SCIENCE

FRIDAY, JANUARY 12, 1917

CONTENTS

MSS. intended for publication and books, etc., intended for review should be sent to Professor J. McKeen Cattell, Garrison-on-Hudson, N. Y.

THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE

SPECIALIZATION AND RESEARCH IN THE MEDICAL SCIENCES[1]

MODERN scientists are not encouraged and are become less inclined, except in the afterglow of an active life, to indulge in metaphysics. The visualization of material phenomena, particularly when set in motion by deliberate experiment and observed in their successive stages, tends to replace speculation as to a more complete, though less verifiable series of facts. This reliance in the natural sciences on observation and experiment rather than on ratiocination is responsible for the great and rapidly increasing body of useful knowledge we possess.

Philosophical treatises by even conspicuous representatives of the natural sciences have seemed to me to differ from those of the metaphysicians in that the former apparently fail to appreciate that the metaphysical game is played subject to certain rules which have the same purpose of order as the rules in other games. Philosophy is apparently a subject like fine arts, about which many people think they have intuitional knowledge. We judge pictures as bad or good not on the basis of certain criteria that have come through the ages to be recognized as essential, but in accordance with whether we like or dislike them. In the same way we may think, because we have a certain facility in the exposition of scientific data, that we can offhand write

[1] Address of the vice-president and chairman of Section K, American Association for the Advancement of Science, New York meeting, December 29, 1916.

an explanation of the larger relations in as direct and convincing a manner as William James did his "Pragmatism." As example of attempts of this sort, I am thinking of books like Haeckel's "Riddle of the Universe," and Shaler's "The Individual." Metchnikoff in his "Studies in Human Nature" would seem an exceptional biologist who has taken the pains to learn the metaphysical rules, and it is an interesting proof of the modern discouragement of speculation on the part of scientists to be credibly informed that the publication of this book sufficed to debar its author from election to honorary membership in one of our most exclusive national societies. This discouragement reflects what I believe to be a fundamentally incorrect attitude in many of us toward metaphysics. We regard it largely through ignorance of its methods, and lack of appreciation of its heuristic value, as a grab bag into which are dumped all conceptions that can not be demonstrated, or as a method adopted from unworthy motives by the scientifically inaccurate.

So much in explanation of an attempt as an alien to speak a language with which I am not familiar, the expressiveness of which, however, I venture to think I appreciate. So much in extenuation of an attempted exposition of one phase of scientific method. The thesis of my remarks is that the best method of accomplishment in the medical sciences is to adopt the bloodhound method of nose to trail, to encourage ourselves in specialization and still more specialization, to dig deep rather than to spread smooth.

My traveling acquaintance, a lawyer, could not understand, when we passed the power dam skilfully blocking the mountain torrent, why I could not explain to him the essential principle of converting the energy of the foaming water into electrical voltage. "I thought you were a scientist," he remarked scornfully, "That is a scientific problem, isn't it?" I had no crushing retort ready for him, but I hope I may at a later day explain and perhaps justify my more or less deliberate ignorance to you, a more discerning audience. The public expect results, but usually misunderstand methods of obtaining them; they are willing to accept the greater returns following greater specialization, but do not always realize that effective specialization takes even more time than generalization, and to some extent excludes it.

I imagine that many of us, if we were to present an ideal system of intellectual self-development in graphic form, would sketch a pyramid with a broad base of knowledge representing the lower educational years, sloping and narrowing upward toward the increasing specialization of a life work. It is inevitable that each additional unit of knowledge, each brick in the structure we are raising, will eventually take its place in some definite relation to every previous brick in the mental edifice by which we represent to ourselves the external world. But is this ultimate structure the one we should have in mind in training ourselves as brickmakers? Do we not confuse this edifice, toward which we may contribute a unit, with the plan by which we develop as contributors? The pyramid is a not unpleasing and certainly an enduring structure; it met admirably the needs of a tomb for Egyptian kings; it may serve as a dignified mausoleum for acquired facts, but the more rapid acquisition or reception of new facts may be better served by an essentially different construction. Certain more modern needs are better met, according to Signor Marconi, by a very thin and lofty antenna. May it not be that the wireless outfit resting on no considerable base, though carefully supported by connecting strands, typifies the modern method of development of one's powers for productive scientific work? Does not this delicate apparatus, shooting up straight from the earth, allow expansion into the unknown which the self-limiting convergence of the planes of the pyramid excludes?

