metaphors in itself already throws doubt upon their validity.[22] This discrepancy between original belief and present criticism is especially marked in the comparisons of the organism with a work of art and with a machine, which we shall discuss next.

The term "organism" is of relatively modern origin. It is only the term "organ," meaning instrument, that has a venerable past. Plato spoke of the eye as an instrument

[21] For a detailed discussion and criticism see Adolf Meyer, *Wesen und Geschichte der Theorie vom Mikro- und Makroskosmos* (Bern, 1900) and George Perrigo Conger, *Theories of Macrocosms and Microcosms in the History of Philosophy* (New York, 1922).

[22] The interplay between the positive phase of establishing philosophical metaphors and the negative phase of revealing them as mere metaphors has been well described by Biese, *Die Philosophie des Metaphorischen,* 106, 226. Biese on page 159 also alludes to the role which the metaphor of the cell state played among biologists of the nineteenth century.

of the senses.[23] Aristotle and Galen used the term "organ" very frequently. It was compatible with the picture of an instrument that an organ or part of the body could be visualized as operating mechanically.[24] But the Greek biologists of the Aristotelian tradition did not forget that an instrument has an artist who designs it and a master who uses it. The artist to them was divine nature and the master, the soul. Whether in part or as a whole, the human body was conceived as an instrument of the soul, formed so as to suit the requirements of the soul.[25] "For the body," says Galen, "is the instrument of [the soul] and because of this the parts of the animals differ greatly from one another since the souls also do. For some animals are brave, others cowardly, some are wild, others tame, some are social so to speak and industrious, while others are solitary. But in all of them the body is adapted to the fashion and faculties of the soul. Thus the body of the horse is endowed with strong hoofs and a mane for it is a quick and proud animal and not without spirit."[26] It is Galen's ambition to prove for all parts of the human body the perfect adaptation of structure to function, even to social demands and passing fashion. This endeavor at times leads to rather ridiculous statements, as, for instance, when Galen proposes the following explanation for man's beard:

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[23] Plato, *Republic,* 508 b.

[24] See Galen, *De usu partium,* VII, 14.

[25] See Aristotle, *De partibus animalium,* I, 1 (642 a) and I, 5 (645 b, 15 ff.).

[26] Galen, *De usu partium,* I, 2 (ed. Helmreich), 1:1, line 14–p. 2, line 4.

> Indeed, the hair on the lower part of the face not only covers the jaw but also serves as an ornament. For a man, particularly in advanced years, looks more stately if his face is nicely surrounded by hair. And for this reason nature left the prominences of the cheeks and the nose bare and free from hair, since otherwise the whole visage would have become fierce and brutal and not at all suitable for a civilized and social being.[27]

Behind this is Galen's conviction that the divine craftsman, the demiurge, has created man as a sublime work of art.[28] This metaphor of the work of art must be taken literally, against the background of Greek civilization. Just as the workman did not count in the highest ranks of ancient society, so the demiurge did not necessarily hold the highest place among the gods.[29] And indeed, his abilities were rather limited. He did not create the world out of nothing; he had to use matter with all its imperfections.[30] Therefore he could not prevent disease from befalling the human body; all he could do was to shape his material so as to foresee all possible dangers and safeguard against them in the best possible way. One might almost say that Galen, the physician, conceived his demiurge in the likeness of

[27] *Ibid.*, XI, 14 (2:154, lines 4–12).

[28] *Ibid.*, IV, 1 (1:195), and V, 4 (1:260).

[29] Thus the demiurge in Plato's *Timaeus* is not the highest god.

[30] This becomes especially clear from Galen's polemic against the biblical concept of God, *De usu partium*, XI, 14. An interesting remark on the ancient concept of the creating deity occurs in the latest novel by Thomas Mann, *Doctor Faustus*, translated by H. T. Lowe-Porter (New York, 1948), 15 f.

a divine physician given the task of framing man's body in a manner best calculated to secure his life and that of his race and to equip him for life's duties and pleasures.[31]

The metaphor of the divine work of art does not disappear when the organism begins to be likened to a machine. In the *Discours de la méthode,* Descartes alludes to a structure conceivable for the human body that would make it respond to external impressions without the influence of the will. This, he adds, would not be surprising if we think of "the divers automata, or moving machines," which the industry of man can construct from relatively few pieces, compared with the manifold parts composing the animal body. We shall then consider the body "as a machine which having been made by the hands of God is disposed incomparably better and has in itself more attainable movements than any of those that can be invented by men."[32] Here, too, the body is perceived as a divine work of art; yet by changing the accent a new concept has emerged. For Galen, the demonstration of divine art is the main aim; everything in the body, animal and human alike, is understandable only from the point of view of its instrumentality to the soul. For Descartes, the body functions according to mechanical laws. It does not need a soul; indeed animals are not supposed to have a soul at all. The

[31] Nature's purpose in building the body is directed toward the preservation of the individual's life and of that of his species and towards making life pleasant. See Galen, *De usu partium,* VI, 7 (ed. Helmreich), 1:318.

