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THE STUDY OF MAN[1]

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THE subject of this address is neither man nor the propriety or the appropriateness of the study of man; it is that study itself. It is not an examination of what chiefly interested Pope and Bolingbroke; it is a consideration of certain biological and social sciences. It is not even primarily the study of man; it is the study of men as organisms, of their structures and functions, in sickness and in health, and of men as persons, in their activities and their interactions; for the characteristics of man are but the uniformities observable among men. Again, our subject is not the examination of what such studies ought to be; it is merely a fragment of a description and analysis of what they are, of how they have been, and of how they have not been, effectively prosecuted. Let us note at once that effective work involves both doing what is effective and not doing what is not effective.

The study of men—even the scientific study—is ancient and respectable. It goes back to Aristotle, to Hippocrates and beyond them to obscure beginnings. To-day it is one of the chief studies of the learned. Like our other activities, it may be divided into two parts, the successful part and the unsuccessful part. Speaking very generally and with due regard to numerous and important exceptions, it may be said that the successful part of the scientific study of men is related to medicine, the unsuccessful part to philosophy and to the social sciences. These relations are not only historical, they are also to be seen in methods, attitudes and traditions.

The successes of medicine and the medical sciences have not been lightly won; from a multitude of failures

[1] An address delivered at the University of Pennsylvania Bicentennial Conference, September 18, 1940.

they are the survivals, the fortunate productions of the best or the most-favored men among an endless succession of skilful physicians. Though pedantry, incompetency and charlatanry have often hindered and in evil times, even for long periods, arrested the accumulations of medical science, since Hippocrates, at least, the tradition of skilful practice has never been quite lost—the tradition that combines theory and practice. And this tradition is, especially in three elements, indispensable.

Hippocrates[2] teaches first, hard, persistent, intelligent, responsible, unremitting labor in the sick room, not in the library: the all-round adaptation of the doctor to his task, an adaptation that is far from being merely intellectual. This is adaptation chiefly through the establishment of conditioned reflexes. Something like it seems to be a necessary part of the mastery of any material or of effective work in any medium, for such adaptation is the mark of every master-workman in every field. Galileo refers to it among artisans, saying:[3] "Indeed, I myself, being curious by nature, frequently visit [the arsenal of Venice] for the mere pleasure of observing the work of those whom, on account of their superiority over other artisans, we call 'first rank men.' Conference with them has often helped me in the investigation of certain effects including not only those which are striking, but also those which are recondite and almost incredible." A similar adaptation is not less evident in the most abstract of the sciences—in mathematics. What, indeed, can be done in mathematics by one who lacks complete intuitive familiarity with the symbols and operations of the science, by one who must constantly think of and be aware of what he is doing and how he is doing it?

Hippocrates teaches, secondly, accurate observation of things and events, selection, guided by judgment born of familiarity and experience, of the salient and the recurrent phenomena, and their classification and methodical exploitation. This is descriptive science. It is not necessary for the craftsman, it is for the scientist. The more complex the things studied by a science, the greater—in general—the importance of descriptive knowledge. For example, taxonomy is more important to zoology than description to mechanics. In the scientific study of men much systematic descriptive knowledge is almost everywhere indispensable.

Hippocrates teaches, thirdly, the judicious construction of a theory—not a philosophical theory, nor a grand effort of the imagination, nor a quasi-religious dogma, but a modest pedestrian affair, or perhaps I had better say, a useful walking stick to help on the way—and the use thereof. Theoretical science is not necessary for the craftsman, or, perhaps, for the descriptive scientist, because both may think in terms of the world of common sense. But theory in the form of some kind of an abstract conceptual scheme seems to be necessary for the effective exploitation of even descriptive science.

All this may be summed up in a word: The physician must have, first, intimate, habitual, intuitive familiarity with things; secondly, systematic knowledge of things; and thirdly, an effective way of thinking about things.

Experience shows that this is the way to success. It has long been followed in studying sickness, but hardly at all in studying the other experiences of daily life. Let us, therefore, consider more carefully what Hippocrates did and what he did not do. He was in reaction chiefly against three things: first, against the ancient, traditional myths and superstitions which still prevailed among the physicians of his day; secondly, against the recent intrusion of philosophy into medical doctrine; thirdly, against the extravagant system of diagnosis of the Cnidian School, a body of contemporary physicians who seem to have suffered from a familiar form of professional pedantry. Here Hippocrates was opposing a pretentious systematization of knowledge that lacked solid objective foundation; the concealment of ignorance, probably more or less unconsciously, with a show of knowledge. Note well that such concealment is rarely altogether dishonest and that it may be practiced in thorough good faith.

The social sciences to-day suffer from defects that are not unlike the defects of medicine to which Hippocrates was opposed. First, social and political myths are everywhere current, and if they involve forms of superstition that are less apparent to us than the medical superstitions of long ago, that may well be because we recognize the latter class of superstitions for what they are while still accepting or half-accepting the former class. Secondly, there is at least as much philosophy mingled with our current social science as there was at any time in the medical doctrines of the Greeks. Thirdly, a great part of the social science of to-day consists of elaborate speculation on a very insufficient foundation of fact.