At all events, it is no longer possible for one to master all, or even several contributory sciences, before turning his attention in a productive way to one of them; there is not time or strength enough. We are no longer in the middle ages, where a genius like Dante could reflect all knowledge that had preceded him in a set of scholastic and poetical treatises, or another like Leonardo da Vinci could contribute to several arts and sciences methods that were fundamental. I appreciate, I believe, the surprising vigor of Leonardo's intellect, but am not willing to admit that his astonishingly successful versatility proves him a type of superman that has ceased to exist. I feel sure that Leonardo's intellectual equal may well be among us to-day, but could never by any chance make notable contributions to subjects so diverse as painting, sculpture, engineering and mathematics. This would seem to prove not that the race of man has fallen off, but that each of the subjects has so grown in complexity as to require a lifetime to master. It is no little factor in success in any subject to be early in the field, to be the first explorer. In many respects it requires greater powers of observation to detect further important details in a landscape, the important and perhaps more obvious features of which have already been described by another. The earlier observer, moreover, has the undoubted advantage of entering on his work with a mind untrammeled by the notions of numerous predecessors.

The most modern equipment for scientific advance need be burdened with no very heavy impedimenta of fact—the newer science develops or rediscovers the methods of other sciences at need without having mastered their content in fact. To justify this light-marching order, which I venture to recommend for the scientist in his invasion of the unknown, I must outline my conception of the nature of scientific progress and then discuss to what extent each science is dependent on other sciences in this advance. Let me repeat that I have in mind primarily the newer biological sciences, particularly those that relate to medicine, and am considering them in relation to one another and to the more fundamental sciences of mathematics, physics and chemistry. My remarks doubtless do not now apply to these latter fundamental groups which seemed to have developed into a more closely correlated and perfect whole where interdependence seems more constant. Am I not correct in assuming that in its early development, chemistry, for example, was less dependent on mathematics and physics than it is to-day? May we not look upon these three sciences as similar in their growth to three adjacent trees which at first stood clear from each other, but which in their further development have intertwined their branches and roots so that they now appear from a distance as more nearly an entity?

At all events progress in the biological sciences depends, first, on discovery of new facts by purely observational and by experimental methods, and, secondly, on the elaboration of hypotheses and theories as a means of uniting these data and as introductory to more facts. Let us consider in some detail the method by which each of these advances is made and in what respect knowledge of kindred sciences is essential in this analysis and synthesis.

It seems obvious to us now that proper appreciation of any scientific phenomenon must depend first on a knowledge of its component parts and their functions. This analysis or dissection not only must precede, but seems at once more intimately scientific than the synthetic stage that follows. I here use the word "scientific" in the specialized sense of acquiring data concerning natural phenomena. The second or synthetic stage is more metaphysical in that it considers data that have been acquired in their relation to one another. The first phase is more intimately scientific, then, in that we are actually in contact with those elements which we describe as facts. The second or synthetic stage is, however, fully as essential to progress in that *without* it we should never pass from a known group of facts to one that is unknown. The synthesis of small or less certain groups of facts gives rise to the working hypothesis which, in its proving or disproving, leads to other facts. Larger or more certain groupings of fact constitute a theory which, in its restatement of evidence, serves as a point of departure for further advance. A theory may stand for an indefinite period as a complete statement of the facts with which it deals, or it may soon be supplanted by a better one. In either case it has its heuristic value.

In eliciting facts certain methods are required, in the larger sense methods of discovery, in a more restricted sense methods of technical precision. It might be thought that in methods of discovery, certainly, a knowledge of the methods of other sciences would be essential, and so indeed they are, but in no exclusive sense. It has never ceased to surprise me to find from conversations with my colleagues in other branches that all experienced investigators employ the *same* methods of discovery—the materials we handle may be as diverse as you like, the technical details incomprehensible to one another, and yet the methods of attack on the unknown remain the same. We all gravitate through experience into the same channels of reasoning, the same methods of planning experiments, of erecting working hypotheses, of rejecting them when they fail of verification, or of trying them further when they pass the first test satisfactorily.

There remain, then, methods of technical precision. For the purpose of this discussion of the usefulness to the biological medical sciences of more fundamental or of merely contributory sciences, we may consider methods of technical precision as statistical, instrumental or experimental. No claim is made as to the inclusiveness of this cataloguing.