[32] "Discours de la méthode," part 5, *Oeuvres de Descartes,* edited by Charles Adam and Paul Tannery (Paris, 1902), 6:55 f.

idea of divine workmanship in analogy to human automata merely serves to explain the possibility of such a marvelous construction. From a biological point of view, Descartes' identification of the body with a machine is a metaphor by which he tries to give a concept of an organized natural object regulated merely by matter and motion. The metaphor has its obvious weakness. A machine is built for a purpose. Thus the clocks, mills,[33] and automata which Descartes mentions all serve human purposes. But what is the purpose of a soulless animal? This question is unanswerable to Descartes, since he rejects the quest for final causes from his philosophy. However, the metaphor assumes significance immediately if applied to the concept of a man who has a rational soul totally distinct from his body. For the purpose of man's conscious and purposeful life, the body can indeed be considered as a machine that will run according to the manipulations of the machinist. And it will run all the better if it has no purpose of its own, if it is stripped of teleological assumptions and of the vegetative and animal soul with which the ancients had endowed it. There must be no other forces at work than those which can be measured and calculated and there must be no other will than that imposed by man's rational will.

In two essays written some seventy years ago Thomas Huxley praised Descartes as having grasped the spirit of the most advanced physiology.[34] Indeed, Descartes' meta-

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[33] "Traité de l'homme," *ibid.* (Paris, 1909), 11:120.

[34] "On Descartes' 'Discourse Touching the Method of Using One's

phor of the body machine proved most fruitful in many respects. In the first place, it made room for a more active attitude toward the body. Galen had imagined the human organism to be so perfectly constructed that an improvement was not even thinkable. Besides, nature was constantly at work to protect and cure. If, for instance, a wound had been inflicted, it was her first intention to glue the severed edges together.[35] Incidentally, we still bear witness to this ancient principle when, in surgery, we speak of healing by the first intention. But a machine has only a certain number of regulations, which in many cases may prove insufficient to restore the damage. One of the consequences of the Cartesian concept, as Neuburger has shown,[36] was a difference in the evaluation of the healing power of nature and of medical interference. The Galenists upheld the healing power of nature whereas many Cartesians tended to stress its limitations. Boyle, for instance, who followed Descartes in the metaphor of the human machine, argued elaborately that many natural reactions in disease were not beneficial but harmful and that the physician, therefore, had to combat rather than encourage them—this, in spite of Boyle's belief that the

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Reason Rightly and of Seeking Scientific Truth'" and "On the Hypothesis that Animals Are Automata, and Its History" in: Thomas H. Huxley, *Method and Results* (New York, 1898). See especially page 184.

[35] Galen, "Ars medica," ch. 29, *Opera omnia* (ed. Kühn, Leipzig, 1820), 1:385.

[36] Max Neuburger, *Die Lehre von der Heilkraft der Natur im Wandel der Zeiten* (Stuttgart, 1926), 49 ff.

human body had been fashioned by God with infinite wisdom.[37] Once this belief weakened it could be asked whether the body was a good or a bad machine. Thus Helmholtz, in considering the eye as an optical instrument, found it so full of defects that he for one would have felt justified in returning it to the optician who had dared to sell it to him.[38] And perhaps it is not by chance that the period of the nineteenth century which made the most fruitful applications of the metaphor of the body machine also became interested in "dysteleology." By this theory Haeckel designated organs which were useless, and dysteleology found its practical culmination in the removal of the healthy appendix as an altogether useless and dangerous part.[39] In the days of Galen this would have been rank heresy.[40]

The most fruitful application of the metaphor of the machine came with the development of thermodynamics and the discovery of the law of the conservation of energy. So fruitful indeed has this metaphor proved that it is still widely used. The first as well as the second law of thermodynamics can be expressed in terms of an engine. The

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[37] See *ibid.*, 51 ff.

[38] H. Helmholtz, *Popular Lectures on Scientific Subjects*, translated by E. Atkinson (New York, 1883), 219. It has, however, to be added that this criticism of Helmholtz is directed only against the optical aspect of the eye, not against its total physiological efficiency.

[39] Ernst Haeckel, *Die Welträtsel* (Leipzig, n.d.), 162 f.

[40] Asclepiades was the kind of heretic who did not subscribe to the maxim that nature does nothing in vain.

first law formulates the impossibility of building a *perpetuum mobile.*[41] The second law rests upon the experience that "it is impossible to construct a periodically functioning engine which effects nothing but the lifting of a weight and the cooling of a reservoir of water."[42] Considered as an engine, the body must be subject to both these laws. Conversely, it would seem that the validity of these laws would prove the body to be an engine. But an engine is not merely defined by thermodynamic laws and, therefore, the applicability of these laws does not yet make the body an engine. The "living" machine, be it a cell[43] or the organism as a whole, exhibits features which make it unique if compared with lifeless machines. It is one thing to speak of mechanisms by which certain functions are made possible, and quite another to speak of the entire living unit as a machine. In a booklet which Carl Oppenheimer published in 1921, he denied that man was *one* machine; rather, he believed that he represented a large system of many small engines. And this system he compared to an entire factory.[44] True, Oppenheimer made this comparison only in passing, but it is nevertheless interesting to dwell upon it for a moment. Here we deal with another metaphor, for a factory is an economic unit of

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[41] See Max Planck, *Vorlesungen über Thermodynamik* (7th ed., Berlin-Leipzig, 1922), 38.

[42] *Ibid.*, 87.