Hippocrates endeavored to avoid myths and traditional rules, the grand search for philosophical truth, the authority of philosophical beliefs, the concealment of ignorance with a show of systematic knowledge. He was concerned first of all not to conceal his own ignorance from himself. When he thought abstractly, or in general terms, his thought was limited and con-

[2] In speaking of Hippocrates, I mean the author or authors of the so-called genuine works of Hippocrates, and wish to express no opinion about the man of that name, whose life is little known. We need here feel no concern for the question whether this man wrote these works.

[3] "Dialogues Concerning Two New Sciences," Macmillan, New York, 1914, p. 1.

strained because he had wide intuitive knowledge based on the habit of responsible action in concrete situations. There is a test for this kind of thinking: the question, "For example?" Those who generalize from experience almost always pass this test; others do not. Indeed, the test is frequently destructive of unfounded generalization and is apt to lead to painful embarrassment. For this reason its use is often inexpedient.

Experience shows that there are two kinds of human behavior which it is ordinarily convenient and often essential to distinguish:

The one is thinking, talking and writing, by those who are so familiar with relevant concrete experiences that they can not ordinarily forget the facts, about two kinds of subjects. These are: first, concrete observations, and observations and experiences which are representable by means of sharply defined or otherwise unambiguous words; and secondly, more general considerations, clearly and logically related to such concrete observations and experiences.

The other kind of behavior is thinking, talking and writing about vague or general ideas or "concepts" which do not clearly relate to concrete observations and experiences and which are not designated by sharply defined words. On the whole, the works of Plato belong to this second class, the Hippocratic writings to the first class.

The so-called genuine works of Hippocrates reveal a method in the exploitation of everyday experiences with the lives and deaths of men that can never be too carefully studied. In the beginning are the cases, the clinical records of the great physician. They consist of bare observations of bare facts, uncolored by theory or presupposition and condensed to the very limit of possible condensation. These are the practicing physician's data, freed so far as possible from everything that is not a datum. The data are of two kinds: the first kind, often contained in the first part of the record, are single observations; the second kind, commonly presented at the end, are observations of uniformities throughout a particular sickness of a particular person.

The next step, after the recognition of uniformities in a particular case, is the recognition of a wider kind of uniformity: the recurrence again and again in different cases, often otherwise very various, of single events or of the uniformity observed within a single case, for example: regularities in the duration of certain fevers, the frequent discharge of fluid through the nose in what we now call diphtheria, and in general the prognostic importance of a wide range of symptoms. The most famous of all the descriptions of such uniformities is that known as the "facies Hippocratica," the appearance of the face at the point of death in many acute diseases: "Nose sharp, eyes hollow, temples sunken, ears cold and contracted with their lobes turned outwards, the skin about the face hard and tense and parched, the colour of the face as a whole being yellow or black."

Throughout a great part of his work Hippocrates is thus moving step by step toward the widest generalizations within his reach. In great part he is seeking a natural history of acute disease, or at least of those acute diseases that were prevalent among his patients. His success was great, and the whole history of science goes far to support the view that such a methodical procedure is a necessary step in the development of a science that deals with similarly complex and various phenomena.

Beyond this stage there is one even wider generalization that plays an important part in the writings and thought of Hippocrates. This is the principle that came to be known, and is still remembered, as the *vis medicatrix naturae*. It may be stated in modern form as follows: Organisms exist in a state such that when a modification, not too great and different from what will otherwise occur, is impressed on them, a reaction appears tending toward the condition that would have existed if the modification had not been impressed. This is by no means only true for organisms, and indeed it has been more clearly recognized in recent years by certain economists in their theoretical studies than by physicians and physiologists.

In order to construct a useful conceptual scheme, Hippocrates proceeded to analyze this process, as he abstractly conceived it, into elements. His analysis and the resulting elaboration of the theory need not detain us. To them we owe the survival of such words as "crisis" and "coction." But the theory, having served its purpose, is obsolete, like Ptolemy's astronomy.

We must, however, note carefully that this obsolete theory, like so many others, once served its purpose well. In particular, it was the firm support of the Hippocratic principle of expectant treatment and of the precept "Do no harm," a principle and a precept which still preserve their utility in the practice of medicine and even in government and the affairs of everyday life, and which are too often disregarded by physicians, surgeons and politicians.

The Hippocratic conceptual scheme suffers from one particular defect that should be carefully noted: It presents a view of the physiological system in a state of equilibrium, without giving a satisfactory picture of the constituent parts of the system or of the forces that operate between these parts. We now know that it is convenient and reasonably satisfactory to think of the constituent parts as chemical substances, fluids, cells, tissues and organs; and of the forces as the forces with which theoretical physics and theoretical

chemistry are concerned. Such a conception was not available to Hippocrates. Nevertheless, his conceptual scheme worked and for a long time worked well. This is, in fact, the test of a conceptual scheme and the only test: it must work well enough for the purpose of the moment. A conceptual scheme survives just so long and just in so far as it continues to be convenient to use it for the purpose of scientific work.