There is some dispute, I believe, as to whether statistics constitute a separate science or merely a method. At all events, statistics are used as a method in all sciences or groupings of fact. Of late, statistics are used to a large extent in certain biological work, notably in the branch of hygiene dealing with vital statistics, and in certain more theoretical branches as the laws of heredity (Mendelism). It is obvious that any science which in its analytic phase accumulates a mass of figures or data will need statistical methods. I am not aware that statistical methods can be learned apart from the constituent facts which they are aimed to elucidate. It seems to me that such methods are best learned by using them, and that there is no particular object in learning the use of statistical methods in reference to wages, let us say, for the purpose of applying them in investigations of the incidence of tuberculosis. In either case we must refer to treatises written by those who have used statistics extensively both for the general methods and causes of error involved in their use. Statistics, to repeat, is not a separate science, but a method employed at need, and a part of any science that uses them.

We have next the use of methods of precision. This may imply the use of a piece of apparatus, or a reaction that has been of service in another science. The use of such a piece of apparatus may suggest itself synonymously with the needs which it was intended to meet. Thus, if in one of our biological products we have reason to wish to measure total nitrogen or amino nitrogen, we should undoubtedly turn to a chemist who would suggest the Kjeldahl or the Van Slyke methods. The reference suggests at once what I should regard as the best method of reapplying the methods of one science to another science, namely, collaboration, or intimate contact with specialists in various branches. The man who thinks he is trained in one science by having passed through it a few years before, may well fall into the error of using methods he has learned rather than better methods since discovered and currently employed by specialists. A personal example may illustrate this fact. A few years ago one of my associates and I were working on a problem which finally required a chemical estimation of the amount of glycogen in the liver. This determination necessitates the rapid reduction of glycogen to glucose, followed by its quantitative estimation from the amount of copper oxide reduced. Fehling's technique had been the classical method followed in such estimations. Not trusting to our own judgment as to superiority of this method, we consulted a graduate student in the department of biochemistry who was working constantly with glucose determinations of this sort, and, following his advice, adopted the modification of Fehling's method which had recently been made by Bertrand. A few months later, on visiting a large eastern hospital where determinations of the amount of glucose in the blood were being carried out, I learned that six months' data had just been discarded, owing to the fact that the physician who was conducting the experiments had trusted his rather unusual training in biochemistry and had overlooked Bertrand's important modification, which he later adopted and which we employed throughout our study, owing to the fact that we had deferred to the opinion of a specialist.

It is doubtful if methods of experimentation, purely speaking, can be carried over from one science to another. We have stated that the methods of discovery in the broader sense are the same in all sciences, however different the component factors may be. In methods of experimentation, however, variation in factors counts. I have constantly been struck with the fact that the chemist experiments in a manner that is essentially different from the one which my work demands. Chemistry is a far more exact science than experimental pathology in the sense that the factors with which chemistry deals are better known. It is interesting to note, however, that a chemist may, and frequently does, accept certain biological evidence as proved which we should reject as inconclusive, owing to the omission of certain controls or checks. This difference in viewpoint is dependent on the failure of the chemist to appreciate certain fluctuations in living material which it is impossible now and will perhaps to some extent ever remain impossible to determine at a given moment. It does not suffice, moreover, to determine the mean of such a variation in a great number of instances, for the purpose of obviating controls in a given experiment.

In dealing with the interactions of two substances in chemistry we have to begin with, under the simpler and usual conditions, union in fixed and in multiple proportions. It is true that in reactions between a weak acid and a weak base there is union in variable proportions, so that a series of compounds are formed. But in general it may be said that in chemical reactions the results may be foretold when the effect of controllable factors such as dosage, temperature, atmospheric pressure and the like, have been determined. The substance concerned in the reaction, and the conditions that affect it, have been rigorously tested and are understood, so that a given result can always be counted on. The experiment controls itself when properly performed. On the other hand, no one can tell what will happen if he injects a million staphylococci into the ear vein of a rabbit. The animal may be dead the next day with no evident lesions; it may die a week later with abscesses in various parts of the body, or it may show no symptoms and recover perfectly. These disparate results are due to the fact that in an experiment of this sort we are confronted with two sets of variable conditions inherent on the one hand in the living microorganism that is injected and on the other in the experimental animal. We recognize the existence and to some extent the range of certain of these variables, but remain ignorant of many of them; the majority of them are inherent in the condition we designate as life and disappear in death. It is incorrect to assert that our ignorance of them is due to an interest in vitalism. We are free to admit that our science is very young, that our data are relatively few, and that our ignorance of the factors concerned is great. And yet we have a group of significant, reliable and practical phenomena that we can reproduce at will when we handle these variable factors in our own way. Many of our reactions, although indefinite from the standpoint of chemistry, are of a delicacy that chemistry rarely, if ever, attains. The point of interest here is that the experimental methods of present-day chemistry not only have not led us to new facts in our field, but do not help us much to explain or control our present ones. In the experiment cited we can not assert from previous experience exactly with what point in the range of either variable factor we are confronted, we can not previously determine our conditions and know that they now actually exist. We know in a general way that in the experiment I have outlined we have to deal particularly with fluctuations in the virulence or pathogenicity of the staphylococcus concerned and with variations in the resistance to infection in the individual rabbit. Our type of experiment, then, is never complete unless we introduce numerous simultaneous and external controls. In the particular problem I have cited, we find that although one million staphylococci killed Rabbit No. 1 yesterday, a subsequent transplantation of the microorganism fails, in the same dose, to kill Rabbit No. 2 to-day. It could be determined that this result is due to a loss of virulence in the microorganism by the introduction of Rabbit No. 3 which is given twice the dose and dies as did No. 1 yesterday. Individual variations in resistance may, to a great extent, be avoided by choosing for the experiment rabbits of the same weight, raised under the same conditions, or, better still, from the same litter.

