Psychology and Scientific Research. II. Scientific Inquiry and Scientific Method

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WE HAVE HAD TO DISCUSS the nature of and the apparent reason for scientific inquiry because "scientific research" has so often been thought of merely as a technique or method of investigation. What we know as the scientific method is a means for pursuing scientific inquiry. If we do not bear this in mind, real progress in scientific research is apt to be thwarted. For the implicit equating of scientific inquiry and scientific method to a technique of investigation leaves out an all-important consideration: the problem of formulating a problem for scientific investigation. For the formulation of the problem for investigation must contain within itself the possibility of going beyond what is now scientifically established if it is to satisfy the definition of scientific research. If the formulation of the problem does not do this, then succeeding steps in investigation are futile.

Although there is likely to be little argument here, some "research" in psychology seems to reflect only a lip service to this fundamental tenet. It may be appropriate to underscore the point here in the words of modern scientists. Whitehead has pointed out that "no systematic thought has made progress apart from some adequately general working hypothesis, adapted to its special topic. Such an hypothesis directs observation, and decides upon the mutual relevance of various types of evidence. In short, it prescribes method" (*12*, 286). Einstein and Infeld have written that "the formulation of a problem is often more essential than its solution, which may be merely a matter of mathematical or experimental skill. To raise new questions, new possibilities, to regard old problems from a new angle, requires creative imagination and makes a real advance in science" (*5*, 95). Oppenheimer indicates that the experimental techniques of science enable us to define and detect our "errors of conception" (*9*, 22).

It should be emphasized that if an hypothesis is to be regarded as adequate it must be more than a statement or description of current data and more than a prediction that data will reproduce themselves. An hypothesis must be tested both in terms of its ability to predict immediate events and its promise of leading to further, more adequate hypotheses. For in scientific procedure there is a never ending process of hypothesizing, a constant flow of one hypothesis from another, with each hypothesis trying to go beyond established formulations in its inclusiveness.[1]

It is the way in which the investigator poses his problem that determines where he will come out—what functional activities he will feel have a bearing on the problem, which of these he will use as the bases for standards in empirical investigation, and what methodological procedures he will follow or try to devise. In this connection it is relevant to note that the popular conception of what makes a scientist "great" is that he has solved problems that have long baffled others. While this may be true enough, a review of the history of science will show that in general the solution of a problem is relatively easy once the problem has been posed and that the real scientific contribution of those scientists we now regard as outstanding is due to the way in which they have formulated problems which they or others have solved. The tremendous advances in the physical sciences since the 17th century, for example, are due more to improved formulations than to changes in methodology. In the 17th century and continuing into the 20th, science sought all-inclusive "laws" and felt that reality was firmly in hand. But today both all-inclusive laws and reality seem more elusive than ever. Contemporary physics is seeing its ultimate particle disappear, physiology is realizing that it is not dealing with the classical closed energy system. The need for a basic conceptual reformulation to bring about newer

[1] In a memorandum concerning the conceptualization of novel problems, Horace Fries has called attention to the necessity of making a distinction between an increase in our understanding and the solution of an immediate problem. He points out that "the degree of success in the resolution of the difficulty is always relative, i.e., better or worse relative to the interests or desires affected. But the *solution* of a problem brings about an adequate *resolution* of the difficulty in proportion to the adequacy in which the difficulty is organized into a problem, i.e., the adequacy of the problemization of the difficulty" (*8*).

and greater understanding is apparent on all sides. In his history of science, Dampier-Whetham has noted that "insight, imagination, and perhaps genius, are required firstly to pick out the best fundamental concepts" (*2,* 457).

Search versus research. Much that now passes for scientific research, not only in psychology but in many fields, has precious little to do with what may be honestly called scientific pursuit. But the surface similarity between much current work and real scientific investigation may be sufficient to deceive the investigator himself. If investigators are not to hoodwink themselves and each other and pervert scientific inquiry for some end that has little if anything to do with increasing our understanding of man, it is clearly imperative that they be concerned as consciously as possible with research that will bring about major reformulations. Otherwise they are forced to close their eyes to important problems that face them or to devote themselves only to methodological problems, rationalizing these activities as research.