In a discussion of scientific hypotheses, Henri Poincaré once remarked:[4] "These two propositions, 'the external world exists,' or 'it is more convenient to suppose that it exists,' have one and the same meaning." The proof of Poincaré's assertion is that in scientific work no use can be made of the proposition "the external world exists" that can not just as well be made of the statement "we assume for the present purpose that the external world exists." Moreover, all our conceptual schemes are in a state of flux. There is hardly one we now use that was used in precisely its present form fifty years ago. It is therefore dangerous to believe that a conceptual scheme is a description of some ultimate metaphysical reality. In other words, belief in the "truth" of a conceptual scheme is for scientific purposes not only irrelevant, it is often misleading.

Our modern theory and our modern practice of medicine are so different in so many ways from ancient theory and practice that only by an effort of thought and imagination can we clearly conceive what ancient medicine was. I have tried to suggest that its merits were great and to specify the nature of some of these merits. To specify its deficiencies is almost unnecessary. However, we may note that until long after the time of Hippocrates experiment was but a feeble aid to observation and that applications of physics and chemistry were altogether lacking because there was nothing to apply.

In our modern period all this is changed. The sciences of anatomy, physiology and pathology, with their many branches, have grown up. They have become experimental sciences and they are becoming more and more sciences of applied physics and applied chemistry. This development has been accompanied by the growth of a conceptual scheme in which the broader generalizations of the medical sciences are incorporated and synthesized.

But it is still true that the investigator must have intimate, habitual, intuitive familiarity with the things that he studies, systematic knowledge of them and an effective way of thinking about them. This is just as true in the anatomical laboratory or the physiological laboratory or the pathological laboratory as it is in the clinic. There is, I believe, no broader induction from our experience of scientific work than this, and few inductions are more important.

The present state of the medical sciences, and indeed of each one of their many principal branches, is the accumulated result of innumerable experimental researches and descriptive studies. In general, each of these departments of science has grown up through the concerted labors of hundreds or thousands of intelligent specialists working with methods that are a part of their professional skill and that are more or less common to them all, working also with a conceptual scheme with which all are familiar. This aggregate of theory, like the methods, evolves and adapts itself to the state of the science produced by the work that has already been done. There is reason to believe that these are necessary conditions for the development of any science and, above all, for any science that deals with very complex phenomena. At all events, there seems to be no example of a highly developed science that is not the product of the labors of many men working skilfully in parallel and in succession with methods, systematic descriptions and classifications that they share. In the early stages of a science the theories are crude and the classification simple. They grow by trial and error and by adaptation into more refined theories and more complex descriptions. In short, the growth of the medical sciences, like that of all sciences, was not planned. That which survives does so because it is adapted to the needs of the scientists. In the development of a science facts when well established are always adaptations. On the other hand, theories and classifications survive, as Mach long ago pointed out, largely because they economize thought and effort, perhaps in some measure, also, because they are felt to possess what mathematicians call elegance.

There are certain deficiencies of the medical sciences to which little attention has been devoted, partly because they are inevitable in sciences that have grown up as these have grown up, partly because of the immense success that has been achieved by doing things as they have long been done, partly because of the peculiar difficulties involved in working effectively to remedy these deficiencies, and partly because such work is different in kind and in method from most of the work to which investigators are accustomed. These deficiencies depend upon the fact that living organisms are immensely complex and that the experimental sciences, by hook or by crook, analyze the concrete reality into relatively simple elements. But the complex reality is never describable by merely adding up these elements, for they exist in a state of equally complex interaction. In a man, as in a machine, effective description involves both a knowledge of the parts and a knowledge of how these parts interact. Moreover, in organisms not only are the parts very numer-

[4] "La valeur de la science," Paris (no date), p. 272.

ous but their interactions are especially numerous. Indeed, many biological adaptations consist precisely in the establishment of new interactions between parts.

Consider the case of hemoglobin. This substance is the sole carrier of oxygen, apart from merely dissolved oxygen, in the blood. Many years ago the conditions of the equilibrium governing the combination of oxygen with hemoglobin were satisfactorily determined. It was then discovered that the affinity of hemoglobin for oxygen is modified when the pressure of carbonic acid, or the alkalinity of the blood, or any one of several other things varies. Next, it appeared that the interaction between oxygen and carbon dioxide, previously unsuspected but revealed by this discovery, greatly enhanced the efficiency of the blood as a carrier of both oxygen and carbon dioxide. In short, this interaction is an adaptation. Finally, it became possible to piece the facts together with the help of mathematical methods and to describe the interaction quantitatively.

