SCIENTIFIC METHOD

THE MACMILLAN COMPANY
NEW YORK · BOSTON · CHICAGO · DALLAS
ATLANTA · SAN FRANCISCO

MACMILLAN & CO., LIMITED
LONDON · BOMBAY · CALCUTTA
MELBOURNE

THE MACMILLAN COMPANY
OF CANADA, LIMITED
TORONTO

SCIENTIFIC METHOD

ITS FUNCTION IN RESEARCH AND IN EDUCATION

BY

TRUMAN LEE KELLEY

PROFESSOR OF EDUCATION, GRADUATE SCHOOL OF EDUCATION, HARVARD UNIVERSITY

New York

THE MACMILLAN COMPANY

1932

Set up and electrotyped
Published September, 1932.

SET UP, ELECTROTYPED, AND PRINTED BY T. MOREY & SON
IN THE UNITED STATES OF AMERICA

"For he that reads but Mathematicke rules
Shall find conclusions that availe to work
Wonders that passe the common sense of men."

Frier Bacon and Frier Bongay, Act I, Scene I.
—Robert Green, 1594.

PREFACE

The present volume is a collection of papers and lectures upon the general theme of research methods with particular attention to that field lying between the factual certainties of the physical sciences and the deductive certainties of logic and pure mathematics. Though many aspects of the biological and social sciences are discussed, the treatment cannot be said to be systematic. The reader is to anticipate such independence between chapters as is in harmony with their origin as revisions of lectures delivered upon sundry occasions. One compensating feature of this considerable independence between chapters is that the reader need not read consecutively, but may quite readily pick up the theme at any chapter. Chapters I, II, III, VIII, and IX are in the main drawn from a course of lectures delivered at the Ohio State University under the auspices of the Graduate School and the Department of Psychology, 1928; Chapter IV is drawn from an address before the American Historical Association, 1929; Chapter V is drawn from the retiring address of the Chairman of Section Q of the A.A.A.S., 1929; and Chapter VI is drawn from the negative of a debate between the author and Dr. W. H. Kilpatrick upon the issue "Resolved, that for some of the vital problems of education philosophy not science is and must remain a guide to the solution," 1931.

I am indebted to my former colleagues at Stanford University for their kindness in answering the questionnaire discussed in the second chapter. I have drawn freely from class reports of students for bibliographical data about men of science, and have especially profited by the reports of Mr.

C. G. Shambaugh upon Charles Darwin and Francis Bacon. I am indebted to Dr. Lucia B. Mirrielees for suggesting the quotation which is introductory to this volume. I have been helped financially in the experimental work reported by two grants from the Stanford University Council of Research in the Social Sciences. I am indebted to the following who have kindly consented to my use of material published by them, Ohio State University Press, Editor of *Science*, Editor of *Harvard Teachers Record*, and Editor of the *Historical Outlook*.

T. L. K.

August, 1932

Contents

CHAPTER		PAGE
I.	Interrelationship between Method of Research and Field of Investigation . .	1
II.	The Rôle of Judgment in "Objective Measurement"	30
III.	What Purposes Are Served by Scales of Mental Measurement?	75
IV.	The Objective Measurement of the Outcomes of the Social Studies . . .	98
V.	The Scientific versus the Philosophic Approach to the Novel Problem . . .	119
VI.	A Defense of Science in Education . .	139
VII.	The Conference Method of Finding the Truth	152
VIII.	The Bearing of Recent Scientific Development upon Problems of Education and Inheritance	165
IX.	Mental Traits of Men of Science . .	192
	Index	231

SCIENTIFIC METHOD

CHAPTER I

INTERRELATIONSHIP BETWEEN METHOD OF RESEARCH AND FIELD OF INVESTIGATION

IT is only within modern times that research activity has attained a social importance which is not merely incidental to the older pursuits, religion, philosophy, education, politics, war, and commerce. Its present dignified position can be traced to the recent epochal advance in the physical and biological sciences. It is clearly a phenomenon of growth. If physics should ever reach such a stage that all a student's time would, in the judgment of teachers, need to be consumed in catching up to what the past had bequeathed, then physical research and experimentation in its truest meaning would lanquish. The essence of research is evolution.

To the sophisticated parent the growth of a child may seem to have many prosaic and predictable elements, but to the child growth consists in the flowering of new aspirations and the fruiting of new powers. Our civilization is like the child in its vitality and outlook, and not like the complacent retrospective parent, and the process of research is itself the bursting new tissue of the social body. Presumably most of our social life must be devoted to the maintenance of the *status quo;* but there is another, a growing part, and no matter what the magnitude of the major portion, if the minor portion is not allowed to flourish, to

appease its gluttonous appetite to repair and improve the social structure, the entire body is sick, is in truth feeling the lingering, or perhaps the galloping, pains of mortal illness. What constitutes the proper balance between these two portions? We have no unit in which to compare the portion of social activity devoted to maintenance and the portion devoted to growth; but axiomatically the greater the latter, the greater the expected growth. Taking money as a sort of unit, should we for every thousand dollars spent, for example, in electrical construction, spend one hundred dollars, ten dollars, or one dollar, in electrical research? Though we cannot answer this question, we may note some matters connected with the problem.

It unfortunately seems to be true that a law of diminishing return holds for scientific research. A small amount of time and effort will at one time yield an enormous result, while in a second instance time and effort a hundred, yes, a thousand times greater, will yield but a fraction of the earlier return. Surely in terms of time spent the geometry of Euclid, the celestial mechanics of Galileo, the laws of heredity of Mendel were not stupendous. What philanthropic foundation would not give its entire resources if it could thereby assure a modern discovery as important as one of these? Such, however, cannot be assured. The insurance company has not yet been established that guarantees valuable outcomes to research. It probably never will be; but society in the aggregate may well take the risk and underwrite research here, there, and in many places, confident that in the long run the return will be munificent. It does not seem that any quantitative rule for the division of funds between maintenance and research endeavors can be adopted, but with the brilliant last century behind us any procedure niggardly to research would be foolish. Organized and professional research can be overdone, and moneys devoted to it poorly

spent. This is to be expected, for in its very nature it is difficult to establish a mill for its accomplishment. Probably no subsidy, however great, could stimulate even Euclid, Galileo, or Mendel, to make second contributions of equal moment with their first. Does it seem sordid to think of these great steps in human evolution in terms of money? If your answer is yes, I would ask you to reflect that within a decade epochal discoveries in chemistry, medicine, and other fields have very literally been bought and paid for. Uncertain though it be, real research can be subsidized, and we, the gainers, should be thankful that it is so. Though we cannot say that subsidized research is uneconomical because no other less wasteful means of accomplishing the same result exists as a standard, we can say that much, I will even say most, of subsidized research is a chasing down blind alleys. What can we expect when we give a man, a good man, food and raiment and a slide rule or other instrument, and instruct him to fructify as man has ne'er done before? If the product is still-born, we are not unfortunate; if a monstrosity, it may take much social agonizing before it is given its lethal dose. (Bewildered Russia is today in the throes of cutting, cauterizing, and patching a monstrosity with the hope of saving its life.) And if, as is rarely the case, the product is a fine healthy mutation which can endure, we are indeed blessed. Research that is worthy the name is the most difficult task that society has differentiated out from the total field of human activity and called upon certain of its members to perform. To die in war, to dedicate one's life to a religious cause, demand great human virtues; but they are known virtues, whereas to be original in a worthwhile manner is a mystery. It is as easy and as predictable as that one should search for and find the Holy Grail.

I have tried thus far to convey two ideas: first, that the odds that called-for research will be fruitful are greatly against

us; and second, that the odds are well worth taking, for when it is successful the winnings are great. In the not distant past the great discoveries were outcomes of individual projects. Today heavily financed, well-organized group investigations are being undertaken in many fields. The consequences of this change upon individual endeavor are by no means altogether salutary. A very real sense of incapacity must come to the young unaided Ph.D. candidate when attempting to break into the field with an "original contribution to knowledge." Let us say that he is a Latin student investigating "objectives in the study of Latin." His work, the product of a little money skimped from his meager earnings and of many weary hours spent by himself and his self-sacrificing wife, takes its place in competition with the "Classical Investigation" which is the product of an organized effort having available hundreds if not in fact thousands of dollars where he had one. This situation is truly discouraging to the individual student. To him a glimmer of light exists when the organized effort is wasteful and more or less futile.

We must not, however, look forward to brightening the outlook of the student in this manner. Probably few will deny that in terms of expense and effort the real significance of the product of individuals engaged in organized research has been much less than that of the same individuals working alone or with but little financial aid. There are probably reasons connected with group inertia and the leveling influence of an average why this is characteristic.

Though this issue is worthy of serious and many-sided investigation by agencies sponsoring research, I wish now to consider it only from the standpoint of the graduate student attempting to make a contribution to some field of knowledge. Research foundations do not generally, at the present time, give an opportunity for the young gradu-

ate student to develop and test his powers. In brief, they are not educational institutions, but factories whose "hands" are journeymen in the matter of investigation, not apprentices. The good student apprentice, unwilling to work more than sixteen hours a day and unable to provide himself with the tools of the master craftsman, even if able to use them, is hard put to it to produce a creditable masterpiece entitling him to enter into the select guild of journeymen. Though from the student's point of view the situation is already bad and is probably growing worse, at least for the present organized research is encamped upon relatively little of the vast field appropriate to investigation. If the student is very hardy he may brave these professional researchers upon their own ground; but if, because of financial reasons, timorousness, or independence, he is not of this mood, he may search some congenial spot not yet staked out.

Though this sounds rather simple, the actual discovery of this promising spot requires the highest order of ability, so that the student is called upon to exercise at the initial step rather more, not less, research ability, than the established professional worker. That he not uncommonly makes a failure of it is unfortunate rather than unexpected. It may be for the best that the difficulties are great, for society is under no obligation to grant doctor's degrees to all who come, and the very rigor of the course which must be followed may be the best means of selecting and training future research workers. This is not the only conceivable means. The incorporation of instructional departments for graduate students in connection with research foundations, thereby making available to the students the equipment and the problems of the foundation, might be a fair and effective means of training and selection. Though this is in a sense done with reference to post-Ph.D. workers, it might well be considered in connection with ordinary postgraduate work.

Believing, for reasons which I will expand upon later, that the highest talent is required in selecting a field and a method of research, I wish now to consider the various fields of human activity and what constitutes investigations in each.

There have been various classifications of knowledge, from Plato and Aristotle to Jeremy Bentham, Auguste Comte, Andre Marie Ampere, Herbert Spencer, and Melvil Dewey, the author of the much reviled Dewey Decimal Library Classification System. Though these systematists of human knowledge do not agree with each other, one finds many recurring lines of cleavage in their systems. In spite of the illustrious endeavors of the past, I have the hardihood to present a classification of my own. If it has merit, it is because it throws into relief certain radical differences in technique of research. I, however, do not claim that the lines of cleavage which I emphasize are original with me. It is probably the fact that each has been noted by early systematists.

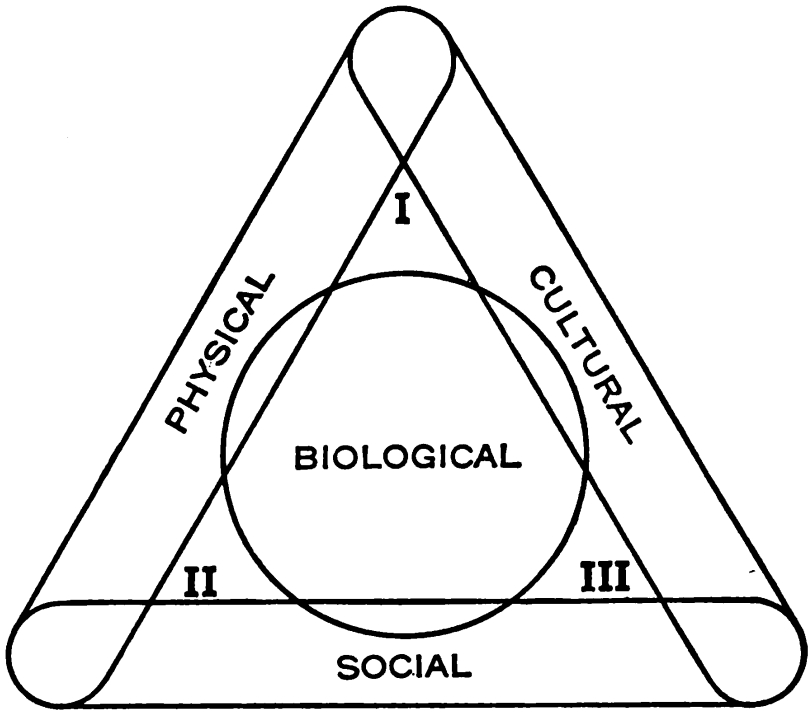
If we start with a cross-section of time represented by the present, we can immediately note a variety of different types of human activity. We may start with any one type and relate the others to it. Let me then ask you to think of the biological sciences as the starting point. Botany, zoology, physiology, anatomy, and medicine all deal with living organisms and phenomena of growth and decay. Quite distinct from this field is that of the physical sciences. Physics, chemistry, and astronomy deal with a larger unit that is growing or changing—the universe, and a smaller unit that is invariable—the element, whether the molecule, the atom, the proton, or what not. These two fields are neighboring. The chemist at times looks upon his atom as being built up or torn down, but, generally speaking, the atom is conceived of as an invariant, not at all showing the phenomena of growth of an amoeba or a man.