[43] See D. R. Goddard in: Rudolf Höber, *Physical Chemistry of Cells and Tissues* (Philadelphia, 1945), 373.

[44] Carl Oppenheimer, *Der Mensch als Kraftmaschine* (Leipzig, 1921), 53, 59.

which the actual machinery is but a part; organization and productivity enter as equally important factors and productivity implies a definite purpose. The metaphor of a factory is related to the term "animal economy" which formerly, more than now, was used for physiology and on which Dr. Ackerknecht has made some pertinent remarks.[45]

The economic metaphors suffer from the same inadequacy as Virchow's sociological metaphor of the cell state; they do not account for the biological phenomenon of form. On the other hand they avoid the great weakness of the machine metaphor which presumes somebody outside the machine for whom it exists and who will make use of it. On the presupposition of Descartes, as we have seen, this somebody outside was the rational soul. It is indeed surprising to see how deep-rooted and widespread this Cartesian dualism still is. But it is also noteworthy that it has become an insufficient basis for modern psychiatry. All psychoanalytical and psychosomatic theories which assume bodily reactions to unconscious psychic processes endow the body with qualities which do not belong to the idea of a machine. As I have pointed out elsewhere, in some respects they show closer affinity to the old Platonic and Galenic idea of the tripartite soul than to the organistic psychiatry of the later nineteenth century.[46]

But I think the time has now come when we should take stock of what we have found so far. We have dis-

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[45] Erwin H. Ackerknecht, "Metabolism and Respiration from Erasistratus to Lavoisier," *Ciba Symposia* (1944), 6:1815.

[46] Owsei Temkin in *Bull. Hist. Med.* (1944), 16:519.

cussed a few metaphors by which the human organism has been compared to society, a work of art, a machine or engine, and an economic unit. All these comparisons were taken from realms outside human biology and I have, therefore, felt justified in calling them metaphors. Short as the analysis has had to be, it has brought out features common to all these metaphors. They were not always mere figures of speech, but integrating concepts used by biologists in guiding their thought. However, we have had to take notice of the fact that they were all open to criticism; being metaphors they were not adequate to their subject. Why then did biologists resort to such metaphors? Why the need to use an image outside one's professed domain?

In the first place it is relatively easy to show that each of the metaphors discussed corresponded at one period or another to a certain general view of events transcending the realm of biology. Neither for Galen nor for the mechanists of the seventeenth century did the animal body stand alone as a divine work of art. A contemptible part like the foot moved Galen to a comparison with the sun and to an enraptured hymn upon the divinity of nature.[47] And to Boyle, the machine of the body was but a part of the great machine of the universe.[48] Helmholtz measured the heat produced by muscular movements as a step

[47] Galen, *De usu partium*, III, 10 (ed. Helmreich), 176.

[48] " . . . that great machine the world . . . that smaller engine the human body." Quoted from Neuburger, *Die Lehre von der Heilkraft*, 52.

toward proving the conservation of energy as a general law of physics. And Virchow viewed the body in the light of his concept of a just human society. Perhaps it is not saying too much of the metaphors of human biology which we have discussed to call them only variations of that greatest of all biological metaphors whereby the organism is called a microcosm, a little world, as compared with the macrocosm, the large world.

The theme, microcosm-macrocosm, is indeed very old and its variations are more manifold than can be mentioned here.[49] In the classical and explicit form which it reached in the late Middle Ages and Renaissance it consisted of a point by point concordance between the world and animal life. If we take Paracelsus as our guide we see that it led him, in the first place, to attribute birth, age, and death to the world: "Now the sky too was a child, it too had its beginning and is predestined to its end, like man, and death is in it and around it."[50] Secondly, the sky had its organs which corresponded to those of man. And finally: "Big man also sickens just as the little one."[51] Thus the picture is completed on its macrocosmic side. On the microcosmic side, this correspondence leads to a medical theory of the proper understanding of diseases, and especially of their courses. The physician cannot study directly

[49] See the books by Meyer and Conger quoted in note 21.

[50] *Four Treatises of Theophrastus von Hohenheim Called Paracelsus*, ed. Henry E. Sigerist (Baltimore, 1941), 20.

[51] Theophrast von Hohenheim, *Medizinische, naturwissenschaftliche und philosophische Schriften* (ed. Karl Sudhoff, Munich, 1924), 8:168.

the genesis and development of diseases inside the body. Paracelsus did not believe in the study of anatomy based on the dissection of dead bodies. Instead he believed in a cosmic anatomy which would teach the physician the correlation between astronomical bodies and human organs and make him recognize human diseases from telluric events: "He that knoweth the origin of thunder, winds and storms, knoweth where colic and torsions come from . . . he that knoweth what the planets' rust is and what their fire, salt and mercury, also knoweth how ulcers grow and where they come from as well as scabies, leprosy and serei."[52]

This correspondence between macrocosm and microcosm is based on a belief that macrocosm and microcosm are related like father and son. After God had created heaven and earth and all creatures he formed a mass which contained an extract from everything created. This mass was what the Bible called the "limus terrae," "the dust of the earth" of which God formed man in His likeness. "Out of this 'limus' the creator of the world made the little world, the microcosm, that is man. Thus man is the little world that is, man has all properties of the world within himself. . . . Thus the big world is a father of the little world. For this reason there exists in the little world the kind of the dragons, the kind of snakes, the generation of vipers and adders, also the nature of wolves, sheep,

[52] *Ibid.*, 176. According to M. Höfler, *Deutsches Krankheitsnamen Buch* (Munich, 1899), 652, "serei" in Paracelsus means a skin affliction.

etc., also of all elements, likewise health and disease. For each child takes after the kind of his father."[53] This concept even allows a literal explanation of the beast in man. If some people are vipers or wolves it is because the essence of these animals entered into the composition of Adam.