One variety of this perversion is represented in the shotgun approach, in which the idea seems to be that if one only gathers enough data, possibly with the use of new gadgets or apparatus, one must sooner or later come out with some sort of scientific result. A precedent for this type of activity was set by Francis Bacon who held that "by recording and tabulating all possible observations and experiments, the relations would emerge almost automatically" (*3,* 58). And in the three hundred years since Bacon's time, many investigators have proceeded either without any clear hypothesis or with what they call "limited hypotheses," often so limited that they cannot possibly provide a springboard for further emergence. Much of such data today is concerned with correlational relationships. The situation is such that to an outside observer reviewing the history of modern thought, psychology seems to be merely "trying correlation after correlation in the hope of stumbling on something significant" (*11,* 495).

Another perversion of scientific method is found in the tendency in some areas of psychology to work out elaborate classifications, with the implication that if the behavior of an individual can only be properly pigeonholed in some static system, then further analysis of a functional nature is relatively unimportant for understanding. Karl Pearson's emphasis on classification as a major pursuit of science undoubtedly did a great deal to establish this misconception. One needs only to review the literature in the field of personality or to watch many clinicians diagnose psychiatric patients to see how some men in these areas are struggling to free themselves from older classificatory systems.

Scientific inquiry and scientific method are also not to be confused with investigations limited solely to a so-called "quantitative approach." An overconcentration on problems of measurement as such can easily sidetrack the investigator from the more important concentration on what data are significant to gather and can blind him completely to the problem of problemization, with its concurrent problem of selecting the standards worth measuring. Furthermore, those who are wedded solely to a quantitative approach are all too frequently unwilling to tackle problems for which there are no available quantitative techniques, thus limiting themselves to research impressive only in the elaborate quantitative treatment of data. Current attempts to refine sampling techniques in the field of public opinion research, for example, while indispensable, run the danger of making investigators myopic to certain areas of inquiry that would seem much more important for an understanding or prediction of public behavior—for example, the problem of asking the right questions, of determining the surety of opinion under different circumstances, or the effect of different interviewing situations on response. The current vogue of factor analysis in the study of personality, while most significant as a means of testing a theory as Eysenck's report shows (*6*), frequently reflects insufficient consideration of the relevance or adequacy of the variables thrown into the hopper for analysis.

The function of experimentation and measurement. In saying that what now passes for research (scientific inquiry instrumented by scientific method) is often only scientific pretension, we do not mean to imply at all that reasonable problems for scientific research can be formulated or operated on without including empirical investigation. Experimentation is clearly indispensable as a test of formulation. An hypothesis can be tested only if one is able to do something with it. But it is often forgotten that the value of an experiment is directly proportional to the degree to which it aids the investigator in formulating better problems. And while a single experiment may solve a problem, it can never give us complete understanding. If an investigator believes that by solving a problem he has achieved complete understanding it only shows that his problem has been defined inadequately and is not a step in the constant, never ending scientific search for more and more comprehensive formulations.

The importance of any scientific experiment in which relevant variables are manipulated must be in terms of the breadth of the formulation it has a bearing on. It should be borne in mind that the first and most significant step in experimentation is to determine *if* the variation of one abstracted phenomenon affects other abstracted phenomena at all. The next most disclosing step is to determine *how* the variation of one abstracted phenomenon affects other abstracted phenomena. We confirm or deny the validity of an hypothesis by determining if and how the manipulation of one variable affects another variable or the total group of phenomena in which we are interested. In the process of using the scientific method of relevant variables, the investigator can discover if and how variables are affected only with reference to some inclusive, higher-order formulation. Otherwise relevant variables could be manipulated forever without making any scientific advance at all. It is also imperative to bear in mind that how much a change in one variable affects another variable does not give us new insight on the "if" and "how" relationship. We determine how much one variable affects another in order to increase our prediction and control, not to increase our range of understanding.

In the process of experimentation, the investigator must be ready to use whatever procedures appear most relevant to an understanding of the problem at hand. These procedures will be both quantitative and nonquantitative. Obviously if we select some phenomenon or characteristic as a variable for experimentation we can do so only because it exists in some degree, some amount, some quantity in relation to the abstracted standard upon which it is based. In scientific research quantitative and nonquantitative procedures are interdependent, and highly refined quantitative investigation may be necessary before one can establish a nonquantitative formulation as, for example, the relationship between the Michelson-Morley experiment and Einstein's formulation. Thus the establishment of any dichotomy between quantitative and nonquantitative procedure is an artificial barrier to scientific progress, separating and taking apart what really belong together in scientific method. Scientific inquiry will be strapped if the investigator feels that he cannot be scientific without being one hundred percent quantitative.