Quite distinct from these two is the field of the social sciences. Economics, political economy, and law are concerned with human achievements, with the state in its entirety and in its relationship to its individual members. To think of the state as a stable organization requires thinking in terms of invariants, as is so largely done in the physical sciences; and to think of it as growing and as composed of changing and growing members requires the concepts of biology. It is quite unique, however, in its concern with human relationships.

In addition to biological, physical, and social sciences, we must provide for another field of human life—culture. This is concerned with individual values, and is particularly expressed in art, music, literature, and religion. Of course it touches each of the other fields. Music cannot be unrelated to physics, literature and art to growth, nor religion to the social sciences and law; but the field of culture is unique in that its ends are those of the individual. The veriest splotch of color on a canvas has served its cultural purpose if it creates a feeling in an individual quite irrespective of any biological, physical, or social value inherent in it.

The accompanying chart indicates the relationships I have just mentioned. We may show by overlapping each section with each other that there are certain borderline activities which may be classified in either one of two ways. We may note that such human activities as mathematics, engineering, and business do not belong to a single one of the four fields indicated. They will, however, quite readily fit into the scheme. Let us characterize the spaces lettered I, II, and III service departments. Then space III is "The Adaptation of Human Talents to an End." Psychology and commerce lie here. They are the adaptation of one's growing sensory motor equipment, drives, and capacities to social and cultural ends. Space II is "The Adaptation of Mental

Concepts to an End." It is the field of applied logic, applied mathematics, and statistics. Space I is "The Adaptation of



Physical Materials to an End." Herein is technology and engineering.

Even with these additions, our picture is not complete. History has been left out. To incorporate it, it is necessary to add another dimension. Think, if you will, of the chart entire as representing the present time only. Think of the triangle as being the base of a tetrahedron whose apex is in a dimension at right angles to the plane of the chart. It is the remote past. Back of biological science is a part of this tetrahedron which is its history. Back of the social sciences, back of the physical sciences, and back of each of the other regions, is the corresponding history. I would ask you to conceive at this point that the history of any one of these fields represented by the present-time cross section lies in the main, but not exclusively, back of the section in

question. For example, astronomy in the physical sciences and religion in the cultural field are in the present-time chart substantially discrete, but in the past they were more closely related than at present. In the history of each there is much in common. Thus the tetrahedon back of the present-time surface must be thought of as an aggregate of strands and not as preserving inviolate the same lines of cleavage as those of the present time.

We have added history as a new dimension. Paralleling this difference in dimension is a difference in research technique. The historical method is not one with the experimental technique which is that of present-time investigations. It has seemed that some historians, with a desire to establish the excellence of their methods, have maintained that they engage in the scientific method just as does the physicist or the biologist. They may engage in it, but certainly not as does the experimentalist. It is more confusing than illuminating to attempt to describe a sound historical method and a sound method in physics as one and the same. The experimental technique is one in which the initial conditions, subsequent steps, and the final outcomes are all within the observation of the experimenter. Because of the completeness of control it is the technique *par excellence*, wherever possible. If any method is to be called "the" scientific method, it would seem that it should be. Historical research is more difficult and its conclusions less trustworthy. Even it, however, has a firmer foundation than does the technique investigating future conditions.

We must add to our tetrahedron a dimension opposite to that called the past, changing the figure into a hexahedron, the largest cross section of which represents the present, the longer tetrahedral portion the past, and the shorter the future. Thus, if we cut the solid with a plane representing the present time, the exposed face is the chart here shown.

The apex of the tetrahedron which lies back of this plane represents that bit of historical evidence that deals with the most remote time. This chart is also the base of another tetrahedron which lies in front of the plane. It is the future, and the most distant future item reliably forecast is its apex. Though the detail of this picture may be greatly elaborated, for example, by greatly shortening the portions of the past and future tetrahedrons dealing with economics and lengthening those dealing with astronomy, etc., still the general picture will maintain, and we shall be left with three great divisions of human activity—that concerned with the history of a subject, that concerned with the relationships completely amenable to present observation, and that concerned with forecasting or estimation of future events.

These three divisions of human interest deal with the phenomenal world. Reality is the standard whereby each is judged. There is a field of human thought quite distinct from this. It is that of logic, pure mathematics, and a certain type of philosophy. It is a field wherein a certain set of conditions is postulated, and then the necessary consequences searched out. For example: If God is good, eternal, omnipotent, and omniscient, and if Adam fell, then—well, *what* then?—I will leave it to the dialecticians of the Middle Ages, who were facile in the mental gymnastics of logical deduction. Or again: If through a point exterior to a line one and only one line parallel to it can be drawn, and if certain other commonly accepted postulates hold, then the consequence is Euclid's geometry and immeasurably more Euclidian geometry than has yet been surmised. Both of these problems belong to the noumenal world. The rules of dialectic govern straight thinking in each, and the excellence of the process is to be judged by the rigor with which these rules of thought have been followed. This, then, is a fourth field of human investigation. It both permeates

and lies outside of the hexahedron pictured. It will not be possible to investigate each of these four fields in detail, but I hope to show that each has its own peculiar hazards, methods, and standards. Every subject has a history to be investigated by the historical method. Every still-existing subject shows relationships and is undergoing changes under our very eyes, and these are to be investigated by the experimental method. Every existing subject may be expected to have an influence upon the future, and this is to be studied by the much less tangible rules governing prognostication and probability. Finally, both independent of these three activities and in connection with each, hypotheses arise and conclusions are drawn to postulated premises, and when this is done the rules of logic should apply.

Instead of a single scientific method there are no less than four methods of research. The most pervasive is that of dialectic or logic. It finds its finest expression in human life in the wonderful pronouncements of pure mathematics. Its greatest hazard lies in the *non sequitur;* and other difficulties lie in the choice of non-independent or otherwise poor premises. The historian may pause in his study of a certain character and say, "If this character was a Jesuit, would he have done this thing?" The experimentalist may pause and ask, "If temperature influenced the reaction, would it account for this discrepancy?" And the prognosticator may inquire, "If condition A, assumed to hold in the future, does not hold, but is modified in a particular manner, then what is the consequence?" We see that the process of logical deduction is a common attendant of each of the other types of investigation. It is an indispensable adjunct guiding, as it were, mental exploration in the process of argument. In short, poor logic can vitiate any method—but good logic alone is insufficient for any except the dialectic method.

Second in definiteness and rigor is the experimental method; third, the historical; and last, the method of future estimation.

A person may become so interested in the history of a subject that it attains value to him on its own account, but more generally the value of history lies in its implications for the present and the future. One may become so interested in a particular experiment that it attains value on its own account, but more generally the value of it also lies in its implications for the future. From this point of view both historical and experimental investigations are initial steps in the investigation of future events. Surely there is no more fascinating investigation than that of the future, providing it is fruitful. But no intrinsic fascination of a subject can compensate for continued failure in investigation and make it worth while to pursue the subject. The question of individual survival of bodily death is perhaps *per se* the most interesting of all questions. Should we all therefore investigate it? Certainly not, for the promise of a fruitful investigation is very nearly nil. One hundred per cent of importance, times a magnitude either zero or in its neighborhood for probability of successful outcome, yields a negligibly small product. All future events hold the promise of weal or woe to individuals expecting to live on into the future, but it is the part of wisdom as well as of self-satisfaction to attempt to estimate only such as it is fairly reasonable to think can be measured. This is so restricting a condition that the estimation of the future represents but a small proportion of scientific endeavor. We may expect it to represent an ever-increasing proportion as the means of estimation are improved and the data pre-requisite to it are extended. I shall return to this question in connection with an investigation in the field of social science, but now I wish to consider the historical and experimental types of research.

To say that one is engaged in historical research does not define his activity, because the historian always comes to his task, or at least always should come, with a motive. The words of Darwin as to what a scientist should observe are equally applicable to what a historian should note. Darwin said: [1]

> About 30 years ago there was much talk that geologists ought only to observe and not theorize, and I well remember some one saying that at this rate a man might as well go into a gravel pit and count the pebbles and describe the colors. How odd it is that anyone should not see that all observation must be for or against some view if it is to be of any service.

Let us say that the historian's present purpose is to write a history of art ten thousand years ago. The most original documents that he finds, the stones of that age that he overturns, tell him perchance of war, of architecture, of commerce, of culinary habits, etc., but not, directly, a word of art, except possibly with one exception. He finds one little splotch of black which might be interpreted as a face on a small portion of earthenware which might have been a vase, in the ruins of a building which might have been a temple. He makes a series of deductions from these bits of evidence which are the most reasonable which human mind can make. If we are to believe anything as to the art of the period, we should believe what he states. We cannot prove him wrong by showing that some other art situation is more reasonable. Under these conditions, is he a good historian, and should we believe him? We see that we are confronted with the same issue as in the estimation of future events. A probability should be attached to the past event as described. If the historian writes, "The people of that time decorated the vases used in their temples with crude human figures done

[1] Darwin, Francis, *More Letters of Charles Darwin*, 1903.

in black," we might express the probability of this being a true statement somewhat as follows:

The probability, let us say, that the black splotch was made by human hands is 80 in 100.

The probability that the black splotch was part of a human figure is 65 in 100.

The probability that the crudity of the black splotch was due to the originator of it and not to abrasion since is 75 in 100.

The probability that the original pigment was black and has not been changed by time is 65 in 100.

The probability that the fragment found was part of a vase is 60 in 100.

The probability that the fragment found was part of an article used in the building whose ruins were excavated is 55 in 100.

The probability that the ruins excavated were those of a temple is 65 in 100.

The probability that the earthenware and the ruins belong to the period under discussion is 80 in 100.

Taking all of these things into consideration, the net probability that the statement is true, which is the product of these various probabilities, is but 4 in 100, or but one chance in 25. Clearly we should not believe the statement, though we cannot formulate a substitute statement covering all of these facts which is more reasonable. In the illustration, the probabilities for each of the facts are listed. In actuality these would not be known, except in the most general way. The example given points out the unreliability of statements of relationship and of historical sequence, which are both things that the historian very commonly does, and perhaps should do, for the separate distinct facts are often of little interest. Certain writers maintain that

it is the prime function of the historian to give the facts and not draw inferences. This, however, would seem to be an untenable position, for no writer can take the space to record all the possible facts. Just as pointed out by Darwin in the case of the scientific observer, he must select and give the pertinent ones only, but the very selection of these has involved a judgment, a drawing of an inference as to relevance, upon the part of the historian.

The historian must write with a motive; in one sense, this is to say, with a bias. There is a bias in his point of view, due to the fact that he lives in a certain time, in a certain place, and has been reared in a certain manner; and there is bias in his immediate interest. History should be written and read with a full knowledge of this. It is of great value for a twentieth-century chemist to interpret seventeenth-century alchemy as the beginnings of chemistry. We have profited by his bias, his twentieth-century understanding, and his realization that alchemy was, in one important sense, not religion or magic, but science. Certainly his account is not timeless; it is a twentieth-century account and not a seventeenth-century nor a twenty-fourth-century recital. History must be rewritten for every generation, and readers of history should get a value from what they read inherent in the channel through which it comes to them.

Pertinence and bias may be differentiated by the writer, but not always by the reader. Suppose a typical honest Democrat writes an account of the United States oil reserves, and in his endeavor not to be biased selects his facts, places his emphasis, and moderates his utterances so that readers in general cannot tell whether the writer is a Republican or a Democrat. The author believes that he has presented things in an unbiased manner, as also do Democratic and Republican readers—but probably not, say, a Socialist. The Socialist finds that pertinent facts have been omitted,

that a capitalist emphasis runs throughout, etc. In short, the writer and the article are biased from the Socialist-non-Socialist point of view, but quite unbiased from the Democrat-Republican point of view. All that we can accurately mean when we say that a presentation is unbiased is that with reference to certain issues it gives a fair and balanced picture as judged by informed, impersonal, and competent critics.

It is commonly recognized that there is an obligation upon the historian to give a picture as little biased as possible. There are certainly two pre-requisites to the accomplishment of this. First, the author must be possessed of a wide culture and a deep understanding of his prospective readers, so that he knows the issues with reference to which his treatment must be unbiased; and second, he must be dominated by the will to be unbiased. The great Leopold von Ranke possessed these traits to a high degree. Even the historian of a very limited field should maintain a point of perspective which is beyond that field. The chemist writing upon alchemy should appreciate the stimulus of religion, the mystery of magic, the gullibility of peoples, the hazards of witchcraft, etc., or he cannot write truly of the strictly chemical developments in alchemy. Who can understand and describe Galileo's denial that the earth rotates, who only understands physics?

A Moseley was a great physicist in his early twenties, a Raphael a great artist in his teens, and a Mozart a great musician before then; but who has had the maturity, the breadth of view, the even temper, to be a great historian at so early an age? One is not unbiased because he is ignorant of his bias. The least biased man is the one who knows what bias is, from a first, a second, a third, and still other points of view, and then determinedly leans neither to the left nor the right, backward nor yet forward. What may be called

leaning forward is the most insidious of all. As examples: Christianity is the goal of mankind, so note how the world moves toward the goal; democracy is good, so I will interpret everything in terms of a good democracy; the world is growing better, and I see improvement in all things; and so on. Of course, if the world does lean forward, these things should not be called bias; but certain it is that this leaning forward attitude is taken time and again with no apparent justification. It is well to be skeptical of it, for we know too little as to what is forward and what backward to place uncritical confidence in anyone's assertion that it lies straight ahead.