There is still another variant of the macrocosm-microcosm idea which connects it more closely with the metaphor of the organism as a state from which our discussion started. Not only the animal but the state has been for ages likened to the universe.[54] In particular it was the sun whose commanding position among the stars was identified with that of the king.[55]

> And therefore is the glorious planet Sol
> In noble eminence enthron'd and spher'd
> Amidst the other; whose med'cinable eye
> Corrects the ill aspects of planets evil,
> And posts, like the commandment of a king,
> Sans check, to good and bad . . .

These lines, from Shakespeare's *Troilus and Cressida* (Act I, scene 3), may stand for many which could be adduced from ancient to modern times. But just as the sun could be connected with the king so it could also be connected

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[53] Theophrast von Hohenheim, *Schriften* (ed. Sudhoff, Munich-Berlin, 1929), 12:37. See also Meyer, *Wesen und Geschichte,* 57 ff.

[54] See Conger, *Macrocosms and Microcosms*, passim.

[55] See E. M. W. Tillyard, *The Elizabethan World Picture* (New York, 1944), 83.

with the heart.[56] And all three pieces were most intricately and significantly combined in one of the greatest works of human physiology, Harvey's *De motu cordis,* which in 1628 announced the discovery of the circulation of the blood. The dedication of this book to Charles the First begins:

> Most Gratious King, The Heart of creatures is the foundation of life, the Prince of all, the Sun of their Microcosm, on which all vegetation does depend, from whence all vigor and strength does flow. Likewise the King is the foundation of his Kingdoms, and the Sun of his Microcosm, the Heart of his Commonwealth, from whence all power and mercy proceeds.[57]

One might be inclined to dismiss these and the following words as the euphuistic style of the body physician Harvey, were it not for the fact that the heart appears again as the sun of the microcosm in the decisive eighth chapter of the book which introduces the idea of the systemic circulation. "So the heart is the beginning of life, the Sun of the Microcosm, as proportionably the Sun deserves to be call'd the heart of the world, by whose vertue and pulsation, the blood is mov'd, perfected, made vegetable, and is defended from corruption and mattering." And again, in chapter seventeen we read: "The heart is as it were a Prince in the Commonwealth, in whose person is the first

[56] See *ibid.*

[57] *The Anatomical Exercises of Dr. William Harvey De Motu Cordis 1628: De Circulatione Sanguinis 1653,* the first English text of 1653 now newly edited by Geoffrey Keynes (London, 1928), vii.

and highest government every where; from which as from the original and foundation, all power in the animal is deriv'd, and doth depend."[58]

In both of these passages, the comparisons have a definite biological meaning. In the latter Harvey compares the heart with the ruler of the state, because the heart originates first, because it does not depend on other organs, notably brain and liver, but has the organs of its movements in itself—"as if it were some internal animal"—so that all the rest of the body depends upon it. Harvey here uses the Aristotelian metaphor of the heart as the "acropolis" and "the supreme power."[59]

But the comparison of the heart with the sun is of even more startling character, for it elucidates Harvey's very concept of circulation. The sun makes vapors rise from the earth; these vapors in turn become condensed and change into rain which moistens the earth. Hence things are generated and storms and meteors arise "from the circular motion of the sun, by coming and going." Likewise in man, the blood reaches all parts of the body while warm and nutrient, but is itself cooled and worn out so that it returns to the heart, "the fountain so to speak or the domestic deity of the body," in order to be perfected. From here it is distributed again "and all this depends upon the motion and pulse of the heart. Therefore the

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[58] *Ibid.*, 59 f., 115.

[59] Aristotle, *De partibus animalium*, 670 a, 23–26, and *Parva naturalia*, 469 a, 5. See also John G. Curtis, *Harvey's Views on the Use of the Circulation of the Blood* (New York, 1915), 44 f.

heart is the principle and sun of the microcosm."[60] Mark the double analogy in this comparison. The circulation of the blood is likened to the circulation of moisture. The latter depends upon the circuit of the sun just as the former depends upon systole and diastole of the heart.

The implications of this analogy for Harvey's fundamental beliefs as well as for some of his mystically inclined followers have been pointed out by Curtis[61] and, above all, by Pagel.[62] Here too we see the Aristotelian Harvey who still clings to a geocentric cosmology with its old ideas about the nature of the heavenly bodies. What a contrast this concept is to the mere mechanical description of the circulation of the blood for which alone we are inclined to praise Harvey! And, on the other hand, this metaphor of the sun of the microcosm allows Harvey, in a few paragraphs, to indicate his notions of the physiological significance of the blood for the life of the whole organism.[63] In a book full of metaphors, these particular ones of sun, king, and heart serve the purpose of sketching a whole system of physiology which, it may be added, is not modern at all. But this does not give us the right to dismiss

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[60] William Harvey: *Exercitatio anatomica de motu cordis et sanguinis in animalibus* (Frankfurt, 1628), ch. 8, p. 42.