As a further illustration of the difference in viewpoint between the chemist and ourselves, let me suggest that the tendency of the former on entering our field of activity would be to devise a more precise method of estimating the number of bacteria used in the experiment rather than to introduce such controls as I have mentioned.

The problem I have given you is one of the simplest with which we have to deal. Conceive of the far greater complexity if we introduce an immune serum against the staphylococcus in such an experiment designed to increase the resistance of the rabbit to which it is given, and you will imagine where the real complexity of our science begins. Such a serum differs in its potency with the individual animal that has produced it, with its age after withdrawal from the animal body, and with the method by which it has been conserved; in other words, it introduces another variable factor. I may again define our mode of experimentation as differing from that of chemistry in requiring the introduction of simultaneous, external controls, the object of such controls being simply to define the effect of those conditions which we recognize as contributing to a given result.

Such differences as these, then, lead me to think that even great experience in one type of experiment will not fit one directly for experimentation of another sort. I do not mean to intimate that training in methods of precision is not of value, however different the conditions may be, but the best training for a given end lies in work and more work with the intrinsic materials involved, not so much as leading to greater technical accuracy as tending towards the establishment of an essentially specialized experimental viewpoint.

We come now to mention the value of multiple scientific experiences as fitting one for the larger synthesis or generalization in a given science. I have not reached that age where such generalizations as I mean appeal to me as the more important field in the experimental sciences, although I recognize that they are eventually necessary to present our work as a whole and in its practical aspects to the world at large. Such generalizations do, of course, imply factual knowledge of the wider sort, and I must confess to being awed at times by the aptness of apparent analogies between the better-known conditions which exist in one science in explaining formative theories in another science. Personally, I also usually doubt the rigorous exactness of the conclusions drawn in respect to the significance of any one science by one who handles freely the data of several sciences. I suspect at once the reportorial viewpoint, the existence of second or third hand, and ever so slightly garbled information. I am inclined to trust the solution of my problems to a combination of specialists rather than to the superman. Here again I plead for collaboration.

In our great, vital and complex science of medicine we can see, I think, an illustration of the ultimate value of intensive specialization and of deliberate or chance collaboration. Out of indefinite, speculative, empirical, bedside methods of the practitioner, have emerged, through the stimulus of the exact sciences, a growing number of increasingly accurate and effective laboratory branches. These laboratory sciences have become of practical value in the diagnosis, prevention and cure of disease, precisely as they have become separate entities and have fallen into the hands of whole-souled and intensive specialists. I make no mention here of the intellectually satisfying value of a concrete body of similar facts which constitutes a science. The relatively rapid applicability of the data of laboratory medicine to human welfare is at once an enormous stimulus to accomplishment and also a potential danger, owing to the possibility of too rapid generalization and application to meet a practical need. There are many who are impatiently waiting with individual needs in mind to apply any method of apparent value we may devise, and it requires at times no little self-restraint to withhold an apparent innovation for greater certainty. Over-enthusiasm greets the advent of every fact that has the least suggestion of practical value. We have ourselves lived through successive eras in medical progress when from each group of specialists was expected the last unraveling of the human mystery. Morphologist, physiologist, bacteriologist, and biochemist has each had his turn. The ultimate truth lies in all these sciences, and again in no one of them alone. The danger to sober advance is not in the successive enthusiasms with which each specialty has been received, but in the dabbling methods of a group of investigators who have attempted to "follow the ball"; investigating a given medical problem in successive years by the latest method in vogue, becoming rapidly in turn pathologist, physiologist, chemist.