The problem of describing the interaction between these two substances in blood was in certain respects a different problem, involving different procedures, from the problems involved in the earlier studies. In particular, it was a problem that could be solved only by the use of certain mathematical procedures appropriate to the treatment of a system in which several factors in a state of mutual interaction are involved. This is a simple case and a mere partial description of almost any interaction in biological systems presents far greater difficulties, yet even here the difficulties were great enough to make the use of special mathematical methods necessary.

When we possess adequate knowledge of a system in which n factors are involved and have arrived at a description of this system in terms of the n factors, so that their interactions are also described, it is possible to reason successfully concerning changes in the state of the system in so far as these n factors alone are concerned, to a given approximation, in the process that is being studied. But when a further factor is also involved our reasoning can never be trusted and is in general illusory. It is perhaps partly for this reason that anatomists, physiologists and pathologists do not practice medicine, and this is probably the principal source of the familiar attitude of suspicion toward the laboratory sciences that may be seen among experienced clinicians. When men reason deductively about the complex affairs of everyday life they nearly always leave out something, or rather many things, both things they forget and things they don't know. More often than not their conclusions are therefore unsound. This is what Whitehead calls "the fallacy of misplaced concreteness." I am not sure that it can be appreciated by any one who has not experienced the difficult task of putting together the pieces obtained by analytical studies and thus building up an adequate description of a system in which many factors interact. Experience alone can teach most people the immense complexity of interactions between many factors, and the mathematical solution of such problems seems to be the only means of clearly conceiving the nature of such phenomena. However, it may be well to consider a simple illustration.

The fallacy of misplaced concreteness is very common in the form of arguments involving "other things being equal." Suppose one is concerned with the correlation between values of two variables, say, x and y. Suppose, further, that it is assumed either that nothing else need be considered or that, other things being equal, this correlation must have a single value. In order to fix our ideas, let the case be such that the unknown relations between x, y, and the other things can be expressed by the equation

$$y = \frac{x+z}{x+2}$$

in which the value of z is a measure of the other things, say, u, v, w, or in other words z is a function of these other things, that is, z varies when either u or v or w varies.

Let us now give z successively the three values 1, 2 and 3. Then:

$$(1) \qquad y = \frac{x+1}{x+2}$$

$$(2) \qquad y = \frac{x+2}{x+2}$$

$$(3) \qquad y = \frac{x+3}{x+2}$$

These equations are represented graphically on Fig. 1.

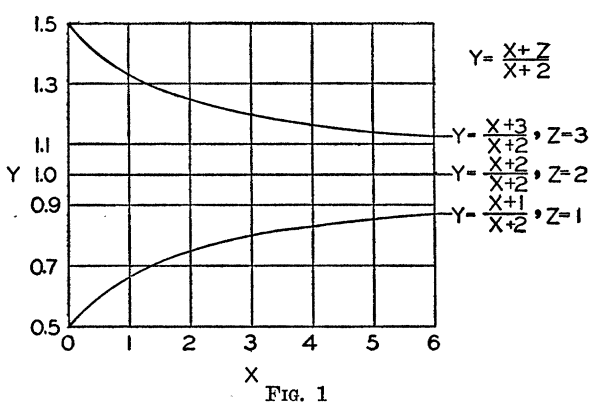

FIG. 1

Evidently when $z = 1$, x and y are positively correlated; when $z = 2$, they are independent, for y is constant while x varies; when $z = 3$, they are negatively correlated. Accordingly, any statement about the correlation of y with x must take account of what happens when z varies or must specify the value of z at which other things are equal. But z is a function of

u, v and *w,* which makes for further complications. In general, it may be said that all arguments involving such notions as other things being equal, or *pari passu,* are probably fallacious except when the universe of discourse is arbitrarily limited by abstraction, as in mathematics and theoretical science. There is never any reason to suppose that until the conclusions have been well tested such reasoning can be safely applied to the complex reality of daily life, especially in that part of reality with which either the medical sciences or the social sciences are concerned.

Imagine two men setting to work, one on January 1, the other on July 1, to measure the duration of daily sunshine. Each might well find after three or four months a high correlation between time (*i.e.,* date) and duration of sunshine. But the first would observe a positive correlation, the second a negative correlation. Neither would be likely to deceive himself on this account, *because he has intuitive familiarity with the things in question and systematic knowledge thereof,* but if he were dealing with a like result from the study of unfamiliar phenomena he would probably fall into error, unless he appreciated the danger of the fallacy of misplaced concreteness.

The medical sciences have suffered and continue to suffer from this fallacy. The rise of bacteriology and its influence upon medical thought and practice may be taken as an example. About the time of Pasteur's first discoveries, the thought of Claude Bernard and of other physiologists seemed to indicate a movement toward the study of the interrelations between many things and a recognition of this kind of study, synthetic physiology, as one of the foundations of the medical sciences and as the source of an indispensable point of view in all kinds of medical work. The discovery of specific pathogenic microorganisms seems to have led back to an oversimplification of thought about the origin and nature of disease. For some time at least, the tendency was to think of diseases as entities hardly less definite than atoms of oxygen or molecules of hemoglobin. Let us recall the fact that even hemoglobin *in situ* is not a single definite thing. The disposition was even more marked to think of the specific organism as the cause—the sole cause—of a specific disease and later to think of the specific antitoxin as the specific cure of that disease. Similarly, simple views of nutrition have prevailed. There was the epoch of calories; we now live in the epoch of vitamins. Hormones also are now having their day, and excess or deficiency of specific hormones, like excess or deficiency of particular vitamins, is often thought of as the cause of a specific disease.