[61] Curtis, *op. cit.*, p. 154 ff.

[62] Walter Pagel, "William Harvey: Some Neglected Aspects of Medical History," *Journal of the Warburg and Courtauld Institutes* (1944), 7:146; "The Vindication of 'Rubbish,'" *Middlesex Hospital Journal* (Autumn, 1945); "A Background Study to Harvey," *Medical Bookman and Historian* (1948), 2:407–409.

[63] See especially the last-named paper by Pagel.

them as an antiquated byplay in an otherwise admirably modern piece of research.

Our contention that all of the metaphors mentioned are only variants of the theme of macrocosm-microcosm needs qualification. The macrocosm is not necessarily the universe of heaven and earth as it was for Paracelsus, Harvey, and Boyle; it may simply be some wider realm which seems to give order and meaning. Virchow's interest, for instance, lay in the concept of a just society. With this qualification it may be said that the metaphors we have discussed, in the beginning at least, helped to place the organism in some scheme and thereby give meaning to its existence. They served to co-ordinate the interests of our human life and the science of life. When the picture of the world and the spheres of interest changed, the metaphors of human biology tended to change, too.

We should, of course, not forget that most of the metaphors mentioned could be used the other way around. That is to say, the organism could be used as a primary concept and the world, society, or economic units could then be explained by conceiving them as organisms. This indeed has been done and is still being done with the same result of co-ordinating life and a wider order of things.[64] Moreover, it is possible that upon closer scrutiny, human biology would not appear as the only science in which metaphors have been used in order to give unity to our concept of the world. The metaphors in science may appear to be a kind of indicator for our prevailing convic-

[64] See the works of Biese, Meyer, and Conger.

tions.[65] But aside from this possibility it seems to me that in human biology special reasons prevail. The phenomenon of the living organism is hard to define; our modern textbooks usually shy away from the task. Moreover, in dealing with human biology the difficulty is substantially increased. We speak of human biology as if it were a well-defined province of science. But are we really agreed upon the nature and scope of human biology? From biology we inherit all the problems of the living organism. To this is superadded the question how far the field of biology reaches in human affairs. There are some who would like to give a biological explanation even to history and ethics. Others there are who would exclude from the field of biology anything not purely somatic. Either extreme has to justify its position. This justification is not the mere scientific inference from facts but is usually a decision

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[65] Biese, in *Die Philosophie des Metaphorischen,* has stressed the integrating function of the metaphor in general and in philosophy in particular. Meyer, in *Wesen und Geschichte,* 109 ff., speaking of the theory of microcosm-macrocosm, admits the usefulness of metaphorical analogies but criticizes the theory for having assumed real similarities at the expense of scientific exactness. Meyer's criticism is certainly valid and in the past similar criticism has served to unveil metaphors. For instance, van Helmont said of Paracelsus: "To wit, he translated the Metaphor of a Microcosme into the truth itself." *Oriatrike* (London, 1662), 237. I am indebted to Dr. Lloyd G. Stevenson for this reference. However, a metaphor consciously and consistently used as a mere figure of speech is not likely to exert great influence in science because of the probable lack of confidence in its validity. It is the transcendence of legitimate metaphorical speech into real identification which has proved stimulating as well as dangerous.

made a priori and for reasons not inherent in biology itself. The realm and nature of human biology, it seems to me, is defined by convictions entering from outside. Therefore, the language too comes from outside; it is metaphorical. The use of metaphors in human biology is not an aberration from which even great men have failed to escape. On the contrary, by using metaphors which they believed to represent adequate and true concepts, Aristotle, Galen, Paracelsus, Harvey, Descartes, Virchow, and Helmholtz shaped concepts of human biology which conformed with their own thoughts and feelings and with the thoughts and feelings of their times.

Science and Society

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8

A science of society is much desired by all of us who would like to see science make achievements in the social field similar to those in the natural sciences. Thus we would like to see science abolish crime, as it has eradicated typhoid fever, or build an efficient democracy without corruption, as it has built fire-proof buildings. These desires though natural enough are from the practical viewpoint like the fantasies of our daydreams which come so easily. It is our purpose to approach the problem more realistically.

First, we may observe that a science grows slowly, and a mature science of society is not to be had quickly. That a science grows slowly is obvious when we consider the definition of a science as a body of knowledge. I rather like the term a pile of knowledge, since an important characteristic of science is that it accumulates. Of course, a pile of knowledge could conceivably accumulate quickly but history has shown that it accumulates slowly, particularly in its early growth, and it takes time to accumulate a big pile. Therefore I think we shall not get a highly developed science of society soon. It may be argued that we have had society with us a long time. Yes, but we have not tried for a long time to accumulate knowledge about society by scientific methods. So the science of society is still very young.

I suggest then that we do not take up for discussion the usual questions, Is a science of society possible? or Have we a science of society? Instead, let us ask: Have we a pile of accumulated knowledge about society? If so, how big a pile is it?

The answer to the question whether we have a body of knowledge about society is in the affirmative. We have, for instance, a considerable knowledge about income, production, migration, the family, and education. And the accumulation of such knowledge is becoming greater.