Most of what has been said applies to the historiographer, but much concerns likewise the historical investigator. The most important function that he exercises is that of selection of material. When a promising original document comes to hand, very great skill and judgment is called for in its internal and external criticism and evaluation, as has been so clearly pointed out by Bernheim.[1] Possibly the talents involved in this process are even of a higher order than those involved in the selection of material. When dealing with periods of history in which sources are few, little judgment is called for in selection of matter, for all or nearly all must be studied; but a study of the later periods, wherein sources are innumerable, presents a different situation. The first task and probably the one calling for the highest ability is that of selection of sources. The writer upon the history of traffic regulation in the United States has his greatest task in selecting his sources and in detecting original and imitative procedures. He must be a genuine detective. Starting with a condition found to exist in a certain year he must follow it back through the mass of antecedent recorded fact to an earlier source. The historian of this subject, or of any other, is not attempting

[1] Bernheim, E., *Lehrbuch der historischen Methode*, 1894, 1903, 1908.

to present a photograph of the past; rather, he is turning the spotlight upon one thread only, that weaves and divides and continues on and on into the past. Imagine a complex tapestry with a yellow cord appearing upon the right-hand margin. Taking a position at the right hand, and without moving, attempt to trace this back to the left. It is now a part of a larger design, and now a lone thread, now not visible at all, but buried beneath the reds and blues which dominate the field. It ceases to be a single strand, and is divided, not once but many times, and is finally altogether lost to the eye, though the separate strands may still be threading their way farther and farther to the left. The tracing of this cord is history, partaking not at all of photography with its even toning and its non-relevant detail. The tapestry falls short as a picture of time, because it ends and is completed at the right-hand margin. The yellow strand so carefully traced abruptly terminates at the right. If this strand were living and growing on and on into the right, then our picture would be complete. Our chief interest in tracing it would be in the light it would give us as to its future growth. In this important aspect the growth of coral provides us with a truer picture. Its past is unchangeable and very dead, just its upper surface living, but that upper surface and the future is built upon the dead past. If this were not so, what interest as living and planning individuals would a study of the dead have for us?

We may point a difference here between history and literature. In history, the deadness of time's puppets, the rigidity of their bequeathal, fairly stuns us with a sense of verity; whereas in literature the vitality, the genuineness and immediate reality of their passions, and the freedom in their powers of choice make them one with us—untrustworthily one with us. The historical novel and the novel-like history do not preserve the distinction here made, but

surely it is a distinction that cannot escape the historical investigator.

If we accept an understanding of the present and a forewarning of the future as the most important purposes of history, we can readily believe that we shall always be enriched by delving into the history of a subject. For this purpose the history of the immediate past is generally of more value than that of the remote past, but there are exceptions. If we thoroughly know the quarter-inch of coral below the growing surface, we know its present and prospective future as well as if we know the shape and structure of the entire coral column. Only when remote history aids in the understanding of less remote and that, in turn, in the understanding of still less remote, does it all contribute to the main purpose. History not written to give literary pleasure but to contribute to our understanding of the present and future should be judged by the extent to which each item in it supplements and develops subsequent items down to those of the living moment.

Nearly every experimental doctor's dissertation contains an historical introduction. It should be judged by the standard of relevancy, and when so judged it is commonly found that paragraph upon paragraph of trite and tiresome matter can be omitted. If the novitiate in historical research will bear in mind that he is aiming to increase the understanding of the future; that he is investigating one definite field; that he is tracing a thread, not making a photograph; that antecedent to original investigation along a line he should know all the important moot issues connected therewith; that he is morally obligated to present complete evidence or a fair sampling on both sides of these issues; that the probability of correct statement of related or sequential events is less than of the events singly; and that principles of temporal order, originality, independence, and creditabil-

ity of evidence exist [1] and are to be followed, he will be headed toward a scientific method in history.

I have no objection to calling the method outlined the scientific method in history, but I would agree with Pasquale Villari, W. S. Jevons, Henry Sidgwick, and other historians that it is not the scientific method of the physical or biological sciences.

Experimental research does not deal with dead and unique material. Napoleon is dead and Napoleon was unique. No similar man under similar conditions can be investigated again. Every experimental set-up made in the past can be made again; thus it is not unique. By repetition it can be made living if desired. Now I know that logically this statement is unsound, but practically it is defensible. A person observes the length of the meter bar in Washington, under standard conditions today. Tomorrow the gravitational systems of the universe are different. The bar has been changed by the earlier observation of it. The observer is older, and all of these things make an exact repetition of the first experiment impossible. Logically we can never secure two independent observations of the same thing. Logically we can never secure a population which is a homogeneous sample. Keynes has criticized statistical procedure because of this point. Why does he not go farther and criticize all of physical and biological science for the same reason? He should logically, as, from the standpoint of the philosopher or mathematician, whose concepts are exact, there is no possibility in the world we live in of exact experimental verification of anything. It is well to recognize that, in exact terms, no two things are samples of the same thing, and that no two experiments are the same. The recognition of this will lead the experimenter to make experiments as

[1] See, for example, Langlois, Charles V., and Seignebos, Charles, *Introduction to the Study of History*, 1898, 1912, 1925.

nearly the same as possible, and will lead him to investigate how well he has succeeded. In the physical sciences such investigation will quite universally be reassuring, in that there will be found to be a great agreement between conditions, as contrasted with the disagreement. In two experiments the gravitational field may vary by one part in a billion, the temperature by one part in one hundred thousand, the present accuracy of observation in a certain instrument by one part in a significant range of one hundred thousand. Physical science, based upon the logical premise of similarity in experimental conditions, need not greatly worry as to the practical identity of such investigations. In the biological sciences such high similarity in conditions is not obtainable. This colony of white rats varies from an earlier colony in certain small but unknown hereditary elements—in certain small but not exactly measured food, temperature, light, and exercise elements. But whereas in the physical experiment an error of one-tenth of one per cent would probably vitiate the investigation, in the biological study a 10 per cent error may not completely cloud the effect in question, so that again the logical weakness of the technique does not prevent its yielding an important practical outcome. In the social sciences the situation is less satisfactory. Under a first condition, increase in the price of beef was followed by an increase in the price of pork. Under a second seemingly similar condition, the price of beef increases. Will this again be followed by an increase in the price of pork? The conclusion is hazardous because of the difficulty of establishing that the elements of similarity are greater in number and more potent than the elements of dissimilarity. However, it is along this line that we are to look for progress in research in the social sciences.

One issue common to all experimental research and not found in dialectic or historical research is the establishment

of approximate or at least sufficient similarity in successive trials or samplings. The issue is clear-cut, and the method is that of successive attempts to secure similar conditions coupled with a measuring of the failure of such attempts. In a certain experiment connected with the rate of growth of wheat, a certain temperature is to be maintained. Now it is nearly universal that our instruments of measurement are more precise than our mechanisms of production. Try our best to maintain 40°C., we shall not succeed within the limits that we are able to measure. Accordingly, we can measure our failure, even though we cannot correct it. We can commonly secure an estimate of our degree of success in maintaining a constant condition, in securing a comparable sample, or in employing the same technique. This process of verification of conditions of experiment is an integral part of the scientific method in experimental research. When measurable discrepancies are found, their investigation leads to a knowledge of their general order of magnitude. Here in connection with experimental science, we find for the first time the concept of probable error attaching to a result and expressed in terms of numerical magnitude, or expressed in numerical terms of probability.

To gain a hearing, the doctor who introduces a new antitoxin must present data upon the probability of efficacy—the percentage of immunity, or the proportion of subjects treated who have been demonstrably benefited. The figures given by the doctor are a statement of what has just been found to hold in the case of a certain sample, but they are also a promise of what may be expected in the future. This concept of future probability is intimately connected with the experimental procedure. Sometimes the probability is so high as to amount to practical certainty, as, for example, that water at sea-level will boil at a temperature between 99.5° and 100.5°C. Thus, if we are boiling eggs scientifically,

we take the temperature of the boiling water as 100° and do not concern ourselves with the possibility of its varying from this. But scientifically we should only show this lack of concern as a result of having earlier established the high probability that the boiling point is between these narrow temperature limits. Professor A. A. Michelson secured a measure of the velocity of light to a large number of figures, but it took him more than another year of arduous work to establish that his earlier answer was correct to within narrow limits. It must not be forgotten that this second year's work was as integral a part of the scientific method as the first. It is not sufficient to "know" a result; it is necessary to know that we know it.

The experimental is the only scientific method that yields this knowledge with any degree of certainty that one demands and is willing to pay for in time and effort. If nothing is known about a certain individual except that he wrote a book, now lost, in 1640, then his date of birth is known within limits. In other words, an appreciable probable error is to be attached to any date of birth given, and it may very well be that no amount of historical research can change this. The certainty of this fact cannot be improved by the utilization of the historical or any other method. If a physicist wishes to know the rate of transmission of sound through a certain medium he can find it once, twice, or any number of times, and secure an answer with the degree of accuracy that he may set and be willing to devote the time to secure.

In Professor John Dewey's analysis of a complete act of thought there is, first, a felt difficulty; second, a definition or specific location of the issue; third, a tentative mental solution; fourth, a mental elaboration of this tentative solution to see if it is thoroughly sound, resulting perhaps in its discardance and the adoption of a second tentative solution—the process is continued until a solution is obtained which

mentally seems adequate; and fifth, this is then tried out in actuality and, if it succeeds, it is accepted as the solution to the problem. If it does not succeed, it results in a new or broader felt difficulty, and the process is repeated until a solution is reached mentally which is adequate when tried out. This results in a sense of conviction, and the problem is solved. I would add one final step. It is not emphasized by Dr. Dewey in his book *How We Think*. After the solution has been found to work there is a mental looking forward, the general purpose of which is to appraise this new solution in the light of possible future needs. I do not think the act of thought is complete without this forward look. If we consider all these steps, we see that there is one step, that of trying out or, as Dr. Dewey expresses it, that of experimental verification, which is possible only in the experimental method. In short, the experimental method is the only one that can parallel in its entirety what we, with Dewey, may call a complete act of thought. Where this is possible, it is the ultimate method. Were one to determine accurately the straight-line distance from Chicago to New York, and ascertain that the probable error of this measurement was one inch, would he then ask travelers what they considered the distance to be, or would he use the historical method and find and trust what earlier people had said the distance was? No, within the limits of his error his determination is ultimate. It is to be replaced only by a determination yielding a still smaller probable error. If a topic can be investigated by the experimental method there is no excuse except that of less effort and expense for using another method. The field for experimental research is as broad as it can operate in, and just as soon as it enters a field it drives out the historical and questionnaire methods, except as preliminary methods. There is no possibility that these will be driven from the face of the earth, but they have

been driven from much of physics, chemistry, astronomy, and engineering, from considerable of geology, botany, zoology, psychology, and medicine, from a little of education, economics, sociology, and business, and from practically none of law, literature, and religion.

It has been implied that the experimental method applies to that field of life, past, present, and future, represented by the present-time chart. The present time must be interpreted as including more than the present instant. We happily are endowed with memories. If it were not so the experimental technique would not be possible. When one lays down his experimental conditions, conducts the successive steps, and notes the outcome, he has at the end the entire experiment before him. If he has forgotten any of it he can do it over and refresh his memory. Memory is a *sine qua non* of research. It can be subdivided into memory for the relevant items, for the possibly relevant, and for the irrelevant. The redintegrative mind, that remembers everything irrespective of its value, is charming as a drawing-room phenomenon, but not of much use in the research laboratory.

In certain fields of investigation the particular items that need to be remembered are so well defined that there is little likelihood of forgetting them. Also in such cases there is usually little tax upon the memory. In another type of investigation the relevancy of certain items does not appear at the time the item is first met, but much later. If, at this later time, the item has been forgotten, the investigator is seriously the loser. Darwin studying seashells and body structures was called upon to remember a tremendous number of characteristics of each of the various species he examined, and hold these facts in mind for years—some of them quite clearly for not less than twenty years. With all these in his mind's eye, he gradually came to see that certain features were significant in relating species with species. At the

end of his investigation, he could drop from memory those items which were not relevant; but through the long years of study up to this point he did not dare forget any item which might sometime prove relevant. In a study such as his it required genius to select well the possibly relevant items and native capacity together with serious effort to remember them. The systematic and classificatory sciences—botany, zoology, geology—impose a tax upon the memory not second even to that of history. A study of the type in question has much in common with an historical study, since the relationships develop and come to light after the evidence is in. If pertinent evidence has been forgotten, the final relationship deduced is but partial, or it is not as well established as might be. This experimental type of study differs from that of history in that the basic items of information are not unique. This was strikingly called to my attention by Dr. Calvin B. Bridges, the geneticist, in connection with the study of the fruit fly. As I watched him at work he noted an odd fly in a certain colony. He said it was a mutation, and brushed it aside without prolonged study. In response to my expression of surprise, he informed me that it would arise again, probably within the next ten thousand flies studied, as mutations always recur.