All this oversimplified thinking has flourished and survived because up to a certain point it is convenient. In bacteriology and in chemical physiology it is more than this; it is probably necessary, for it affords the simplest possible conceptual scheme useful for certain purposes. Its usefulness is like that of Boyle's law for gases—volume varies inversely as pressure—which is true only approximately and only with important restrictions.

Indeed, nothing is more convenient than to be able to think of a phenomenon as simple, of its cause as single, and to feel that, accordingly, there is but one clear, straight path to be followed in action. This is just the kind of situation that satisfies men of action, who are fortunately numerous among doctors, and there is nothing about the growth of medicine in recent years that is more important or that has, upon the whole, contributed more to the relief of suffering and the cure of sickness than the increase in the number of pathological conditions, complex though they are in reality, that may be regarded as mere cases of a particular disease caused by a single cause and curable by the use of a single remedy. Perhaps nothing in modern medicine would have seemed so strange as this to Hippocrates.

The disposition to think in this manner has also been strengthened both by the influence of the conditions of modern life upon the practice of medicine and by increasing specialization. So it comes about that sick people are often regarded as cases rather than persons, and many things in the history of their sickness, many others concerning their environment, which were familiar to general practitioners of an earlier day, are likely to be overlooked and not even suspected. And yet disregard of such factors or of others that seem unimportant in the light of oversimplified theory leads to disaster. The disregarded factors are perhaps as often as not among those which the old-fashioned general practitioner intuitively recognized and which can sometimes be discovered only through intimate acquaintance with the patient and familiarity with his environment, with his life day-by-day, and with his family history.

There are signs of a growing interest in such considerations and of a corresponding realization that successful medical practice must take account of the patient as a person. One reason for this is not obscure, for the practice of medicine is action under the burden of responsibility, of partial responsibility for the outcome of a sickness and complete responsibility for the results of treatment. In the long run responsibility for decision and action is likely to cure the errors of oversimplified thinking, for, as Bacon said, sciences are judged by their fruits, and in medical practice the outcome of treatment is no less a test of the doctor's use of his conceptual scheme, and in the end of that scheme itself, than is the outcome of a laboratory experiment a test of the theory of the experiment.

Theories that will not work must be modified or abandoned. If they are constantly tested and will not work, sooner or later this will happen. But untested theorizing may continue indefinitely.

The facts of pathology and bacteriology, the specific procedures that have been discovered in the treatment of infectious diseases, in nutritional disturbances and in conditions associated with disturbances in the balance of hormones have given rise to theories that seem to be too well established to be overthrown. And yet we shall do well to remember the development of modern physics. For the diagnosis, prognosis and treatment of many cases, at all events, such theories are sufficient. Since they are thus successful, it seems probable that when they fail it is owing to disregard of factors neglected in the theories. In like manner Boyle's law successfully describes the behavior of a gas when temperature remains constant, but fails when temperature varies widely.

In sum, by the process of trial and error, the practice of medicine slowly eliminates fallacies of misplaced concreteness. There is perhaps no sphere of activity in which this elimination is going on more continuously and more effectively, for here theory on a grand scale and a great accumulation of systematic descriptive knowledge are at the foundation of thinking, which is therefore explicit. The thinking, however, is nearly always modified by intuitive familiarity born of experience and by at least a vague sense of the dangers of elaborate deductive reasoning. Among the best physicians it is therefore cautious. But, above all, the doctor's thinking issues in decision, and decision in action. Thus the thinking is continuously put to the test of observation and experiment.

One thing is lacking that would greatly contribute to the efficacy of this elimination of the fallacy, namely, a thorough understanding of the logical nature of the fallacy and easy familiarity with the complexity of the usual mathematical interrelations among many interdependent factors. For the interdependence of many variables can only be treated mathematically. Accordingly, acquaintance with this interdependence and familiarity with it are neither more nor less than acquaintance with and familiarity with the properties of certain kinds of mathematical operations in certain kinds of mathematical systems. But it is almost never possible—one might say it is never possible—to formulate clinical reasoning in mathematical terms. Therefore the doctor, lacking a certain logical discipline through inexperience of mathematical practice, can not clearly conceive the intricacy of the problems that confront him, for here, as everywhere else, practice is necessary to understanding.

So much for the present state of scientific medicine, of which the merits and defects seem to be fairly plain—at least in those aspects which concern us here.