But it may be argued that a science is more than census taking, although it might also be argued quite successfully that the basis of any science is obtaining data. However, the definition of science as knowledge has been extended to read: Science is organized knowledge. Now a body of knowledge is likely to have some organization, since there are similarities and differences. Knowledge is not often piled together in a miscellaneous manner. But we usually mean more than classification by the word organization: we mean relationships. It is these relationships that help us to determine cause and effect, that enable us to predict.

We do have organized knowledge in the social field, as is indicated by the classifications of the special social sciences such as economics and sociology. More than that, we have knowledge of relationships between phenomena. We know, therefore, for many different commodities the effect on price variations in supply.

This knowledge of relationships tends to be limited in time and space. Thus when we say that the correlation between the birth rate and economic conditions is +.5, the observation is limited to countries with cities in modern times. We do not know that it is true of all times and in all regions.

It is interesting to consider, therefore, the extent to which these relationships can be extended, that is, generalized into propositions that are universally applicable.

There are undoubtedly some propositions that are of wider applicability than others. Thus, that marriages fluctuate with economic conditions is true more generally than that births vary with good times and bad times.

To other propositions that seem to be very generally true, such as the law of supply and demand, exceptions may be found. Where these general propositions have been submitted to measurement, the correlation is never +1. Hence there are exceptions. To other general propositions which have not been extensively measured there appear to be exceptions. Psychiatrists say that alcoholics drink to escape conflicts. But if there were a perfect correlation between alcoholism and mental conflict, then those who have mental conflicts would be alcoholics as truly as alcoholics have mental conflicts. But there are those who suffer from conflicts who do not drink. Sometimes the general propositions are so vague and indefinite as to be meaningless. Thus psychoanalysts state that an oldest son is jealous of the younger brother or sister. But if all persons have some jealousy toward those with whom they must associate and share, the proposition probably means that oldest sons are more often jealous of younger sons than younger sons are jealous of older sons. For this proposition measurement is needed, since there are probably many exceptions.

The reason that there are few if any general propositions without exceptions or no coefficients of correlation of $+1$ or -1 between social phenomena is that there are many variables. Thus when we observe that not all elders are conservative, we mean that there are factors other than age affecting conservatism. It is commonly admitted that in the social field there are many more variables affecting a social phenomenon than affect a physical or chemical phenomenon in the inorganic field. In the biological field there are many variables but it appears that they are more readily held constant in assessing any one relationship than is the case in the social field. So it may be that the physical, the biological, and the social sciences do differ and will differ in their success in establishing general propositions.

But I would say that a highly developed science can exist and be very useful indeed without general propositions to which there are no exceptions. The proposition that business depressions are followed by unemployment is a useful piece of knowledge, even if there are exceptions. If the correlation between business cycles and employment at a certain period and in a particular area is $+.8$ it is still valuable knowledge even if the correlation is not $+1$. And in so far as control is an objective of the use of knowledge one does not particularly care what the knowledge of unemployment and business was in ancient Rome.

Indeed if we want scientific knowledge in the social field for purposes of planning and control, we can make use of propositions that are true only some of the time by putting more effort into finding out the influence of other variables at the particular place and time with which our planning and control are concerned.

Thus if we wished to make plans to control juvenile delinquency we would want to make use of our knowledge that in modern cities in the United States dilapidated and congested housing is related to juvenile delinquency. But the relationship is not always causal: slums have been razed and model housing constructed and yet juvenile delinquency exists. So other factors must be considered, such as parks and playgrounds, supervised play and club work. Also, there is the relation of juvenile delinquency to broken homes and to low income, factors which are not so easily controlled. The problem would be much easier, of course, if the correlation between housing and delinquency were $+1$. Here the absence of a general proposition means that we must work with more propositions of limited applicability.

But instead of deploring the absence of general propositions of universal applicability, as many critics do, we might be grateful for the knowledge of relationships that we do have, even though its applicability is limited. Indeed, the need of knowledge in the social field is for the specific rather than for the general, if problems are to be viewed pragmatically. We want to know how to avoid a war with Russia within the next few years, or how to get rid of the existing political machine in Memphis, Tennessee, or how to improve the efficiency of the Congress of the United States in the next decade. This emphasis upon the specific in view of the scarcity of universal propositions brings us, surprisingly, to a consideration of "society," the second noun in the title of this paper. We have so far been considering science only.

Society is usually viewed as the behavior of groups, the relations of the individual to the group, and of the individuals one to another. The study of these subjects has led to a good deal of general knowledge about conflict, imitation, cooperation, leadership, rewards, and punishments, the learning process, habit, response to stimuli, and mob action. But this knowledge tends to be rather too general to help explain or predict the specific. Thus we know we learn better by repetition and faster if we are eager to learn. But this knowledge does not help us much in explaining why some are criminals and others not, even though criminal behavior is learned. Nor does our general knowledge of cooperation and leadership help us to explain why the League of Nations failed and why the United States did not.