The process of forgetting non-relevant facts of a plant, an animal, or a geological formation, is always an intrinsic part of the activity of a scientist. The mind, merely as a receptor of sensory impressions, is as prone to remember the irrelevant as the relevant. The geologist viewing a stratified formation on a near mountain, partly in the shadow of a cloud, is rather more impressed merely as a sensory experience with the outlines of the shadow than with the evidence of differences in stratification. He should remember the latter, while he should forget the shadow. It is within his power to choose what should be remembered and what should not.

As soon as we pass beyond the field of such immediate memory as is affected by positive and negative afterimages, those things are remembered, as so clearly shown by Professor R. S. Woodworth, that have been mentally traced in the mind. The shadow and the less pronounced lines of cleavage are within the field of vision of the geologist, and he by an act upon his part mentally traces the stratifications. Very shortly they alone are in the storehouse of memory. The shadow has gone forever. This act of tracing a relevant aspect of an experience is in the experienced scientist habitual, but the building up of the habit has taken time and has been the result of a conscious process. The essence of the process lies in that step of the complete act of thought described as the forward look. The first time the novitiate in geology remembered a stratification, it was because in thinking of his future needs in the light of his geological interest he decided that it was a fact that he would probably, or at least might, need.

Surely there are individual differences in native mental capacity, when one analyzes the steps of memory involved in a complete scientific type of investigation, he sees that they are largely subject to conscious control. First there is the location of an interest—I am a geologist, not a botanist; second, there is the becoming familiar with the field—the study of geology, not botany; third, my future needs, my looks ahead, involve rocks and fossils, not trees and leaves—so that, fourth, from the total of my sensory experiences I will select and trace in the mind only those of geological import. Surely, step by step, these are matters subject to conscious control. Now, if this is so, it establishes the distinction between two sciences such as botany and geology as a phenomenon of nurture, not of human nature. It is in differences existing in the outer world that we seek to differentiate between botany and zoology, not in innate differences in the human mind. These differences which the

world about us imposes are basic in our lives, and not to be swept aside. They probably make or modify a majority of the differences we are aware of.

However, they do not make them all. Within our own structure there are physical and mental differences which result in the same external experiences being different to different individuals. We may say that the difference in a mountain slope seen by a botanist and a geologist is due to an acquired difference between the two, but the difference in the mountain seen by the man of normal vision and one color-blind is not. It is even probable that the difference seen by a natural scientist and an artist is not. A little study shows that many of the important differences between the various fields of life can be associated with differences which are innate in the mental structures of the participants in these different fields. The severity of the memory tax placed upon the geologist by the study of geology is perhaps quite comparable to that placed upon the botanist by the study of botany, while the difference in the two demands is a consequence of training. On the other hand, a different type of memory demand and a greatly different severity in demand is made by history on the one hand and, let us say, mathematics on the other. A man endowed by original nature with a poor memory would find it hard to succeed as a systematic botanist or geologist, but he might do well as a mathematician.

Of all the possible distinctions that can be drawn between different fields of human activity, those connected with differences in the original natures of successful participants are probably the most vital to the student while in the process of choosing a vocation. We have already considered certain of the special demands upon the historian. Though in each of the activities represented by the present-time chart before us every part of a man's nature is called into

exercise, there is a great difference in the insistence of the call. The demands upon verbal facility, word knowledge, and fluency are much less in the physical and biological than in the social sciences. The demands upon mathematical ability are much greater in the physical sciences than in art, religion, and literature. The demands upon keen observation and accurate mental manipulation of spatial relationships is very great in the biological and other experimental sciences. The emotional tone that attaches to experience must be high in art, music, and religion. A sense of satisfaction in social contacts eases the activities of the teacher, salesman, preacher, and social worker. We unhappily have no final criterion for determining if the mental differences just mentioned are in large part due to differences in original nature or not. Evidence can be cited which is highly indicative that each is deeply rooted in genetic structure.

This chapter has been concerned with delimiting the fields of science, and has dealt in some detail with historical research. The next chapter deals with the future and the field wherein human opinion is dominant.

CHAPTER II

THE RÔLE OF JUDGMENT IN "OBJECTIVE MEASUREMENT"

ANTICIPATION is a characteristic of youth, retrospection of age. Forecasting is the youngest activity we are aiming to develop with scientific precision, and history is perhaps the oldest. We should make no invidious comparisons between these two, for each supplements the other, and each fills a very real human need. The anticipation of youth is chimerical unless moderated with the judgment that a study of history can give, and a sojourn in the past is a fantasy, and, however delightful, practically quite futile unless directed to the need of the future. If the past bordered upon the future the only approach to the future would be through a study of the past. We have, however, interposed between the two the present; and due to the happy faculty of memory the present should be thought of as more than a point in time, as an actual expanse wherein resides the experimental method. Our study of the future should therefore draw heavily upon history for background, for perspective, for knowledge of tendencies to date; and it should draw upon the experimental method for the proof, or convincing establishment, of such relationships and sequences of cause and effect as have been suggested by less precise earlier historical and experimental study.

Having as complete proof as historical and experimental study can provide it still must devolve upon the judgment to say that the conditions leading to the as yet unknown future outcome are similar to those of some historical situ-

ation or some just terminated experimental situation. This act of human judgment can no more be avoided than the act of judgment in the so-called exact sciences which concludes that two present things are similar samples, or that an experimental situation is so like an earlier one as to permit its being used for verification of the finding of the earlier one. The experimentalist unconsciously affected by the outcome of his experiment does not ordinarily feel that a very heavy tax has been made upon his judgment when he pronounces two experimental situations as similar.

A chemist is investigating the flashing point of a particular oil. He performs the experiment once with a certain outcome, which I will designate by the letter A. He performs it a second time, keeping some five main conditions the same as before—while many presumably trivial conditions are not the same—and these five are of course not absolutely the same. The outcome of the second experiment is A′, and A and A′ are the same within very narrow limits. The chemist's report is that the experimental conditions of the second experiment were the same as in the first, and this report presumably is entirely independent of the similarity of outcomes. After the experiment is over, knowing as he does that A′ is almost identical with A, he is willing to stake his reputation upon the proposition that the experimental conditions were similar. Would he do so, had the final reading of the experiment, giving the outcome A′, been made by a second person, and the result sealed for much later publication? Perhaps he would have done so under these conditions, but the point is that these are now the conditions of the prophet and not those of the mere experimentalist. They are conditions which throw into high light the responsibility imposed upon the judgment of one who would investigate the future.

Whereas the ordinary experimenter can perform and reperform an experiment, including the initial set-up, the subsequent steps, and the discovered outcome, the person who investigates the future cannot. He is dealing with unique material, just as is the historian. This particular set-up is the only one that is available with which to estimate the particular bit of the future just ahead. Since no experimental verification of similarity of conditions is possible, an act of judgment alone can meet the issue, be it the course of the stock market, the success of a political party, or the future happiness of a newly married couple. No matter how thorough the approach, the question always arises: Are the conditions of this present moment, out of which the future springs, essentially the same as those the outcome of which is known as a result of the historical and experimental study? Clearly, if this question is answered in the affirmative, there must always remain some probability that the answer is in error. In the case of most astronomical forecasts, this probability is surely very small. However, even here, if newspaper reports are to be trusted, twentieth-century eclipses do occur at times measurably different from those predicted. Meteorologists make errors in their weather forecasts, and prognostications of adult mental level from the test performances of childhood are at times sadly in error. From the most precise scientific forecasts to the least there is no exception to this fact of error of estimate. We must accept the principle that in the estimation of future facts there is always an act of judgment and always a probability of error.

Another kind of forecasting centers around mental values, as such values always have a future reference. The curriculum is an important illustration of this class. The inclusion of any item in a certain curriculum is a result of the estimate of some person or some group that knowledge of it will have a value in the later lives of the pursuers of the curric-

ulum. When we consider the importance in our lives and in those of our children of elementary, high-school, and college curricula, we cannot in sense and reason turn away from this problem of future values with a sneer that it is outside of the realm of science. Light may be thrown upon it by the best exercise of historical investigation, by innumerable facts revealed by experimental method, and by checks upon the outcomes of the soundest forecasts, put into practice, of earlier years. There is no step in the complete act of thought, including that of experimental verification, that should not be involved in the attack upon this problem.

The fact that many of the values that lie in a curriculum are values because fellow-citizens so consider them increases the difficulty of the problem, making it necessary to judge the outcomes by agreement with human judgments of values rather than by agreement with external objective facts, but it does not otherwise change the technique of investigation. If 100,000,000 people in the United States think it valuable to know that Columbus discovered America in 1492, then to a prospective citizen of the United States, one who is to live and talk the same language with such people and their children, it *is* valuable to know this fact. In a case like this, the scientific builder of a curriculum is called upon to ascertain what "the people" think about the importance of Columbus and 1492, just as the physicist is called upon to ascertain what God thinks about gravity. God thinks that under certain conditions the rate of acceleration should be constant, and we may believe He does not change His mind. What God thinks is the physicist's standard.

Though "the people" do, over long periods of time, change their minds, no more ultimate standard is at hand, so that the builder of a curriculum is obligated to know this social standard. Some things are matters of evidence, and no social judgment to the contrary is entitled to any weight

—the rate of acceleration of a falling body is of this sort. Some things are matters of human belief, and no experimental evidence affects them—such is the present meaning of a word. Surely in a world wherein words play the part they do with us, there must be rules of procedure for an investigation connected with them. There are, but the rules are not as incisive as those of physical science.

Because the serious investigator of human values has a more difficult task than the natural scientist, he is not outside of the pale of science. Rather he is more deeply involved in it. The intelligent putting of the question "What is the rate, or rate of change, of acceleration of falling bodies?" finds an invariable response. The best conceivable way of ascertaining the meaning of a word, let us say "virtue," will yield a variable response, and a properly weighted mean tendency of responses must be found. This second problem is intrinsically more difficult than the first. We should not say that because of the greater difficulty workers with this second problem are better scientists than workers with the first; for the excellence of scientific activity is to be judged by the rigor with which the scientific method has been adapted to the problem in hand, but we certainly may say that there is nothing in the problems which makes one a more scientific problem than the other.

Determining a word's meaning is a problem in forecasting, for its meaning is its significance when found in future situations. Various complexities increase the difficulty of this operation. Forecasting offers greater hazards than historical or experimental investigation. As a standard, the consensus of human opinion is clearly more variable and difficult to ascertain exactly than a standard which is a law of inanimate nature. The intrinsic complexity of the concept of a word like "virtue," even in the best consensus of opinion sense is great compared with the complexity of

most of the terms distinctive of physical phenomena. Truly all the hard problems have been thrown together in this field of value—the future meaning of human expressions.

The instrument for attacking this problem is the questionnaire. What an anticlimax! The most difficult field of research, that possibly the most pregnant for human welfare, to be handled by the weakest device that has ever obtruded its prying and at times discourteous self into the respectable field of science! It is necessary to admit this, for how are mental values to be ascertained save by finding out what the valuing agents believe concerning them?

Occasionally a field of human activity undergoes a transition, and changes from one in which the issues are purely mental to one in which they are objective in the scientific sense. Such was the case when cosmology ceased to be religion and became astronomy. When this took place an entire set of issues earlier to be answered by human judgment disappeared in thin air, and new issues, to be solved by scientific observation, took their place. No thinking person will deny that this was a great social advance. Medicine is undergoing the same transition, business is influenced, and education is touched. We should encourage the movement, but meanwhile it would be foolish to deny that there are and always will be wide, important, intimate, and enjoyable phases of our lives in which mental appraisals rule. Does one listen to "Lead, Kindly Light" because of considerations of candle-power? Are teachers hired because they have been found to stand high on a yardstick of honesty, or industry, or fill a scant peck of trouble? From university professors down, they hold their positions as the result of human judgments, or misjudgments, as the case may be.

This matter of misjudgment is important. All that we mean by it is that more and better judgments disagree with it. A first university professor is a better one than a second

if in the long run those, especially in responsible positions, appraising university professors so think. When there is disagreement, the minority, again giving proper weight to the various judgments concerned, is in error. In this field of human values there is no more ultimate definition of error. Excellence in the matter of professors is that which in the long run is believed to be excellence. Though a changing standard it is for any given era ultimate. Obviously it cannot be judged by the standards of an era not yet in existence. An Einstein, if guilty of the same extremities of thought as today, would probably lose his position to a Euclid in early Grecian days, and rightly so: he would have been a poorer professor at that time.

It has been intimated that judgments of human values should be weighted in accordance with the importance for the matter in hand of the judge. The one judging gains his importance because of the judgments of still others judging him, and the process continues *ad infinitum*. In judging the excellence of university professors, every Tom, Dick, and Harry should not have equal weight with, say, university presidents, colleagues, and national associates in the same field. Tom, Dick, and Harry would not themselves wish that their judgments be given equal weight with experts in the field in question.

Ask one hundred persons, chosen at random throughout the United States, for advice as to the best entomologist. It is to be expected that most of them will admit being poorly informed upon the matter of entomologists, and advise seeking advice from office superiors, gang bosses, teachers, preachers, doctors, and locally known men of affairs. Ask these and they will delegate the task to college graduates, scientists, teachers, etc. One may continue the process, being confident that upon every step he is getting more and more intelligent and trustworthy judgments. The final

group is representative of individuals whose judgments should be heavily weighted as compared with those of the first group, and it is to be noted that this heavy weighting has come out of the judgments of the first group itself.