The social sciences are very different from the medical sciences. Their development has been different; their present state is different. The habits, the attitudes, the procedures of social scientists are, in general, very different from those of medical scientists. And to-day the applications of medical science are innumerable, while it is hard to find effective applications of social science. Let us consider the two groups of sciences comparatively.

Near the end of the "Nicomachean Ethics,"[5] Aristotle prepares the way for his transition to the study of politics with the following remarks:

> Must we not, then, next examine whence or how one can learn how to legislate? Is it, as in all other cases, from statesmen? Certainly it was thought to be a part of statesmanship. Or is a difference apparent between statesmanship and the other sciences and arts? In the others the same people are found offering to teach the arts and practising them, *e.g.*, doctors, or painters; but while the sophists profess to teach politics, it is practised not by any of them but by the politicians, who would seem to do so by dint of a certain skill and experience rather than of thought; . . . experience seems to contribute not a little; else they could not have become politicians by familiarity with politics; and so it seems that those who aim at knowing about the art of politics need experience as well.
>
> But those of the sophists who profess the art seem to be very far from teaching it. For, to put the matter generally, they do not even know what kind of thing it is nor what kinds of things it is about; . . . For while people experienced in any department judge rightly the works produced in it, and understand by what means or how they are achieved, and what harmonizes with what, the inexperienced must be content if they do not fail to see whether the work has been well or ill made. . . .

Elsewhere Aristotle says,[6] ". . . people who have spent their lives observing nature are best qualified to make hypotheses as to the principles that bring great numbers of facts together."

Aristotle's criticism may still be made, more than two thousand years after, of much of our current social science, and his explanation of the grounds for his criticism may still be given. In their work social scientists rarely combine theory and practice, and still more rarely work hard, persistently, intelligently, responsibly, unremittingly on the phenomena, in direct, intimate relations with the men and things they study. Accordingly, they commonly lack intimate, habitual, intuitive familiarity with the objects of their investigation.

[5] Book X, from "The Works of Aristotle," ed. by W. D. Ross, The Clarendon Press, Oxford, 1925, Vol. IX.

[6] "De generatione et corruptione," I, 2, 10.

In the social sciences special methods and special skills are few. It is hard to think of anything that corresponds to a mathematician's skill in performing mathematical operations or to a bacteriologist's skill in cultivating microorganisms or to a clinician's skill in making physical examinations. Even in conducting an interview, skill is to be sought among physicians, or certain lawyers, rather than among the generality of social scientists.

Classificatory descriptive knowledge, which is so conspicuous in the medical sciences and in natural history and which has proved so essential to the development of such sciences, is relatively lacking in the social sciences. The most serious effort in this direction with which I am acquainted is Pareto's taxonomic study of the residues, that is, of the manifestations of sentiments. Successful and important as this is, it is but the beginning of a vast and difficult undertaking. Moreover, there is no common accord among social scientists concerning the classes and subclasses of the things they study, and there is even much disagreement about nomenclature.

The theories of the social sciences seem to be in a curious state. One body of theory, that of economics, is highly developed, has been cast in mathematical form and has reached a stage that is thought to be in some respects definitive. This theory, like those of the natural sciences, is the result of the concerted efforts of a great number of investigators and has evolved in a manner altogether similar to the evolution of certain theories in the natural sciences. But it is hardly applicable to concrete reality. As Marshall has said:[7] "There is . . . no scope in economics for long chains of deductive reasoning; that is for chains in which each link is supported, wholly or mainly, by that which went before, and without obtaining further support and guidance from observation and the direct study of real life." Pareto goes quite as far in condemning the applications of economic theory.

The reasons why economic theory is so difficultly applicable to concrete events are that it is an abstraction from an immensely complex reality and that reasoning from theory to practice is here nearly always vitiated by the fallacy of misplaced concreteness. Such application suggests the analogy of applying Galileo's law of falling bodies to the motion of a falling leaf in a stiff breeze. Experience teaches that under such circumstances it is altogether unsafe to take more than a single step in deductive reasoning without verifying the conclusions by observation or experiment. Nevertheless, many economists, some cautiously and others less cautiously, are in the habit of expressing opinions deduced from theoretical considerations concerning economic practice. There is here a striking contrast with medicine, where it is almost unknown for a theorist inexperienced in practice to prescribe the treatment of a patient, and where it is well understood that apprenticeship in a hospital is the only effective preparation for practice.

In other fields of social science theories are generally not held in common by all investigators, but, like philosophical systems, tend to be sectarian beliefs. This is true even in psychology, where the conflicts of physiological psychologists, behaviorists, Gestaltists, psychoanalysts and others sometimes almost suggest theological controversy.

Further, it should be noted that social scientists often seek something else rather than convenience in the construction of their theories. Consider, for example, the following remarks of Durkheim:[8]

> A concept is an essentially impersonal representation; it is through it that human intelligences communicate.
>
> The nature of the concept, thus defined, bespeaks its origin. If it is common to all, it is the work of the community. Since it bears the mark of no particular mind, it is clear that it was elaborated by a unique intelligence, where all others meet each other, and after a fashion, come to nourish themselves. . . .
>
> The collective consciousness is the highest form of the psychic life, since it is the consciousness of the consciousnesses. Being placed outside of and above individual and local contingencies, it sees things only in their permanent and essential aspects, which it crystallizes into communicable ideas.