Because scientific methodology is now so often equated solely with quantitative procedures, it may be useful here to distinguish what seem to us to be the function of quantitative procedures in scientific method.

First is the design of controlled experiments or other systematic investigations which involve measurement for the specific purpose of checking a hunch, validating an hypothesis, or testing a general law in a specific concrete situation. As we have already emphasized, the verification of this hypothesis is itself to be regarded only as a stepping stone to further, more inclusive hypotheses. In the fields of psychology and the social sciences, this general function usually translates itself into the purpose of checking some experienced relationships and causalities in an effort to intellectualize and systematize hunches that seem significant.

A second role played by quantitative measurement is the systematic recording of data. But it must be emphasized again in this connection that the accumulation of quantitative results is profitable only to the extent that some previous intellectual excursions have led to an hypothesis which is subjectively held with some degree of surety. Recording without an hypothesis in mind, if it is indeed possible at all, has no place in scientific method.[2]

A third function of quantitative research is to establish norms for the purpose of studying single cases—in psychology, for example, individual or group variations. As any experimental, clinical, or social psychologist knows, quantitative standards are of the utmost importance in predicting how specific individuals or groups of individuals will react in specific situations. But again it must be borne in mind that one undertakes measurement for such purposes only after the formulation of some hunch which may itself be based on nonquantitative evidence. And we must furthermore remember that we can only measure something relative to an arbitrarily established norm.

Whereas most investigators would undoubtedly give a nod of approval to the thesis that quantitative and nonquantitative procedures are interdependent in scientific method, much current work in psychology and the social sciences indicates that in practice this kind of thinking and research-planning is not followed, and that, on the contrary, there is often a conscious or unconscious attempt to imitate the physical

[2] Occasionally Charles Darwin's work has been used as an illustration of the way in which an hypothesis suddenly appears if one can only accumulate sufficient data. But in the famous first paragraph of his introduction to the *Origin of species* (1859), Darwin clearly belies any such contention.

> When on board H.M.S. "Beagle" as naturalist, I was much struck with certain facts in the distribution of the organic beings inhabiting South America, and in the geological relations of the present to the past inhabitants of that continent. These facts, as will be seen in the latter chapters of this volume, seemed to throw some light on the origin of species —that mystery of mysteries, as it has been called by one of our greatest philosophers. On my return home, it occurred to me, in 1837, that something might perhaps be made out of this question by patiently accumulating and reflecting on all sorts of facts which could possibly have any bearing on it. After five years' work I allowed myself to speculate on the subject, and drew up some short notes; these I enlarged in 1844 into a sketch of the conclusions, which then seemed to me probable: from that period to the present day I have steadily pursued the same object. I hope that I may be excused for entering on these personal details, as I give them to show that I have not been hasty in coming to a decision.

And we find in Darwin's letter this further statement (*4*, 183):

> In October 1838, that is, fifteen months after I had begun my systematic enquiry, I happened to read for amusement Malthus on population, and, being well prepared to appreciate the struggle for existence which everywhere goes on from long-continued observation of the habits of animals and plants, it at once struck me that under these circumstances favorable variations would tend to be preserved, and unfavorable ones to be destroyed. The result of this would be the formation of new species. Here then I had at last got a theory by which to work.

scientists, in the false belief that their success has been due chiefly to the quantitative techniques they have designed. It may therefore be worth a brief historical glance at Isaac Newton's procedure to gain some perspective on the role of quantitative experimentation in verifying and extending nonquantitative observations.

Although Newtonian concepts have been superseded, the Newtonian method remains essentially unchanged and still provides the framework for most of modern science. While Newton's aim was to find absolute, mathematical "laws of nature," his method clearly consisted of (1) simplification and isolation of fundamental concepts, (2) formulation of relevant hypotheses on the basis of these essentially nonquantitative concepts, and (3) intensive quantitative verification and amplification of these hypotheses. Although the concepts of mass and the mutual attraction of gravitation are inherent in a falling apple, it is doubtful if they would ever emerge from a statistical study of *all* falling objects on the face of the globe. As Newton expressed it, "Our purpose is only . . . to apply what we discover in some simple cases, as principles, by which . . . we may estimate the effects thereof in more involved cases." The inverse of this, the attempt to find the "principles" in the welter of "involved cases," would have seemed senseless to Newton.