A weighting somewhat of this general character was in the minds of the framers of the Constitution of these United States in connection with the election of the President. A return to a weighted judgment in selecting experts even for the highest federal administrative office would be in harmony with other social practice; scientific associates are more potent in establishing the merit of scientists than people in general; literary producers are more potent in establishing the excellence of literary production than readers of literature, and they in turn more potent than non-readers. And so it goes throughout the entire field of human thought.

An idea as to what is the consensus of opinion may be gotten in many ways, but some systematic procedure is highly desirable if a scientific issue hinges upon the outcome. A carefully worded questionnaire given verbally to a goodly sample of competent judges by friends in such a manner that serious answers are given just as a matter of courtesy, would probably yield as nearly fair results as could be expected. These conditions are seldom if ever obtainable; in fact, the fairness of conditions is nearly always in question. The various issues affecting questionnaires in general can well be discussed in connection with a particular questionnaire, of which I, the author, am guilty.

The purpose of this questionnaire was to determine what scientists consider the place of questionnaires and also what is appropriate conduct in the matter of giving credit for aid received in research work. This matter reached such importance in courses upon research taken by students writing theses, that I felt justified in imposing upon my colleagues

at Stanford University for their opinions. Before presenting certain results of this questionnaire, I wish to emphasize that what constitutes appropriate acknowledgment of aid received is what fellow scientists so think. No set of instruments, no well-fed rats, no armchair speculation, can give us the final answer to this question. It is a question of human appraisal, and whatever is the consensus of opinion is the ultimate answer. I did not therefore use a questionnaire where some other method was possible. The questionnaire is as follows:

ACKNOWLEDGMENT OF AID IN RESEARCH WORK

Stanford University, May 25, 1928

Dear Sir:

This is a questionnaire dealing with the granting of recognition for help received. I am sending it to professors at Stanford in the various fields of science. Even though you consider all questionnaires unmitigated nuisances, or are especially busy, I still hope you may see fit to answer Question (a).

If you think that beginning research workers might profit by evidence as to what scientists consider appropriate acts of courtesy in giving recognition to colleagues and to others, kindly run through the questionnaire rapidly. I shall send a summary of the findings to all who reply, but I shall not quote specific individuals.

Very sincerely yours,

(a) In your opinion is a questionnaire ever an appropriate instrument for use in the conduct of serious research in any scientific field?

In the following paragraphs, A is the author of a research and C is a person who may or may not be entitled to credit for some phase of it. In each instance the question raised is whether A should give credit (in preface, text, or footnote) to C. One of the following answers written in the right margin is desired: *Yes; No; Optional* (i. e. no ethical issue is involved, so the matter should be entirely at A's option); *?* (i. e., the appropriate practice would vary and would depend upon conditions not men-

tioned in the paragraph. It is to be assumed that all conditions not mentioned are quite typical or normal).

1. C is A's paid draftsman and does high-grade work. (In questions 1, 2, and 3 the pay comes from A.)
2. C is A's paid mathematical assistant and in the pursuit of A's problem develops formulas and provides mathematical proofs of which A, only an average mathematician, is incapable.
3. C is A's paid assistant and in the pursuit of A's problem hits upon a novel technique (one unknown to A and not in general use) which A forthwith adopts in his future work.
4. How would you answer Questions 1 to 3 if the pay accruing to C comes not from A, but from institution funds allocated upon the recommendation of A? 1..... 2.....3.....
5. How would you answer the same questions if C is A's wife receiving no "salary" from A? 1.....2..... 3.....
6. C is a colleague of A's in a university or research institution and has independent duties. Incidentally, in a shop talk, A gets a novel idea from C which he proceeds to utilize.
7. C and A as in 6. In a conference called by A, who is a poor mathematician, A gets and proceeds to utilize mathematical aid such as might be given by any one having had a first-year college course in algebra.[1]
8. Same as in 7, substituting "calculus" for "algebra."
9. Same as in 7, substituting "advanced differential equations" for "algebra."
10. Student A gets an idea from a lecture given by Professor C to an elementary college class and elaborates and incorporates it in a published study.
11. Same as 10, changing "elementary" to "advanced."
12. A is writing a dissertation under the direction of C. Certain techniques given in the literature are called to A's attention by C. These are adopted by A, but the problem and all novel techniques have originated with A.
13. A as in 12. A feels "stimulated and inspired" by C, but cannot point to important specific techniques for which C is responsible.

[1] The word "year" should have been omitted from this question on account of its connection with 8 and 9.

14. Professor A gets an idea from a class paper written by student C, and elaborates and incorporates it in a published study. (Any comment you care to make on Item 14 will be appreciated.)

Now if there are readers who do not already know it, it may be stated that the receipt of a questionnaire causes the temperature of certain people to rise. I jeopardized my good, or at least uncolored, standing in more than one instance, in sending out this questionnaire, but I do not regret it, for I believe the results are of definite value to writers, especially young writers of scientific articles.

Question (a) quite unmistakably has the words "ever" and "any" in it. Read it again:

(a) In your opinion is a questionnaire ever an appropriate instrument for use in the conduct of serious research in any scientific field?

Now the high-temperatured recipients of the questionnaire seemed to think that I took an unfair advantage of them by putting in these words. They wanted to answer this question with the word "no." In fact, some of them did so answer it, and then qualified their answers in such a manner as to mean "yes." For example, a biologist writes, "No, with possible exception of psychology"; a medical man writes, "Rarely"; and another states, "Usually not satisfactory, but sometimes the only possible method"; and another, "No, except as a method of determining average opinion, interpretation, point of view"; a psychologist, "Not in general. It is, in rare instances"; another, "Very infrequently."

Of course, if there is any exception, the answer to the question as put is not "no," but "yes." I have no desire to take an unfair advantage of a person who writes "yes" to this question. In this case, all that I infer is that he thinks there are one or more conceivable situations in which

a questionnaire is appropriate in a serious research. I certainly do not infer that the respondent is a general believer in questionnaires.

With all due deference to my Stanford colleagues just quoted, I submit that these are illogical answers to the question, which was, "Is a questionnaire ever an appropriate instrument?"

The questionnaire was sent to 204 scientists at Stanford, and 155 replied. Three of these replies, however, contain practically no information, so that 152 usable replies were received. I feel very grateful to my Stanford colleagues for their courtesy in answering, for I know that many of them quite resent receiving a questionnaire. Of the usable replies no less than 20 were "no," qualified in such a manner that they were logically answers of "yes" to the question as put; and no less than 30 answered "yes" with elaboration so that I would very clearly understand that they had serious misgivings as to the value of the questionnaire. A very characteristic feeling is found to run through these elaborated "yes" responses. A biologist says, "Yes, but rarely"; an engineer, "In some cases"; another, "It may be appropriate, depending upon the type of research. This most certainly is, since it is a question of 'opinion'"; another, "In rare instances"; an historian writes, "Yes, but rather rarely, where attitudes or opinions as such are valid data"; a worker in the field of medicine writes, "Yes, but damn seldom." I also found the comment, "Questionnaires are as a rule not viewed with enthusiasm by the recipient, and the casual way in which they are often treated is the greatest practical drawback to their utility." Need I add that the last two quotations are not from the same author, however similar their views?

Another in the medical field prints "No" with an exclamation point of sufficient size to be read across the room.

This was not the only evidence of feeling that a sensitive soul could infer from the questionnaires returned. Another writes, "Yes, but with marked limitations. If used to obtain facts"; a physical scientist writes, "It might be in psychological research"; I have given you only a few of the qualified statements. One-third of the replies were qualified in some manner. The reason may be that those replying felt that in justice to the logic of the situation, they had to reply "yes," but that in justice to their past experience with questionnaires, they needed to assert that ordinarily they did not deem them satisfactory instruments. Some of these qualifications may have been due to the feeling that the interpreter of the results would misinterpret the respondent's views unless the respondent himself provided the cue. One social scientist replied to the question, "Yes and no. I give this answer because I could not leave it to anyone else to interpret my 'yes.'" Unfortunately the fear of misinterpretation here expressed is justified by certain unhappy experiences which are the rather common lot of those who reply to questionnaires. I sincerely hope that my interpretation of this present questionnaire will do no injustice to anyone's view.

Seventy-three, or about 50 per cent of those replying gave the single word "Yes," but we may well believe that they were also more or less skeptical of the practical value of questionnaires.

Fifteen, or 10 per cent, replied with the word "No" unqualified in any manner, and another 10 per cent gave no answer, or answers of uncertain meaning. We thus find that between 80 and 90 per cent of those replying can conceive of some situation in which a questionnaire is an appropriate instrument for use in the conduct of serious research in a scientific field. A very substantial number of these doubt its general utility. Two views as to the field in

which it is useful may be inferred from the quotations read; *e. g.* one correspondent writes, "Yes, but with marked limitations. If used to obtain facts"; and another, "Yes, but rather rarely, where attitudes or opinions as such are valid data."

Are these very different views both correct? If facts of a unique nature known only to specific individuals are desired, then an oral or written questionnaire addressed to the one person knowing these facts or to one of a few who know them, is surely the only method of approach. It is in fact an historical method. Let me mention the report of a questionnaire of this sort that runs into a couple of thousand pages, and that is used by thousands. I refer to *Who's Who in America*, or to any similar reference book. This is merely a report of a questionnaire. If we had an authentic *Who's Who in Rome* in the year 100 B.C. would it not be held as the most authoritative of historical documents? Surely this type of questionnaire is justified.

Now consider a questionnaire of the other type—one that merely aims to secure a consensus of opinion. We had such a one in these United States of America upon November 6, 1928, and as a result of it Herbert Hoover was elected President of the United States. It was a situation in which we wished to know the consensus of opinion as to the fitness of certain men for office, and we used the Australian ballot, which is simply a secret questionnaire.

In these two fields the questionnaire operates and it alone does operate. We cannot investigate unique facts by the experimental method. No man born on the Fourth of July is born again on that same date, thus permitting a verification of the fact that the Fourth of July is his birthday. To establish this fact, we have to ask someone who knows.

Also, we cannot secure a consensus of opinion upon some issue of meaning or value except by questioning all or

a sample of those the consensus of whose opinions we desire. Therefore just as long as these consensuses of opinion are important to know, a questionnaire is the only appropriate instrument for their discovery. As one prone to trust objective measurement, it is with regret that I find no other instrument for studying this phase of human life.

Some answers to question (a) were neither "yes" nor "no" as requested, but "uncertain." If we call an answer with the word "uncertain" one-half "no" and one-half "yes" we can determine the average number of "no" answers by various scientific groups. Upon this basis there were, from the entire group, 18 "no" answers, to this first question, which is 13 per cent. These were drawn in quite different proportions from the various sciences. Ranked in order of decreasing percentage of "noes," we have [1] one-fourth of those in the physical sciences—physics, chemistry, and geology—expressing an unqualified disbelief in questionnaires, one-sixth of those in mathematics and in medical science so doing, one-eighth of those in history and engineering, one-twentieth of those in the biological sciences, and none at all in the psychology group—represented by education, philosophy, and psychology—or in the economic group represented by business, economics, and political

[1] SUMMARY OF ANSWERS GIVEN TO QUESTION A

	YES	NO	PERCENT OF NOES
Biological Group (Botany and Zool.)	21	1	5
Economic Group (Econ., Polit. Sci., Business	20	0	0
Engineering Group	26	3½	12
History (Hist. and Journalism)	13	2	13
Mathematics Group	5	1	17
Medical Group (Phys., Anat., Pathology, Med.)	28	6	18
Physical Science (Phys., Chem., Geol.)	14	4½	24
Psychological Group (Psych., Phil., Educ.)	17	0	0
Total	144	18	11

science. Clearly, answers have been colored by the specific vocation in which the person answering is engaged. Thus the physical scientist, who in his scientific work is never interested in a unique fact or in a consensus of opinion, is quite prone to consider that the questionnaire is entirely indefensible, while the social scientist, forced to deal with these things, shows the most confidence in it. I would not, however, give the impression that the 37 people in the social sciences replying were uncritical of the questionnaire. The arraignments of many of them are severe.