And now note that we are well acquainted with a great number of essentially impersonal representations, such as acceleration in dynamics, entropy in thermodynamics or natural selection in biology, that we well know to have originated with a particular person or persons. Whatever his motive, Durkheim is endeavoring to set up a hypothetical entity that can only cause inconvenience in work because, so far as we know, consciousness is a function of, or is associated with, individual nervous systems. Long ago the biologist Le Dantec said of the Ehrlich school of immunity that when they discovered a new phenomenon they invented a *phenominine* to explain it. And very much longer ago William of Occam stated the precept known as Occam's razor: "Entia non sunt multiplicanda praeter necessitatem," which is to say that our conceptual schemes should contain no more than the necessary elements.

On the whole, it seems fair to say that the social sciences in general are not cultivated by persons possessing intuitive familiarity, highly developed, sys-

[7] "Elements of Economics of Industry," Macmillan, London, 1905, p. 397.

[8] Selections adapted from "The Elementary Forms of Religious Life," pp. 432–37, in R. E. Park and E. W. Burgess, "Introduction to the Science of Sociology," University of Chicago Press, 1921, pp. 194–96.

tematic, descriptive knowledge, and the kind of theories that are to be found in the natural sciences.

There is not a little system-building in the social sciences but, with the striking exception of economic theory, it is of the philosophical type rather than of the scientific type, being chiefly concerned in its structural elements with words rather than with things or, in old-fashioned parlance, with *noumena* rather than with *phenomena*. This involves what I have already described as thinking, talking and writing about vague or general ideas or "concepts" which do not clearly relate to concrete observations and experiences.

For scientific purposes, or for clear thinking of any kind, experience shows that such things will not serve. In support of this assertion I venture to appeal to the late Justice Oliver Wendell Holmes, who once remarked:[9] "I have said to my brethren many times that I hate justice, which means that I know if a man begins to talk about that, for one reason or another he is shirking thinking in legal terms." I shall presume to make a single exegetical remark on Holmes's text: the phrase "shirking thinking in legal terms" may be generalized to read "shirking thinking in terms that can be used for even rough and ready logical purposes or for any sort of clear thinking."

I believe it not unfair to take as an illustration of what is here in question Reinach's definition of religion:[10] "An ensemble of scruples which impede the free exercise of our faculties." After stating this definition, Reinach at once goes on to remark: "This minimum definition is big with consequences, for it eliminates from the fundamental concept of religion, God, spiritual beings, the infinite, in a word, all we are accustomed to consider the true objects of religious sentiment." He has previously pointed out that definitions of religion are many and diverse and that they have not been found convenient in scientific work. The general confusion that has ensued from their use might well suggest the inference that to set up definitions of such a word, at all events without taking very unusual precautions, is inexpedient. Reinach's definition, like most definitions of religion, is a more or less precise designation of attributes of some religions; in other words, the statement of what the author believes or wishes to believe a satisfactory *differentia*. Reinach's remark about the consequences of his definition is almost comic. What are the possible consequences of adopting a definition? Assuredly, no definition can modify the phenomena or the relations between the phenomena. On the other hand, it can and ordinarily does modify the behavior of the person who accepts it, and Reinach naïvely admits as much by noting that certain things are eliminated from the fundamental *concept* of religion. Now, what he eliminates in the beginning will, unless he blunders, not be found in his final conclusion.

Why does Reinach speak of "scruples which impede," and not of needs which further "the free exercise of our faculties"? He is evidently referring to phenomena which arise, at least in part, from systems of conditioned reflexes, and his restriction in pejorative. We know that hostility to contemporary religions was common in Parisians of his class at the time when he wrote "Orpheus." It is therefore not unlikely that such hostility partly explains the defects, from the scientific point of view, of his definition.

A further difference between most system-building in the social sciences and systems of thought and classification of the natural sciences is to be seen in their evolution. In the natural sciences both theories and descriptive systems grow by adaptation to the increasing knowledge and experience of the scientists. In the social sciences systems often issue fully formed from the mind of one man. Then they may be much discussed if they attract attention, but progressive adaptive modification as a result of the concerted efforts of great numbers of men is rare. Such systems are in no proper sense *working* hypotheses, they are "rationalizations." Or at best they are mixtures of working hypotheses and "rationalizations."

Thinking in the social sciences suffers, I believe, chiefly from two defects: one is the fallacy of misplaced concreteness, the other the intrusion of sentiments—of Bacon's Idols—into the thinking, which may be fairly regarded as an occupational hazard of the social scientists. There can be little doubt that this intrusion is one of the factors that make the quotations just cited from Durkheim and Reinach unacceptable as science. Let us consider one more example.