The ultimate solution of each medical problem lies in the combined attack of a group of investigators converging from different points of the scientific compass, each trained in a separate method and employing it intensively. The problem of cancer, for example, is now being studied by the morphologist who describes hitherto undifferentiated structures in the malignant cell by special staining methods; by the immunologist who demonstrates the presence of reaction bodies in the serum of cancerous animals and human beings; by the chemist who shows that certain substances given parenterally inhibit or stimulate cell growth, or who produces similar results by the use of various diets; and by the expert in vital statistics who shows the actual increase or decrease in incidence of the disease; by the biologist who shows in Mendelian tables the heredity of the disease in animals; or, again, the effect of cross-breeding on transmission of the tumor; and by the physicist who demonstrates the effect on the tumor growth of X-rays or radium. I have not exhausted the category, but merely wish to indicate that the significant advances in each of these methods of approach are made by specialists. Do not misunderstand me to mean that any one of these investigators may not be led by his work to assume seriously and purposefully the activities of any other type. Pasteur was a chemist who became a biologist and probably the greatest contributor to medicine, although without medical training, because he followed his problem to the bitter end into whatever field it led, with little regard for the fact that he was, technically speaking, unfit to encroach on medical territory. He rediscovered medicine from a new angle, untrammeled by any preconceived notions of how disease was regarded. Ignorance of veterinary medicine did not prevent him from isolating the causative agent of anthrax in cattle and from utilizing an attenuated virus in its prevention. Failure to have studied the central nervous system of man was no obstacle to the man who discovered the essential cause of hydrophobia and the means of preventing it. Imagine insisting that Pasteur's curriculum should have included medicine as a necessary prerequisite to the discovery of the fundamental principles of the infectious diseases.

I hope you will not take my remarks as indicating anything but the highest appreciation of instruction in the sciences in general as the best training for the youthful mind, or as contributive to general culture. You will not accuse me of advocating early vocational training without a preliminary survey of the realm of knowledge. To be specific, you will not imagine that I discredit the now universal requirements that premedical students should acquire a modicum of chemistry, of physics, and of biology as furnishing an intelligent, scientific viewpoint for their subsequent study of medicine. Such a survey is not only good, but very properly prescribed as necessary. My remarks have been directed at a very different level and type of intellectual development from this; we have been considering our own particular problems as investigators. What I have been interested in discrediting is the persistence of ideas of machine-made education into the productive years of scientific life; the idea that if we seek eventually to become effective we should take care to perfect ourselves laboriously in each of the branches that have been regarded as fundamental. There is a real danger that we may spend our lives preparing ourselves for an indefinite piece of work that we never even start. It is, of course, much easier to continue preparing ourselves, to keep our scientific judgment strictly symmetrical by endeavoring to fit in each contribution that *others* make into its proper place, rather than to insist that one particular piece of work must be done *now* and to the exclusion of everything else. This insistence, however, I consider to be the true raison d'être of specialization, the only basis of real productivity.

These remarks, to repeat, are not a recommendation for educational anarchy, but an explanation of how a somewhat one-sided development may not only not be inconsistent with, but indeed the very essence of highest accomplishment. This is not so much a recommended program as an explanation of how things really work out. It is intended to some extent as helping to dispel the discouragement that I believe has come to many of us when we cease to be mere recipients of information and in a position to think and to do for ourselves in a chosen profession. I must confess to many hours of doubt for more years than I care to admit, as to whether I should really accomplish *anything*, owing to the fact that I had failed to become a good chemist *en passant*. It was always and increasingly too late to turn back and repair the errors or omissions of education, and as my problems finally gripped me instead of merely inviting me, I silently gave up the struggle to remodel my life. And in following some of these problems in certain of their ramifications, I found that although I could never hope to learn chemistry, I was curiously enough collaborating in investigations that utilized that very type of chemistry which my work required. I was absorbing in this intimate way certain very restricted forms of chemistry in the making.

Out of such experience has gradually formed a certain working philosophy, or, better, a philosophy of work which I have tried here to present to you. Those of you with less limitations may well question much that I have said, you may assert that breadth does not of necessity mean superficiality, and per contra, that digging a hole does not necessarily mean that it is deep, but in certain respects I am sure you will agree with me. Specialization in science, even in the narrowest sense, is essential to real accomplishment. Any extension of knowledge is dependent on an attentive consideration of a relatively small group of facts to the temporary exclusion of less related facts. To a great extent the smaller the group the greater the concentration possible, and the greater the resultant accomplishment. Each science is independent in so far as the individual investigator is concerned, and correlatively all sciences can be learned with each specific scientific problem as a point of departure, at least so far as the needs of that problem demand. On the solution of problems depends the future of science.

FREDERICK P. GAY

UNIVERSITY OF CALIFORNIA