Let me now turn to the later questions. No matter whether the scientific investigator is in the physical or the social science field, when it comes to writing a report for the reading public, there is a single standard of courtesy which should be shown, and it is that given by the consensus of opinion of scientific people. It is true that some who replied to my questionnaire expressed annoyance saying that any honorable man would have right standards of his own, and be fully competent to answer all questions pertaining to crediting colleagues and assistants. However, the judgment whether this is so lies with fellow-scientists. Certain it is that my respondents have radically different standards, as I can show by quotation; and I feel no warrant for attributing these to differences in honor—rather, these men have had different experiences, have been bitten in different places, have made different acts of mental transfer, this one adopting his policy from the courtesies of society, this one from the practice of business, etc. Judged by the consensus of opinion, and I reaffirm that that is the appropriate standard, many of the views expressed are erroneous, but from the internal evidence I do not think any of them are insincere. The consensus that I can report is that of some 150 people constituting a sample of just such as establish the standard in this matter. If it is in error it is only to be so established by a

wider consensus. Before discussing the specific questions I must explain in a few words the steps followed in summarizing the replies. In tabulating the answers I have taken certain liberties as follows: Credit for the reply, "uncertain" is equally divided between "yes" and "no." Credit for the answer "yes—optional" is likewise split between "yes" and "optional." Credit for the answer "yes—?" is split between "yes" and "?," but as the "?" was used only four times in some two thousand replies, whatever is done with it is quite immaterial. In some 5 per cent of the replies the meaning was a trifle ambiguous. In case the ambiguity was pronounced, the reply has not been used in the tabulation; but in general the meaning seemed to be quite clear, and tabulation made according to the judgment of the tabulator. One reply which was ambiguous so far as the specific question, "Should the author give credit to the contributor in the preface, text, or footnote?" was repeated several times. It was, "The author should make the contributor a joint author." This view is well represented by the following quotation. "I think that one of the best ways of meeting such situations as these is that of joint authorship. That was the method of the greatest, most inspiring teacher and research man I ever knew, David Starr Jordan, who nearly always made his co-workers junior authors with himself." The question of joint authorship undoubtedly might arise in connection with many of these issues; but, on the other hand, every hypothetical case described in the questionnaire might be such that joint authorship would be highly misleading, for the author's contribution might be one hundred times, or more, that of the contributor. Those proposing joint authorship seem to do so in a generous mood, but it may be questioned whether an author, no matter how truly generous and desirous of helping a colleague or assistant, is treating his public fairly if he puts over a joint authorship

work which is almost entirely his own. Generosity may cease to be a virtue when this is done.

From the accompanying table giving a summary of the replies to the various questions, we may note some of the important tendencies shown.

SUMMARY OF ANSWERS TO QUESTIONS 1–3, 6–14

	1	2	3	6	7	8	9	10	11	12	13	14
Yes..	42	116.5	133.5	106	31.5	44	69	64	75.5	51	41	95.5
Opt..	53.5	16.5	7	15.5	35.5	36	24	26	24.5	33.5	66.5	12
No. .	48.5	10	2.5	13.5	67	50.5	38	44	33.5	49.5	24.5	26
? ..					1	.5			.5			.5
Total	144	143	143	135	135	131	131	134	134	134	132	134

CHANGES IN ANSWERS TO QUESTIONS 4 AND 5 FROM THE ANSWERS TO QUESTIONS 1, 2, 3

	4			5		
	(Q1)	(Q2)	(Q3)	(Q1)	(Q2)	(Q3)
Greater obligation to credit.....	12	8	5	37	13	6
Less obligation to credit.....	2	2	0	4	9	7

Questions 1, 2, and 3 were drawn up with the intention of providing a series of situations wherein the amount of the contribution differed, with the hope of developing some principle which would be a function of amount. In question 1 the wording is intended to imply that the contributor, a draftsman, gives of his best trained services; in question 2 that a mathematical assistant, trained in a more intellectual calling, gives of his best; and in question 3 that an assistant gives of his best, and in doing so provides a technique that the author could not well have had in mind when hiring him.

The first question is:

C is A's paid draftsman and does high-grade work. (In questions 1, 2, and 3 the pay comes from A.)

The median reply to this is that it is optional with the author whether credit be given or not. In sentiment those replying with the word "optional" certainly lie in an intermediate position between those replying with "no" or with "yes." Thirty-three per cent stated that no credit should be given, 37 per cent that it was optional with the author, and 30 per cent stated that it should be given. A not uncommon comment in connection with question 1 was that it would not hurt the author any to give the draftsman credit. This is perhaps a selfish point of view, but that hardly lessens its importance.

There are three parties to the transaction we are discussing —the author, the contributor, and the scientific public, and it is important that no unfairness to any one of the three result. It would be an unsound standard to judge of the fitness of the procedure by its effect on the author only. In fact, if any "rights" are to be neglected, it is only his own that the author has the right to neglect. The view just mentioned was frequently voiced, as witness these statements: "I have answered (the question) generally in what may be termed the larger courtesy. It does no harm to be generous in such matters, and one can well afford to do the graceful and generous thing in matters of this sort." A second quotation: "This is a matter of courtesy rather than a matter of ethics, if one can separate the two. C has been paid for his work in cash, but if his work is unusually good, the courteous thing is to acknowledge it in the preface." Here is a different view. "No credit *need* be given to any assistant whether common laborer or Ph. D., chemist, or other, for applying any methods known to his craft." Common observation shows us that stenographers, typists, draftsmen, printers, and proofreaders do not ordinarily receive published credit for their parts in a published work. The following view, voiced by a chemist, is not uncommon,

though probably not quite a median opinion. "If the paid assistant did only what he was told to do, however well, but made no independent contribution to it, there should be no occasion for mentioning him. . . . There would be an endless list to recognize if one went beyond the bounds of contributions which are notable, specific, and original." Occasionally a technical or clerical worker does receive recognition. When Professor Karl Pearson acknowledged in print the excellent typography of an involved statistical article, we felt—that is, if we tried to read the article—that the acknowledgment was well merited. The workman here who set up the involved mathematical text did not do a routine job—not a set-up in a score would be as well done. The workman had gone beyond what is reasonably expected of typesetters, not in originality, but in skill. In granting this recognition, one need not believe that Professor Pearson was making a special effort to be kind and generous, rather that he felt as a matter of ethics that recognition of this superior piece of artisanship should be given. The less the granting of recognition is in response to the whim of the author, and the less it is influenced by personal feelings of friendship, of inferiority, or superiority, the better. The author who goes out of his way to give credit to eminent men and to his superiors in rank, and who never, judged by any statements of his, gets help from contemporaries of equal or inferior rank is not unknown. Though he may not be respected in his own community, he not infrequently does make quite a showing in distant parts.

Endeavor as one might to make a general practice known and thus introduce some sort of standard into the giving of credit, there will always remain a field wherein the judgment—not the friendship—of the author should operate. It seems that the granting of recognition to the draftsman is in this field.

Questions 2 and 3 are:

2. C is A's paid mathematical assistant and in the pursuit of A's problem develops formulas and provides mathematical proofs of which A, only an average mathematician, is incapable.

3. C is A's paid assistant and in the pursuit of A's problem hits upon a novel technique (one unknown to A and not in general use) which A forthwith adopts in his future work.

The median judgment upon these questions was that recognition should be given: 81 per cent so affirming in connection with question 2, and 93 per cent in the case of question 3. The contributor referred to in question 2 is not a mechanician, but one who possesses a mental capacity and information commonly gotten by more or less advanced study. He uses this whole-heartedly in the cause of the author. He is, if you will, a high-grade mental artisan. It seemed to be quite generally the feeling that he should be credited. The occasional observations written in would seem to warrant the belief that those who answered question 1 in the negative and questions 2 and 3 affirmatively were generally of the same opinion as was the professor who wrote: "Originality is the first test in these cases. Information that anyone in his position would be expected to give needs no mention." A certain few answered question 1 "No" and questions 2 and 3 "Yes," who held different views. One advised giving credit in the situation of question 2 "unless A is so poor in mathematics that the work was simply like looking up a reference." Thus pronounced non-mathematical ability upon A's part is given as an argument for not giving credit. Another advised giving credit and commented as follows: "This questionnaire fails to recognize the fact that credit cannot be divorced from responsibility, and that he who takes credit beyond his understanding rides for a fall." Thus pronounced non-mathematical

ability on A's part is here given as an argument for giving credit. Can both of these views be right? Possibly they are. If A does not now remember the binomial theorem, and asks his mathematical assistant to state it for him, surely no acknowledgment is needed. But if A does not know that such a theorem exists, or its function, but his mathematical assistant does properly use it for him, then even though meanwhile as a result of his assistant's tutelage he has learned the appropriate use of the theorem, it may well be that acknowledgment to his assistant should be given. Still another view, though one that hardly seems tenable in university fields, whatever the practice in industry, is that the author need give credit for nothing that his paid assistant does, provided he himself can understand and master it. The quotation given mentioning the sad plight of the fellow who comes a cropper when he fails to give credit for matter beyond his understanding pictures but one aspect of the threefold view already mentioned. For one's best selfish interest he should not claim too much, but long before a check because of fear of getting caught is operative, there should be another check to conduct, in a desire to be fair to one's helpers and one's public. One should not claim matter as his own upon which is the stamp of discovery of another, whether minor assistant, casual acquaintance, or eminent colleague. The unanimity of opinion in the case of question 3 that credit should be given to the contributor is not due to a similar unanimity in reason. Some of the best reasons given draw a distinction between the situations in questions 1 and 3, or 2 and 3. A person earlier quoted states: "Credit must be given to any student, whether common laborer or Ph. D. chemist, who has invented or discovered either previously or for the occasion, a method required as a link in the boss's chain." As pertaining to questions 1, 2, and 3, and others, one writes: "The crucial point is whether the

assistance is an intellectual contribution or a merely mechanical contribution. . . . I maintain that such assistance is as definitely mechanical in differential equations as in draftsmanship. To acknowledge it should be optional. When the assistance involves a *novel* suggestion, however, it becomes necessary for the man who receives it to acknowledge his indebtedness scrupulously, whether to a famous colleague, a paid assistant, or a patient relative." The patient relative aspect is considered in question 5, and source of pay is considered in question 4.

Question 4 is:

> How would you answer Questions 1, 2, and 3 if the pay accruing to C comes not from A, but from institution funds allocated upon the recommendation of A?

Of those who replied to questions 1, 2, and 3 that no credit should be given or that it was optional, some 10 per cent in the case of question 1, 24 per cent in the case of question 2, and 50 per cent in the case of question 3 considered that there was a greater obligation that the author give credit if the author is not the source of pay than if he is. The rest expressed no change in obligation with a change in source of pay. Something of the standards of business do seem to apply. These standards would proclaim that if the services of a helper are paid for by the author they belong to him. The opposing view seems to be a majority opinion, though we may doubt if the majority view would go as far as one person who stated: "It does not make a particle of difference as to where the pay comes from." The majority view is better represented by the following: "[Give] credit where credit is due. The question of pay, salary, etc., does not enter in most cases. Even though the pay comes from A in each case, seldom does A personally support the research. If A is in business, the funds come from the business, or A

is merely the custodian of funds set aside for some particular use; seldom if ever does A represent the actual source of the funds, hence the matter of pay does not to my mind enter the problem." This writer seems to hold that though theoretically the source of pay is an issue, practically in such situations as ordinarily arise, it is an immaterial point. I do not believe from any evidence at hand that it would still be considered immaterial if the decision were between recognition for work done for pay and for work done out of kindness, by some willing-to-be-imposed-upon acquaintance. Probably under such a condition the percentage who would consider there was greater obligation to give recognition when the assistance rendered was not paid for than when it was would increase considerably over the 10 per cent, 24 per cent, and 50 per cent found when the pay was shifted from the author himself to an employing institution. Perhaps source of pay has very little to do with the matter, but pay or lack of it is quite certainly a factor. One respondent to the questionnaire certainly does not consider it minor. He writes: "Originality is the first test. . . . The question of pay is the second test. If the assistant is paid he obtains that in place of credit except . . . in exceptional cases. If in doubt, give credit." It is regretted that more direct evidence is not available, but it seems quite certain that the last view given is not representative of the median view. Considering the answers to question 4 together with those to later questions, it seems reasonable to believe that the median view holds that pay, as contrasted with lack of pay and possibly even source of pay, is a factor. Several respondents expressed the opinion that in the case of paid workers the practice of recognition should be a matter of contract or mutual agreement entered into at the time of employment. This does not seem to be a practical solution, for though it settles all issues as between author and contributor, it does

not settle them as between the author and the public. Professionally it would be very unfortunate if a man of wealth could, by agreement with employees, foist himself upon the public as a great research worker, if, in fact, the brains back of his publications were those of his paid assistants. It would be better that there be no contract upon this matter than that there be one neglecting the rights of the public.

Question 5 is:

> How would you answer questions 1, 2, and 3 if the contributor is the author's wife receiving no "salary" from her husband?

This question treads upon delicate ground, and the varied response indicates that little can be told as to just what part a wife has played in a research, by the author's published acknowledgment. We may, however, get a rough idea as to what is the central tendency of opinion. In the matter of crediting assistance of the grade of draftsman, there is a tendency voiced by some 36 per cent of those not stating in answering question 1 that it was obligatory that the draftsman be recognized, that there was an increased obligation that the wife be given credit. This 36 per cent represents persons who said the draftsman should not be given credit, but that it was optional or obligatory that the wife-draftsman should be, plus those who said it was optional in the case of the draftsman but obligatory that credit be given in the case of the wife-draftsman. Some 10 per cent took an exactly opposite view—that there was less obligation to credit the non-paid wife than the paid draftsman. This issue is truly not a simple one; to credit a wife for draftsmanship when the husband is getting renown for, let us say, proving that the earth goes around, may be the occasion of very invidious comparison. The possibility of this may not have occurred to the 36 per cent who would credit the wife, but not the draftsman. One would be loath to conclude that

the husbands would wittingly place their wives in a position permitting unfavorable comparison; rather, one would believe that the 36 per cent held that there is a greater obligation to spouse than to paid draftsmen, and aspired to express such added obligation by giving recognition. This seems to be correct, for in connection with the higher talent considered in questions 2 and 3 we find even greater percentages—50 and 60—believing that there is greater obligation that the wife be credited than that the paid mathematician or research assistant be. In these two cases no invidious comparison results when activity of wife is compared to that of husband. There is a small minority who take just the opposite view. Approximately 8 per cent maintain in the case of question 2, and 5 per cent in the case of question 3, that there is less obligation to credit the wife than the paid worker. No written comments throw light upon the viewpoint of this small minority, unless the observation of the professor who wrote, "Do anything you can get away with," is pertinent. I do not hazard to pick one reason for the tendencies shown out of the multitude that any normal married couple could propose. Rather let some of the husbands speak for themselves. This may not be entirely fair to women, for most of my respondents are men. It will not, however, be unfair from the standpoint of a consensus of opinion of scientists, for most scientists are men. So far as the median view is determinable, we can say that there is slightly added obligation to give credit to a wife than to a paid worker, though a goodly number maintain that there is no difference in obligation due to the marital relation. A biologist states, "Credit obligatory if no salary. Wife relationship not the issue." A social scientist expresses the opinion that "the factor of family relationship does not alter in the slightest the ethical question." Just the opposite view is voiced by another social scientist, who writes, "This

is a question of family relations on which I hesitate to enter." It seems to be viewed as a matter of family relations by the person who wrote "I would not dare give other than full credit to my wife." Still another, ". . . would mention [his] wife, an act of courtesy only." A small group proposed that the author at least give the wife the opportunity to be mentioned or not as she may desire. As the rights of the public are generally minor in this matter, this has much to commend it.