Macaulay says:[11] "[The errors in the works of Machiavelli] arise, for the most part, from a single defect which appears to us to pervade his whole system. . . . The great principle, that societies and laws exist only for the purpose of increasing the sum of private happiness, is not recognized with sufficient clearness." What is the source of this great principle? Evidently it is not an induction from experience. What is the meaning of purpose as applied to the existence of societies? From a scientific point of view, purpose must be somebody's purpose. Like consciousness, it is associated with individual nervous systems. How can the sum of private happiness be measured? Assuredly not with any instruments or by

[9] "Justice Oliver Wendell Holmes: His Book Notices and Uncollected Letters and Papers." Edited by Harry C. Shriver, New York, 1936, p. 201.

[10] "Orpheus, A History of Religions," Horace Liveright, New York, 1930, Introduction, p. 2.

[11] Essay on "Machiavelli."

any procedures that were at the disposal of Macaulay. Is it not evident that Macaulay's "great principle" and his "purpose" of the existence of societies are both expressions of his sentiments, and that "the sum of private happiness" is, in the sense of the logic of modern science, a meaningless phrase? Finally, what is the probability that if Macaulay were writing his essay on Machiavelli in September, 1940, he would feel disposed to make similar assertions? The sentiments, like most other things, vary with time.

Sentiments have no place in clear thinking, but the manifestations of sentiments are among the most important things with which the social sciences are concerned. For example, the word "justice" is out of place in pleading before the Supreme Court of the United States, but the sentiments associated with that word and often expressed by it are probably quite as important as the laws of our country, not to mention the procedure of the Supreme Court. Indeed, such sentiments seem to be in many ways and at many times the most important of all social forces. The still dominant European intellectual tradition treats such things as if they had their origin in the logical thinking of those who manifest them. Yet the sentiments arise and manifest themselves in a manner that is hardly more appropriate for such treatment than is the manner in which the instincts and the passions manifest themselves.

The attribute "justice" is by men variously ascribed to various actions. This ascription varies with time, with place, with age, with sex, with social status, with purpose, with economic interests, with emotional excitement and with innumerable other factors. For the word "justice" is the expression of an attitude. In general, it is irrelevant to inquire whether an assertion which is the expression of an attitude is logically and objectively true or false.

Such attitudes and sentiments are closely related to conditioned reflexes and in part arise from the process of conditioning. This may be illustrated by considering the contrast between the meanings of such pairs of words as house and home, woman and mother, man and comrade, acquaintance and friend or enemy.

The acquired characters of men may be divided into two classes. One kind involves much use of reason, logic, the intellect; for example, the ordinary studies of school and university. The other kind involves little intellectual activity and arises chiefly from conditioning, from rituals and from routines; for example, skills, attitudes and acquired sentiments. In modified form, men share such acquired characters with dogs and other animals. When not misinterpreted, they have been almost completely neglected by intellectuals and are frequently overlooked by social scientists. In their study a great opportunity seems to present itself for the application of physiology.

The conclusions of this comparative study are as follows: First, a combination of intimate, habitual, intuitive familiarity with things; systematic knowledge of things; and an effective way of thinking about things is common among medical scientists, rare among social scientists. Secondly, systems in the medical sciences and systems in the social sciences are commonly different. The former resemble systems in the other natural sciences, the latter resemble philosophical systems. Thirdly, many of the terms employed currently in the social sciences are of a kind that is excluded, except by inadvertence, from the medical sciences. Fourthly, sentiments do not ordinarily intrude in the thinking of medical scientists; they do ordinarily intrude in the thinking of social scientists. Fifthly, the medical sciences have made some progress in the objective study of the manifestations of sentiments; the social sciences, where these things are particularly important, have neglected them. This is probably due to the influence of the intellectual tradition. Sixthly, in the medical sciences special methods and special skills are many; in the social sciences, few. Finally, in the medical sciences testing of thought by observation and experiment is continuous. Thus theories and generalizations of all kinds are constantly being corrected, modified and adapted to the phenomena, and fallacies of misplaced concreteness eliminated. In the social sciences there is little of this adaptation and correction through continuous observation and experiment.

These are very general conclusions to which, as I have already said, there are numerous and important exceptions. Perhaps the most important exceptions may be observed in the work of many historians, of purely descriptive writers, and of those theoretical economists who scrupulously abstain from the application of theory to practice.

When we reflect upon these differences between the two kinds of studies of men, shall we not do well to think also of the fruitfulness of the medical sciences and of the unfruitfulness of the social sciences? But let us not try to say what is here cause, what effect. Human interactions are intricate and obscure, and the art of studying them is difficult. That is, we can but feel, a part of the cause of the habits of thought and procedure of social scientists, and of the unfruitfulness of their science as well. Yet, assuredly, there is no simple cause of the present condition. What we can say with some confidence, for it is the lesson of experience, is this: The social sciences will become more fruitful when in certain ways the thought and procedures of social scientists conform more closely to those of medical scientists.