In developing his methodology, which he nowhere explicitly defines, Newton was in effect systematizing what had become over the centuries the *de facto* method of the "natural philosophers." Nineteen hundred years before Newton, there was sufficient evidence for Aristarchus to advance his heliocentric concept of the universe. This significant concept was, of course, lost until Copernicus, reading the ancients, discovered that some philosophers had "thought the earth was moved." "When for this reason, therefore, I had conceived its possibility, I myself also began to meditate upon the mobility of the earth." The immediate result of this fruitful hypothesis was, of course, a systematic theory which, however, still depended for its acceptance on the principle of mathematical simplicity. Galileo, sensing the importance of experimental verification, provided the last historic step by means of his telescope.

The Newtonian era probably represents one of the most significant and fruitful epochs in human thought. Relevant to our discussion here, the birth of scientific inquiry was accompanied by a formulation of concepts which have determined and dominated thinking up to the present day. Copernicus meditated "upon the mobility of the earth." Newton, age 23, "began to think of gravity extending to ye orb of the Moon." Kepler gave support to the Copernican system because "I have attested it as true in my deepest soul," and "I contemplate its beauty with incredible and ravishing delight." Harvey "began to think whether there might not be a motion [of the blood], as it were, in a circle." Huygens and others formulated the principle of the conservation of what later was termed kinetic energy. The list is virtually endless. In every area of human thought startling and productive contributions were made. Since there is no reason to suppose that the 17th century was especially propitious for the birth of genius, one wonders if the productivity of this period may not be attributed to a fortunate blending of unfettered speculation coupled with a new awareness of the need for empirical verification at every step. Remove the speculation and only barren measurement remains.

Operationism. In the past quarter-century the basic tenets of operationism have so interested all science and have become so ingrained in the thinking of most scientific investigators that no discussion of the role of experimentation in scientific inquiry can be complete without a consideration of the place of operationism which is, historically, a "recent formulation of some of the essential features of the experimental method and of empiricism generally" (*7*, 250).

The impetus for operationism came from Bridgman in physics with the recognition that concepts such as distance have different meanings when used in different contexts. The concept is, therefore, a construct of the observer and not "a thing in itself." It follows that if the variables with which an experimenter deals are products of the experimenter's ingenuity and cannot be specified by pointing to them, then they must be specified by pointing to the procedures employed by the experimenter in creating his constructs. It is only by pointing out the procedures employed in experimentation that the investigator can convey to others the constructs he is dealing with.

Unfortunately, however, the generality of Bridgman's approach has sometimes been lost sight of. There is nothing in the general statement of operationism which delimits or in any way prescribes the defining operation to be used. Bridgman himself has asserted that "any method of describing the conditions is permissible which leads to a characterization precise enough for the purpose in hand, making possible the recovery of the conditions to the necessary degree of approximation" (*1*, 246). Those writers who assert that defining operations must necessarily be "physicalistic" are gratuitously adding a restriction not inherent in the operational approach. This insistence probably trace back to the feeling that physicalistic constructs are somehow more "real" than others and has led to a fundamental misconception and perversion of the operational approach as originally stated.

The inhibiting effect of this artifical restriction has probably not been severe in those sciences more closely concerned with the physicalistic. In psychology, however, this has tended to exclude the use of psychological constructs and, as Pratt has stated, "to place a stamp of approval on certain limited fields of research in which hypotheses can be neatly formulated in the language of the older sciences" (*10*, 268). We have indicated that a study of relationships alone does not constitute scientific research. Real research must always involve constantly higher-order abstractions. In the field of psychology many of these abstractions cannot possibly be "pointed to" in any narrow operational sense and many of them are not easy to manipulate experimentally. While a scientific investigator must rely upon operational concepts, he must remember at the same time, as Feigl has said, that "operationism is not a system of philosophy. It is not a technique for the formation of concepts or theories. It will not by itself produce scientific results. These are brought about by the labor and ingenuity of the researchers" (*7*, 258).

Selection of standards. A major problem confronting any investigator is the selection or discovery of the standards to use in his investigation. The dictionary defines a standard as "that which is set up and established by authority as a rule for the measures of quantity, weight, extent, value, or quality."