The candidate for the doctorate who is publishing his first research should carefully consider this matter from the standpoint of his young wife, and of the outsider observing them both through the medium of the preface. I shall not be so unkind as to quote from the prefaces of certain doctors' dissertations, but one finds in many of them acknowledgment of the major professor for his vision, continual inspiration, and guidance through the intricate mazes of the research, and to the wife for self-sacrificing aid in computation, typing, and in the taxing job of proofreading. It is not occasionally, but is generally the case that the young married candidate for the doctorate is obligated, very deeply obligated, to his wife, but such recognition in the preface as I have mentioned is a cheap coin in which to pay. I am sure it is ordinarily not so intended, and that it is merely an error of judgment which leads to acknowledgments of this sort. One would think, as he reads of computing, typing, and proofreading, that the wife's contribution is represented by the saving of a few dollars for clerical help. As I have seen the sacrifices of wives "for the good of the cause" I know that typically this is not the case. The wife, unskilled in computation, spends more than mere hours of time when she struggles with the columns of figures which the husband gives to her. The hunt-and-peck typist or even the five-finger artist, does not, in the service of a

husband, stop when the clock strikes 5. She works after the children are in bed until midnight, and so it goes. No acknowledgment in the preface can pay this debt, and usually the attempt is a sorry failure. As to how the husband shall meet the obligation is a family matter which I do not conceive to be my business to attempt to answer, but that a few kindly condescending words in the preface does not meet it is a public matter just as soon as the masterpiece is in print. Question 5, as put, stated that the husband was the author and the wife the contributor. To gain perspective I suggest that when writing the preface the husband imagine that the situation is reversed, and write as though the wife were the author, and the husband the contributor. I dare say it will be quite an illuminating experience.

Let us now consider issues arising between university and research institution colleagues.

Question 6 is:

> C is a colleague of A's in a university or research institution and has independent duties. Incidentally, in a shop talk, A gets a novel idea from C which he proceeds to utilize.

Ten per cent of the respondents stated that no credit should be given, 11 per cent that it was optional, and 79 per cent that credit should be given. There were a number of qualified answers. Typical of one wing is the professor who said, "Yes, [A] should credit C, and should ask permission of C to use [the idea]," and also the one who wrote ". . . if two scientist talked together about their plans for future research, and number two proceeds to utilize one of number one's ideas and gets a piece of research done on it (this has happened to me two or three times) then I consider that number two's behavior is absolutely unethical, and no amount of public acknowledgment can make up for his behavior." It seems that to these men quoted the "idea" remains the

sole property of the originator, even when he has not earmarked it and staked out future rights in it, as of course he would not, if it occurred incidentally in a shop talk. Of quite a different view is the pessimist who writes, "Just getting an idea involves no obligation. Conversation is supposed to be (occasionally) stimulating." Also quite different is the following view:

> As related to this subject, I want to say that I believe in the most complete freedom in the commerce of ideas. In other words, I believe in a man airing his ideas and letting the consequences fall where they may, not troubling himself to inquire whether he gets credit for them or not, no matter how original and valuable they may be. Likewise, I believe in a teacher giving help, regardless of whether the student or other person ever gives him recognition or not. In other words, regardless of the manner in which these questions are answered, I should not change my position on this fundamental proposition.
>
> In other words I don't believe in one man saying "dibs" over an idea, or in his acting in any dog-in-the-manger manner about it. However, I appreciate, from the other point of view, the fact that any courteous and considerate person will give all the recognition he can to those working with him and associated with him, perhaps even giving credit for good intentions when no service of any real value was rendered. Usually the man who can give credit occupies the more strategic position and a little notice of a poor devil farther down on the ladder makes the latter struggle harder—assuming this is desirable.
>
> It is the teacher's lot not to get much credit no matter how far his students progress nor how much he has done to help them along the road. This makes no difference, however. His reward is in their success, not in his being acclaimed for it.
>
> I can nearly think of a Bible reference that makes my whole position clear: "Cast your ideas upon the waters, and two will grow where only one grew before." That doesn't sound quite scriptural after all, but the essence of good doctrine is there.

Truly, this man who almost quotes Scripture has a kindly and generous point of view, but it quite certainly is not the

median view. The great majority of opinions lay between these two classes quoted. I cite the following as more or less typical of the median view. "[It] depends upon whether [the] idea is a mere germ, or has been partly incubated, or is practically hatched. . . . If partly incubated, he has no right to use it without . . . express permission, and certainly [he] should make proper acknowledgment."

We may conclude that recognition is obligatory if the idea gotten is important as it stands, and not only as later elaborated by A and if C himself recognizes it as an important original idea. If the idea is claimed and justly claimed by C as his property because he is the originator, it would seem that A is obligated to ask permission to use it with due recognition, while it would seem that C is probably obligated to permit such use, for his original deliverance of the idea was not in confidence. If A's request to use the idea in further work is denied by C, it then seems that A should only continue in case he makes a clean breast to the public of the situation, and, one would expect, only in case he considered the friendship of C of less importance than the work in question. If both parties take the "larger courtesy" view, the general progress of science is furthered. As repeatedly indicated in replies received to the questionnaire, this issue is not an academic one. There exist incompetent workers who seek to profit by the ideas of their colleagues. There exist competent egotists who revere their own ideas so highly that they see them expressed without credit in the works of their colleagues, when in fact the views are merely those that any reasonable man working on the same problems would develop of his own accord; and still more trying, there exist workers with poor memories, who feel that ideas in truth gotten by them from earlier sources are in fact their own, and that they should be given published credit for them. In this maze no course will please everyone, but a

generous attitude, a strict sense of honesty, and a realization, as one respondent puts it, that "to fail to give credit consciously for work by another is the most contemptible form of theft" will smooth out many a difficulty.

Question 7 is:

> C and A as in 6. In a conference called by A, who is a poor mathematician, A gets and proceeds to utilize mathematical aid such as might be given by anyone having had a first-year college course in algebra.

The issue here is whether the call for assistance by the author places him under special obligation to give credit. Twenty-three per cent said the giving of credit was obligatory, 27 per cent that it was optional, and 50 per cent that it was not called for. A biologist says "Whether the aid be much or little, credit should be given to C." The view is expressed that since no money wage was given, credit is obligatory. The opposing view is more generally held, not an inconsiderable number attaching little or no importance to the fact that A called for the assistance, and these respondents reacted solely upon the basis of the intrinsic importance of the contribution. One professor states specifically that if C simply saves A the trouble of looking in a reference book, no credit should be given. A mathematician considers that "A could probably find the necessary information by himself if he took the time," and that no credit is called for. It is this professor's view that "credit for *new* ideas or *techniques* should be acknowledged." Another professor states that in the case of question 7 he would feel "no more inclined to offer credit than he would to a stenographer who copied his manuscript." As an expression of the median view I quote an engineer as saying that "It depends upon the time expended, and the help given to A." Another professor states it thus: "Not worth bothering about," and

another writes, "Credit would here be a doubtful compliment to C. A would better convey his gratitude to C in private."

Though 50 per cent said that no credit should be given in the preface, text, or footnote, it is not to be presumed that they would not express their gratitude. As in the last quotation "It would be a doubtful compliment" to acknowledge this work, because of its elementary nature. If, however, C should desire such acknowledgment, it ought to be obligatory upon A to give it. A is certainly indebted to C in some manner. A nice solution, as more than one suggested, would be "A says to C, 'I'd like to acknowledge your help.' C says to A, 'Don't mention it.'" As this issue little concerns the public, a private expression of gratitude, if deemed sufficient by C, should be the best way to handle it.

Questions 8 and 9 change the issue in a sense, making it a public matter. Question 8 is the same as question 7, except that the aid given is in calculus, and not algebra, and question 9 is the same as 7 except that the aid given is in advanced differential equations, and not algebra. In question eight 39 per cent think that no credit should be given. Twenty-seven per cent consider it optional, and 34 per cent obligatory. In question 9 the percentages are 29, 18, and 53. There are two important changes between the situation numbered 9 and that numbered 7. First, a higher grade of ability is called into play, and second, the scientific public is concerned, for it desires to know if the author himself is a master of calculus and of differential equations. The very fact that customarily the author's name is attached to an article is indicative that there is a personal interest in the author as well as in the subject matter of the article.

The consensus of opinion is that a much greater obligation to give public recognition exists in cases 8 and 9 than in case 7. This seems to be good practice, and quite irrespective of the wishes of C, the author should set himself

straight with his public, and in case the contribution of C does lie beyond his present training he should mention C by name or, if C should so prefer, refer to him as "a mathematician." It is, of course, little of a compliment to an advanced mathematician to say that he rendered help upon an elementary problem in calculus, so it may well be that C would not care to have his name mentioned. A rather striking and, I estimate, not typical reason for not crediting the contributor, given by a medical man in connection with questions 7, 8, and 9, is "Mathematics *per se* is a subsidiary method in biological science: merely a means toward an end. Once the phenomenon has been discovered, analyzed, and mechanism proven, mathematics may assist in making our knowledge orderly. Putting mathematics first, and thus crediting it with more than it can do is putting the cart before the horse." Instead of this view, should one not rather acknowledge merit wherever it may raise its head above the dull horizon of the prosaic? Does one hold a mighty general in low esteem because he is merely an agent in the cause of others? Is Robert E. Lee in the cause of Jefferson Davis a negligible character?

Questions 10 and 11 deal with the rights of a professor in the material of his lectures. In this connection I will ask you to recall the generous view of the man who almost quoted Scripture. Do scientists generally consider that they have a personal right in the matter of their general public and classroom lectures? Question 10 is:

> Student A gets an idea from a lecture given by Professor C to an elementary college class and elaborates and incorporates it in a published study.

and question 11 is the same except that the class in question is an advanced class instead of an elementary one. In the case of question 10, 48 per cent of the respondents consider

that recognition is obligatory, 19 per cent optional, and 33 per cent that it is not called for. In the case of question 11 the percentages are 56, 19, and 25. There is considerable qualification of answer. A few maintain without qualifications that the professor should be credited, but most of the comments received expressed the view that only under certain conditions was the student called upon to acknowledge the lecture contributions of the professor. In quite a real sense, the student may consider that his tuition pays for all the information he receives which is not original with the professor—in the capacity of teacher, the professor is hired to pass on the accumulated wisdom of the ages, and only in his capacity of research worker is he called upon to extend it. Accordingly, as one respondent writes, "Give no credit provided the idea is classic, already in the literature." Incidentally, scarcely any professor would want credit for an idea that was classic. Not infrequently one finds a professor credited with something of which he is not the author. He may himself be to blame for this in not having presented the matter to his classes in such a way that it was obvious he was not the originator.

From the standpoint of the public the more specific the credit given, the better. In giving credit a professor may adopt the "larger courtesy" and in asking for it, maintain the "bigger generosity," but the public does not desire either. It wants the unadulterated facts. In giving credit the student should first search for the truth—he ordinarily makes more errors due to ignorance than to intention—and second, he should realize that published acknowledgment is honey to the young professor, and not exactly vinegar to the older one. He should be scrupulous in giving it where it is due. It is to be noticed from the responses to questions 10 and 11 that little distinction is made in this matter between elementary and advanced lecture material.

Questions 12 and 13 concern themselves with the rights for recognition of the professor supervising the work of a student. All degrees of intimacy of supervision exist. I have heard from a very creditable source of one case at a certain university in which the professor under whom, according to the records of the registrar's office, the student did his research work for the Ph.D. degree did not, at the time of the final examination, know the subject of the research, or that he was the student's adviser. I would call this minimal supervision. On the other hand, as stated by one of my respondents, there are cases "where most of the work [has been] done by us [major professors], thesis, method, evaluation of data, and finally grammar." Between these extremes lie the bulk of theses, all alike in that on the books of the registrar and in the minds of colleagues, one certain professor is in a sense sponsor for the work. Ordinarily the professor's part is so material that there is no issue as to whether credit should be given. Questions 12 and 13 present less certain cases. Question 12 is:

> A is writing a dissertation under the direction of C. Certain techniques given in the literature are called to A's attention by C. These are adopted by A, but the problem and all novel techniques have originated with A.