The knowledge we need to explain a specific event is usually a special combination of factors and not some general proposition. World War II is to be explained in terms of the Treaty of Versailles, national pride, the prospects of European control, the grain fields of the Ukraine, the German military tradition, etc., and not by the general knowledge about group behavior known as conflict. This explanation of World War II, and of World War III if it comes, lies in terms of history and tradition; and the cluster of special factors that explain it are to be found in the culture, as the sociologists use the term, or in the social heritage, rather than in any biological propensity to fight. This propensity in its hereditary expression is a constant. The variables are cultural. For culture is changing rapidly in nearly all its parts, which are themselves interconnected, and hence supplies the variables. So then the word society must comprise the phenomenon of culture, which in the special social sciences is so often treated under the heading of institutions—economic, political, educational, etc.

We have now discussed the concepts of both science and society. Our next step is to comment on method.

Primitive peoples had a good store of reliable knowledge, especially about material culture. But their method of accumulating it was largely the slow and wasteful one of trial and error. In the last few centuries we have learned to avoid much of the costliness of trial and error by utilizing the method of science that is the knowledge of relationships. From such past accumulations of knowledge on relationships we now set up a hypothesis of a relationship in which we are interested and then test it with data.

This method is applicable in the social sciences and is in common use, as it is in the natural sciences. In applying the method in social science we use somewhat different devices from those used in natural science.

For instance, the laboratory experiment is not very often feasible in social science. The laboratory implies two techniques. One is the elimination of the influence of factors other than the ones concerned. The other technique is to set up a relatively inexpensive and often small-scale test of the hypothesis. In social science we are seldom able to set up a model in a laboratory. It would be difficult to try out a United Nations in the laboratory. Nor is a city a laboratory, as is sometimes claimed, for in a city we cannot control the variables. But we do in social science have several very good ways of eliminating the influence of extraneous variables. One of the most common is to hold certain variables constant. This can be done, for instance, very beautifully by partial correlation. Partial correlation is to much social science what the laboratory is to natural science.

However, there are many areas of social science where partial correlation is not applicable and also indeed where statistics are relatively unknown, as in much cultural anthropology and in some historical work. But, as there are similarities and identifications, an increasingly large amount of data in the social field is analyzed quantitatively. Numerical expression of some sort is possible; hence quantification expands as time goes on, and the use of statistics grows. Practically, the collection of statistics often calls for extensive administration and becomes so costly that only the richest countries and the stable governments have statistics enough for much scientific study. Very lately new methods have been developed for reducing the burden-some statistical costs. One of these is statistical sampling. As the laboratory permits a miniature model in natural science, so sampling enables the social scientist to work in miniature.

So in much of the social field, I think, we have fundamentally the same scientific method as exists in natural science, but somewhat different techniques. There are however some qualifications of this statement. For instance, in psychiatry and in religion there appear to be some types of investigation where the test is through the feelings rather than through the eyes. In natural science observation is made through the eyes and perhaps occasionally through the ears. Feelings may be less reliable than eyesight.

To appraise science and society further, perhaps, we should next consider some of the obstacles to the development of science in the social field.

The scholarly tradition is an obstacle, in my judgment, to social science as it was to natural science. For the literary scholarly tradition values verification somewhat less than does the tradition of science and gives more acclaim to other intellectual virtues such as analysis, interpretation, argument, discussion, and imagination. The literary scholarly tradition often accords high honors to publications displaying these virtues even when the question "how do you know it?" has not been answered by evidence. Of particularly high prestige, unfortunately, in the social field is synthesis, or the construction of systems of ideas. Thus in economics the person who develops a theory, that is, builds a system of ideas, often acquires more honor than one who verifies a hypothesis. In other words, prospective social scientists do not hold steadfastly enough to the discipline called for in verification. Instead they are drawn off into intellectual display where the rewards are still very great.

In view of the slight development of the scientific tradition in the social field and the high development of literary-scholarly-philosophical-ethical tradition, not all workers in the field have a clear picture of what science is. While many, if questioned, would admit that science is organized knowledge, they appear to accept ideas and theories as knowledge. Also some are so obsessed with the "organization" part of the definition, that is, system-building, that they seem to forget, at least temporarily, the importance of verification.

Others see science not as knowledge but as understanding. Some consider that the purpose of scientific work is to produce not new knowledge but wisdom. Still others think that since knowledge is used for control any effort that leads to effective control is, *ipso facto,* science. But the purpose of scientific research is to produce reliable knowledge, not wisdom, understanding, or control, though knowledge may be used for these ends. There is a difference between knowledge and its use.

Another hindrance to scientific work in the social field is the amount of emotional bias that exists in many areas. The topics dealt with which stimulate great emotion are, for instance, sex, crime, race, religion, freedom, authority, war, social classes, family life, politics, the distribution of wealth, etc. There are, nevertheless, many areas of social inquiry which perhaps stimulate no more emotion than a chemical compound or an insect. Such areas are found, for instance, in anthropology and in history. If data were adequate and the variables few, prejudice would not be notable as a hindrance to social scientists, for then emotion would not distort our picture of reality. But when data are inadequate and the variables too many for our tools, then our results are inconclusive or, at best, approximate. Under such conditions, prejudice and other emotional biases have a distorting effect and increase error, even if they do not lead to a wrong conclusion.

The demands of social action at times appear to be a hindrance to scientific work, though it would seem that they should be a help. For instance, we must often vote or take a position on some issue whether we have adequate knowledge or not. We cannot follow that great adage of the scientist, "suspend judgment," until the evidence is adequate. Urgency in contemporary problems forces us to reach opinions rather than to wait until we acquire knowledge. Add to this situation the scarcity of general propositions and there necessarily appear approximations and fairly good opinions, but certainly exact science does not emerge. But this demand of social action which is met by reasonably good opinion and by approximations may lead increasingly to more knowledge and greater exactness.