The problem of selecting standards is much more complicated than is often realized, for the reason that the conditional relationships we abstract out of a total situation and except for which the situation would not exist do not themselves exist in their own right. Nor is there any adequate intellectual explanation of their existence. These conditional relationships or aspects of a total phenomenon that the scientist calls "variables" are not God-given and are not limited. Einstein and Infeld point out that "physical concepts are free creations of the human mind, and are not, however it may seem, uniquely determined by the external world" (*5*, 33). Any adjective or any adverb can serve as a potential basis for a variable. Variables that provide the bases for standards are purely the creations of man, enabling him to formulate an abstract, common, determined phenomenal world. The variables employed in any scientific research are based on intuitive judgments and in any concrete investigation depend upon the way in which the investigator has formulated his problem. Since problems are formulated differently in different fields of inquiry, the aspect of a phenomenon that we choose in one field to serve as the basis for a standard in that field will not necessarily be applicable in another field of inquiry. Furthermore, the aspect of a phenomenon that may serve as a basis for standards within any one field will vary according to the nature of the hitch in a concrete situation.

Here words play their familiar tricks even with the thinking of the scientist, who may tend to forget that in his necessary use of word symbols for his thinking and communication (*space, time, I.Q., attitude,* etc.) he is employing abstractions which he cannot, as a scientist, implicitly or unconsciously assume as real in investigation. And it is only to the extent that the investigator is aware of his own transformation of adjectival or adverbial relationships into noun qualities that he maintains the possibility of discovering new conditional relationships except for which a phenomenon would not exist. If abstracted characteristics of the situation are unconsciously reified, complacency or a defensive attitude results.

When we decide on a standard, we take some aspect of a phenomenon, some variable, as a basis for measurement. Since the phenomena with which science deals are so enormously varied, the quantitative units employed in any investigation will depend on the nature of the problem at hand—e.g., distance will be measured in angstrom units or in light years. Also, obviously, we cannot necessarily quantify one standard in the same way we do other standards. While precise units of measurement may be applicable in the physical sciences, in psychology, if we are using some aspect of experience as the basis for a standard, we may have to be satisfied with crude introspective measures such as "more than" or "less than." Whitehead has pointed out that "we must entirely separate psychological time, space, external perception, and bodily feeling from the scientific world of molecular interaction. This strange world of science dwells apart like the gods of Epicurus, except that it has the peculiar property of inducing our minds to play upon us the familiar antics of the senses" (*13*, 62).

Since every standard is based on a man-made assumption, and since it is possible for man to use an infinite number of abstracted subphenomena as bases for standards, the criterion for the selection of what shall be used as the basis for a standard is essentially its usefulness in determining whether or not the abstracted subphenomenon with which we are dealing is constant, verifiable, and potentially helpful in solving our original problem. Also, of course, the basis to be used for our standard must be subject to voluntary recall and to intellectual manipulation.

How do we proceed to select the standards we will use in actual empirical investigation? Since we must start with the nature of the particular hitch we have experienced, abstract generalizations or rules cannot be given. The best that can be done is to describe the apparent functional process that goes on.

It seems to be something like this: In the course of following an acquired interest in understanding why certain phenomena occur (in physics, biology, psychology, and other sciences) we encounter a difficulty which no previous investigator has resolved to our intellectual satisfaction or perhaps has faced as we face it now. The assumptive world we have built up from experience (which includes the abstracted scientific concepts that have a bearing on the problem) proves inadequate as we try to intellectualize the hitch we have run into. There is no empirical evidence we can find that describes all the conditions except for which the phenomenon that puzzles us would not exist.

In trying to intellectualize the inadequacy of our assumptive world, we discover that a certain condition or set of conditions have not been taken into account. We abstract out of the hitch-situation those aspects we believe are probably necessary to our understanding of the original hitch. We use these aspects of a phenomenon as the bases for our standards, and we vary their "amount." We may have an understanding of why such conditions are important at the time we think of them or we may only have a vague hunch that they are important and may intellectualize them much later. If we have an immediate understanding, we can design our investigation rather precisely. If we have only a hunch, a certain amount of trial and error in experimentation is necessary. But this trial and error takes place within boundaries we set and is not to be mistaken for a shotgun approach. In either case, we design the empirical test of our new basis for a standard with reference to other phenomena that have already been established as bases for standards. We do this in an attempt to determine whether or not the variation in the new basis we have selected for a standard affects old standards and is affected by them according to our formulation.

Our formulation may be validated in some circumstances if the new aspect of the phenomenon we have introduced is affected by other functional aspects. Or, under other circumstances, our formulation may be confirmed if the new aspect we have introduced is not affected by other standards. If our empirical test confirms our formulation and we find that we have abstracted out an aspect of the phenomenon that is the necessary condition for the existence of the total phenomenon, then we can say that we have the basis for a new standard and can proceed to think of it quantitatively.