Such then is a brief appraisal of science in the study of society. Some comments by way of discussion, however, need to be made.

The high development of a science of society, which I hold to be greatly desired, would nevertheless not solve all our social problems. The existence of a body of knowledge is not a guarantee that it will be used, or that it will be used rightly. For instance an employer may not care to have knowledge about the low standard of living and the hazards of life for his employees nor to use it for the purpose of raising their wages. As an executive he may be much more responsive to the pressures of his stockholders for profits. So also the government of South Africa, when dealing with the large number of Negroes, may not use the knowledge which anthropology furnishes about racial equality. It may be primarily interested in maintaining white dominance. Thus if knowledge is an obstacle to power it may not be used.

Knowledge may also be misused. We have some knowledge about the influence of propaganda and of how to persuade individuals to act. Herr Goebbels, the minister of propaganda under the Nazi regime, possessed such knowledge and great skill in using it. Yet who shall say that he used such knowledge for the good of society? Knowledge is sometimes like an invention—for instance, a knife which may be used for good or for bad.

It is not clear that scientific knowledge will solve the social problems that arise from the abuse of power. Those in power can use scientific knowledge to remain in power as truly as those who suffer from the abuse of power may use scientific knowledge to improve their position and to control power. Even if we should ever acquire the knowledge to prevent power and selfishness, or even to lessen them, and did so, society might be the loser, for the love of power and selfishness are great driving forces.

However, it seems to me that a large accumulation of scientific knowledge about society should help somewhat the resolution of difficulties that arise because of different value systems. In class and race prejudice, knowledge of the distribution of inherited ability lessens the power of the subjective factor in building rationalizations to support prejudice, but not too much should be expected from social science in solving value conflicts. Even though knowledge reduces the freedom to rationalize emotion, it must be observed that the educated seem to rationalize about as easily as the illiterate.

These remarks are made to suggest cautions in our expectations from social science. The achievements of science have been most spectacular in physics, chemistry, astronomy, and biology—so much so that our hopes are that similar scientific marvels may be achieved in all walks of life. To many of us science has been oversold. Men live emotionally rather than scientifically or even rationally much of the time. A large part of our life is taken up in acquiring, playing, worshiping, loving, praising and receiving praise, adventuring, seeking sociability, and trying to forget. Social science knowledge is more likely to affect the materials and milieu of these activities than the nature of them or the time put in on them.

That human nature can be changed we know. That is, our original inherited biological capacities are somewhat unorganized at birth and through the learning process a child can be brought up with any one of a great variety of sets of habits. There are, it seems, children who have been brought up by wolves, and have only a wolf culture, with no human traits such as language and the use of hands at all. Other deviations from a human pattern occur. Men and women go through life as celibates. There are hermits. The ascetics and penitents persecute themselves through life. But such extreme departures from norms of human behavior appear desirable to only a few and are often accompanied by adverse effects.

On the other hand physical fighting, at least with deadly weapons among adults except in organized warfare, has been controlled in large modern societies, as compared with preceding centuries. In early times killings in Iceland were common occurrences, yet in the nineteenth century it is said that only two murders occurred in that once bloody island. There is little evidence of any adverse effects on the citizenry, although psychiatrists do uncover a good deal of latent hatred as a disturbing factor to personality.

The reduction of physical combat with deadly weapons seems not to have been accomplished through the method of rearing children. Rather, the control of this type of conflict has been achieved by social organization. But new social organizations are seldom the product of the application of scientific method in the social field. New social organizations are rather social inventions which are achieved by the old practice of trial and error. Still, there is reason to think that we may sometime make social inventions by the use of scientific knowledge as we now make mechanical inventions in the industrial laboratory.

Discussing this question of changing human nature, by reducing the conflicts or by bringing up children with less pugnacious habits, suggests that a body of knowledge about society is not enough in itself to improve social conditions or to solve social problems. Several other steps are necessary in order to make constructive use of this knowledge.

For instance, in the physical sciences we are accustomed to the dichotomy of pure science and applied science. This dichotomy is seldom found in the social sciences, for the reason that there are so few general propositions or relationships that are specifically applicable to a particular situation without a good deal of fitting to other variables.

But even in physical science the application of pure science to practical problems becomes itself a science. Thus there are the various engineering sciences, such as chemical or electrical engineering. So also in the social field the application of the general knowledge calls for something like social engineering. Even though we know that we can develop an infant's personality in one of many different directions by controlling group influences, there is need for further knowledge about which group, which influence, how much, at what time, and by whom.

But even after such so-called social engineering knowledge exists, to make use of it in society the people or the leaders or both must be persuaded that such social engineering is desirable. And finally after the propaganda or education is successful, some group must be organized or persuaded to carry out the application of the knowledge, be it a school, a corporation, a political party, a labor union, or a million families. The creation of social science and the making use of it is not as simple, as someone has said, as making butter and spreading it. We have spent some time in saying we must not expect too much of social science nor must we expect anything too soon. But these cautioning remarks should in no sense be interpreted as pessimistic.