Once an investigator has discovered new aspects of a phenonmenon that can serve as the bases for standards, it is only too easy for him to slip into the misconception that the particular operation on which he has settled as suitable to the problem at hand exhausts the subject and says all there is to be said about it. This leads to the reification of the very construct which operationism, for example, was devised to avoid. Any science becomes stagnant if it does not regard the discovery of new variables as its primary concern.

We cannot agree with those investigators who believe that the basic variables of all sciences are the same if we can only find them. As we have already pointed out, in psychology this leads to an artificial restriction of the problems dealt with, sometimes to the extent of eliminating from consideration the most pertinent variables. For example, in the attempt to study certain perceptual phenomena, emphasis has been placed on such easily defined variables as "farther than" and "bigger than," where the more psychologically meaningful variable in many cases is probably the subjective feeling of "surer than." If our awareness of a change in an external event is to be considered at all functional in nature, then the subjective sense of surety accompanying the perception must be of primary psychological significance. In the case of those perceptions we label attitudes, investigation of the surety with which attitudes are held under different conditions has lagged far behind our interest in measuring the "direction" of the attitude or opinion.

A note on analysis. The use of the term *analysis* is a poor and misleading way by which to describe the processes involved in determining the variables we will use in our scientific thinking. For analysis assumes the existence of entities existing in their own right which together make up a total phenomenon, and suggests that all we have to do is somehow isolate them, by analysis, for manipulation. Analysis becomes synonymous with the classification of variables in terms of abstracted, fixed, and reified standards.

As we have already indicated, there is an infinity of variables that provide the bases for an infinity of standards. We have said that all adjectives and adverbs furnish a potential basis for standards. And from a study of the history of our language we know that emerging situations bring their own new bases for standards—e.g., the "snafu" of the G. I. When we analyze by using existing standards we make nouns out of adjectival or adverbial relationships often without knowing. For analysis is possible only by using existing standards. Analysis thus does not add anything to our understanding of the functional activities involved in transactional relationships. Hence analysis is not at all similar to what must be regarded as the scientist's constant obligation to discover those aspects of a phenomenon except for which it would not exist. Likewise synthesis—the putting together of that which we have taken apart—is a process by means of which we cannot get any more into the synthesis than is included in the standards made use of in analysis.

The functional activities we pick out for attempted intellectual understanding are those related to the immediate hitch we face. This means, then, that although an infinite number of conditional relationships exist, in any concrete scientific pursuit the range of conditional relationships an investigator might pick out as important will be limited, and will be bounded by the nature of the hitch he has encountered. Scientific progress results from the ability to pick out the most relevant conditional relationships for empirical investigation, not by further analysis of established variables alone.

(*This is the second of a series of three articles.*)

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Frederick Gardner Cottrell: 1877–1948

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FREDERICK GARDNER COTTRELL, who died at the age of 71, was among those fortunate pioneers who live to see many of their dreams for the betterment of mankind come true. As a scientist, engineer, and humanist, he loved to explore new fields of applied science, to uncover new ideas which his associates might explore and test in detail, to encourage experiments, and to foster projects of promising industrial and social value.

Specifically, he made several important contributions which will be long remembered by scientists and engineers. Among them is the Cottrell electrostatic precipitator, known simply as a "Cottrell," for the precipitation of dust and mist. Also of note are his boiling-point apparatus, the Research Corporation which he established, and the chemical applications of pebble-bed furnaces.

He was a vigorous, unselfish, imaginative physical chemist and industrial engineer who acted something the part of a catalyst in bringing together inventors, engineers, scientists, and industrialists to develop new processes. In these repeated endeavors, Dr. Cottrell had no thought of personal gain or prestige, but was intent upon seeing the wheels of progress turn. He lived modestly, indeed frugally, and cared little about his own comfort. Clothes or traveling accommodations were less important to him than a visit with a productive scientist, or a good book. He read widely and was an indefatigable conversationalist. His abiding interest in people revealed itself in his spoken thoughts. His approach to a group of related ideas or to a new industrial application of science emphasized individual scientists even more than the work they had done. One idea led rapidly to another and in turn to still others, usually by association with names and faces that came to his mind. It was this intense interest in men, coupled with a keen knowledge of the facts and implications of his field of work, that enabled him to accomplish so much.

The broad scope of his interests brought him into early contact with the problem of financing research and development work. With income from patents on the Cottrell precipitator he set up the Research