Thirty-eight per cent say that credit should be given, 35 per cent that it is optional, and 37 per cent that it should not be given. Thus the median reply is that it is optional. I wonder how the question would have been answered, had it read "A is writing a dissertation under the direction of B. Certain techniques given in the literature are called to A's attention by a third party, C. These are adopted by A, but the problem and all novel techniques have originated with A." Surely the question as put indicates that the major professor has done just about as little, and that not original, as could well be, and still 38 per cent consider it obligatory

that credit be given. Would these 38 per cent ask that recognition be made if the plain unvarnished truth constituted the recognition? The major professor himself could hardly desire that that be cited as his contribution. He would surely prefer that the matter be not mentioned. Let us see some of the reasons given for saying that credit should be given.

A social scientist answers question 12 with "No," but adds, "I think a student who has had the guidance of a professor in connection with a dissertation should always acknowledge (in the preface) the receipt of assistance from the professor." This view, in its demand for recognition of the major professor, is certainly not far from the median view. It seems difficult to justify as a matter of ethics. Perhaps it is just a little white lie. Explain it as one will, we should accept the fact at its face value, and advise students under all conditions to acknowledge their major professors in their prefaces—failure to say something kind just is-not-done in polite society, no matter how little the professor has contributed. A second social scientist says that credit should be given, and writes "Owing to the peculiar duty of the professor toward his candidate, one would be prompted to say 'No' here, except that it is important to teach the student to lean over backwards in this matter of acknowledging credit." The particular professor just quoted did very consistently throughout the entire questionnaire, as judged by the median opinion, lean over backwards in calling for recognition of help received, but his argument should not justify the responses of others who do not so lean. Their most common observation was "[Giving credit] is a matter of courtesy."

Question 13 is:

> A as in 12. A feels "stimulated and inspired" by C, but cannot point to important specific techniques for which C is responsible.

Thirty-one per cent said the recognition was obligatory, 50 per cent that it was optional, and 19 per cent that it was not called for. The median judgment is that the matter is optional with the author, but the medial feeling as judged by comments, is that the author should exercise his option by giving credit to the major professor. The question as put is a poor one, being capable of considerable difference in interpretation as to the contribution of the professor, but all of the reasons applying to situation 12 for giving credit seem to be present here, plus certain very pertinent added reasons.

The last question reverses the issue, and raises the question as to when the professor should give credit to one of his students. The question is:

> Professor A gets an idea from a class paper written by Student C, and elaborates and incorporates it in a published study.

No less than 71 per cent said that credit should be given. Nine per cent said that it is optional, and 20 per cent that it should not be given.

No other question in the questionnaire seemed to suggest so many personal and generally unpleasant experiences as this one. We are certainly dealing with an issue which has been the occasion of much heartburn. A young professor writes "Rumor has reached me of numerous instances where students have worked hard at projects, only to have instructors 'cop' the major portion of their projects. It seems convenient for A to develop amnesia for sources." This is a very unkind remark. It has not emanated from a student who has failed to pass a professor's course, but comes from a professor, and it does not stand alone. Another professor of advanced years and long experience writes, "I have heard, however, of some flagrant cases where the 'prof' has pirated and published without acknowledgment mighty good stuff

done by various students and assistants." A head of a department writes, "I know that students have not received appropriate credit for ideas presented by them." An economist writes, "American university professors have been guilty of grave injustice in my opinion in failing to recognize the moral responsibility they have to students in such cases as [questions] 2 and 3. Not only this, but I have professional books on my shelf in which elaborate footnotes appear, taken almost wholly from theses by students, which theses were deliberately assigned by the professors to get the facts needed for such footnotes. There is one nationally known professor at————————who, I have been informed more than once, practically requires in advance of his students, even Ph.D. candidates, that any findings shall be his for his use in books. This, it seems to me, is an outrage, but the student is generally helpless in such cases." The average age, rank, experience, and salary of the professors just quoted is, I should judge, 55 years of age, head of a department, 25 years experience, and salary in the upper 5 per cent. No matter how one would react to these views if presented in the college daily, sponsored as they are, they must be treated seriously. A suggestion made by a young professor as to why A does not give credit to C is to be found in the following quotation: "Reluctance to credit C may be due, in some cases, to a correct suspicion (which crediting C might cause others to share) that A might better have left the elaboration of the idea to C. If A suggests this to C, and for various reasons C does not do so, or is not sufficiently able or experienced to do so, then it is proper for A to go ahead. In such a case he should gladly credit C." In the last five quotations the most unkind observations have been made by the two youngest professors, but the other three have been no less forceful in expressing the belief that due credit frequently has not been given to students. I have selected

the quotations just given because of the type of view expressed.

I will now select just as strong statements of the opposing view as I can find in my questionnaire answers. A social scientist considers that credit to the student need not be given, and says, "Have you read Kipling's poem 'Even As You and I'? In it he suggests whoever did anything worthwhile got a great many of his ideas from others. I think a good conscience that is not psychopathic is about the best guide." A biologist says, "The students in the questions depend on the professor, either directly or indirectly, for the ideas they acquire, hence do not need specific acknowledgment. If the student shows promise, the professor would do well to encourage him to develop his ideas for himself. If he is not interested, the professor may take it over without violating professional courtesy or ethics." A medical man writes that the "chances [are that] C lacks originality. [The] professor only refreshes [his] memory, and elaborates on [his] pre-existing knowledge, belief, intuition, etc., just as from general reading. There must be a limit to giving credit to everyone who has written something which may be useful to a reader. . . ." [1] An engineer considers the giving of credit optional and writes, "The original idea was probably presented by A to the class. C simply calls A's attention to the matter. In most cases, the elaboration and presentation are the major part of the work."

Many intermediate views could be cited. Here are a few of the most typical. A mathematician writes, "Perhaps

[1] The rest of the quotation from this professor is, ". . . but in my opinion credit is due to those intimately or personally associated in the prosecution of problems of research, regardless of the nature of the assistance or cooperation—all this promotes an atmosphere of fairness and sincerity in the study of problems, useful criticism, and gives inspiration and zeal to younger men. This has been my experience with many associates and students. It places the responsibility where it belongs, also makes the professor less fallible and authoritative."

[the] student should be taken into full confidence and given a chance to collaborate in the study and share the honor of joint publication. Discouraging otherwise." A physical scientist advises giving credit "if [the] student had [the] idea too." Another from the social science field says that credit is obligatory if the "student really consciously had an idea" but that it is not called for if "the professor and not the student *got* the idea. Except for human contacts, we would no doubt be relatively deficient in ideas, and if each were religiously traced to its source, a large part of scientific literature would be just that. I presume it has happened that one got a very fruitful idea by misunderstanding someone else." A biologist states that the answer to this question is "yes or no, depending on whether C recognized the value of the contribution." These quotations are only a few of the same general tenor.

Quite a number of quotations could be given, the tenor of which is that credit should be given, not so much as an ethical right, as a means of encouraging and stimulating C to further work. A historian writes, "If the policy of giving due credit to the students for any ideas their class papers may contain were generally followed, students might feel it more worth while to put real thought into their work." A biologist writes, "Credit of this sort will mean much more to the younger man in many ways, and may stimulate his interest to an unusual degree. This is a big part of the teacher's business."

In spite of wide diversity of view there is an undoubted central tendency shown. If a student present an idea that is new as judged by the wisdom of the professor, and if he sense something of the import of this idea, then before the professor uses it he should endeavor to get the student to develop it fully either independently or as part of a joint study. If the student does not care to do this, the professor

is at liberty to use it himself, but only with due acknowledgment. In none of the replies was it considered that the student had an inalienable right to the idea to the point that the professor should not use it without the permission of the student after the student disclaimed any intention to develop it. Ideas gotten from papers written by students but not in the minds of the students writing the papers require no acknowledgment.

The question just discussed is the last one of the questionnaire. If we now try to formulate a general rule governing all cases, we shall find much need for qualifying and additional clauses. It, however, is worth the attempt. A few such generalizations are volunteered by the respondents. Here is one:

> The responsibility of the senior toward the junior is much greater than vice versa. Obviously, the credit means more.
>
> In general, credit and responsibility go together. The origin of an idea is a fact which should be recorded with scientific accuracy. It is good ethics (and also good policy) to make the record complete.

Here is another: "The man who acknowledges too much has lost nothing; the one who acknowledges too little may lose a friend." Another writes: "In general, I think it best to be generous in giving credit to others, especially to those not so far up the ladder." Another says: "Life is too short for any but an unselfish attitude on any of these problems." Another writes: "Giving credit is a simple courtesy which is so cheap that anyone can afford to be generous. Furthermore I believe that this acknowledgment enhances rather than detracts from the author's kudos. . . . A failure to do so indicates a lack of generosity, even a meanness that is most objectionable." Another says the "basis of ethical duty to give credit to another is 'a special indebtedness to another for an idea that has not been so published as to be-

come common knowledge.' . . . Giving credit upon other grounds is a matter of good manners, not of ethical duty." An unmarried man observes that "A little extra appreciation may win or hold the good will of employees, friends, wives, and other enemies." The shortest summary found is "Make acknowledgment in case significant help is rendered." This statement is rather too abridged to meet all the issues. A graduate student formulated the following: "Credit should be given in every case where the material cited would add to the prestige of its originator." This is an excellent formulation, but as it does not distinguish between pay and gratuitous help, and does not keep the public in mind as one party, it does not meet the median view. I would propose the following: "*First, an author should give published credit whenever the contributor has stepped outside of his regular line of duty as it is related to the author, and when such recognition will add to the prestige of the contributor. Second: a student, when writing a thesis, should follow the usual courtesy of expressing appreciation for the help rendered by the major professor, whether he has gone beyond what would reasonably be expected of him or not. Third: an author should make such recognition that his readers will not credit him with phases of work appearing under his name beyond his present training or capacity to produce.*" This formulation is intended to include all cases from the exceptionally able, paid clerk or technical assistant, to the patient relative or the thought-provoking colleague or student.

With this particular questionnaire in mind, let us attempt to make a few generalizations about questionnaires. It was mentioned that 204 scientists were solicited, and that 155 replied, though a few of the replies did not contain useful information. An examination of these, together with an examination of the names of those not replying, suggests that there is a greater likelihood of not getting a useful answer

if (a) the recipient is sick, (b) on sabbatical leave, (c) not well known to the sender of the questionnaire, (d) holds a minor position of one year tenure. No other tendencies were discernible. It does not seem that any one of these should select people prone to be non-typical in a certain given manner in their attitude upon the matter of granting credit. It is accordingly believed that the sampling is a fair sample of those polled, and it is also believed that those polled are a fair sampling of scientists in the United States. At best this important matter remains a matter of belief, as it cannot be proven without an investigation much more extensive than the questionnaire itself; and such is characteristically the case with polls by questionnaires, for 100 per cent, or even 75 per cent as here, of return is exceptional. This lack of demonstrable fairness in the sample is probably the greatest shortcoming of the questionnaire. The user of the questionnaire should never lose sight of it.

A second intrinsic difficulty not unrelated to this first is an ethical one. What right has the sender to ask for the time and honest reply of the recipient? And if he has no right, what reason is there to think that he will get an honest reply made after due reflection? One of the respondents to my questionnaire who sent in an incomplete paper wrote "[questionnaires] are sent to busy men who must either take time off to hunt up data or think it over or else answer superficially. This is so involved that I find it necessary to go over it several times before getting the gist of it." Two or three others expressed similar sentiments. In these cases I wonder if the trouble was so much with the complexity of my questionnaire as it was that I had no claim upon the time and effort of the recipients, at least none that appealed to them. I would put this as the second great shortcoming of the questionnaire. To meet it, so far as possible, I think every questionnaire investigation should be so planned that a benefit

will accrue to the parties answering; perhaps a promise of the results of the study is sufficient. Less than this can hardly be offered. One respondent wrote that duplicate copies of every questionnaire should be sent out, for if the matter is worth the time of the recipient, it would be of such value that he would like to keep a copy for his files. It should not be deemed sufficient to say to the recipient, "You will be helping the cause of science." As to that, each should be the judge for himself.

We may lay it down as axiomatic that every human being connected with a study should get some satisfaction from it. The recipient of a questionnaire is a human being, not a machine to answer questions. He is the intermediary between the author's hope and its realization. To constitute an organic work, the middle must be vitalized as well as the two extremes. The leaves of a tree bribe the trunk to send up water and food from the roots, by offering good fixed nitrogen in return. Under these conditions the trunk accepts the task and becomes an integral living part in harmony and in willing cooperation with the roots and with the leaves.

The third shortcoming of the questionnaire method lies in the difficulty of making from *a priori* considerations a fair selection of experts. As previously mentioned, this need not be an insurmountable difficulty, for lesser experts can be used to select greater ones.

The questionnaire is sometimes used in a field where the experimental method could be employed. In such a case it is clearly a makeshift, and indefensible except where time and cost necessitate it. Such was the case when the oral questionnaire method was used to draw up the manual of trade specifications used by the United States Army during the war. The judgments of a few officers stating the qualifications needed in, say, an airplane hangar attendant, could

be quickly gotten, while an actual qualitative and quantitative survey of the duties of such attendants would have been a long undertaking.

We may conclude this investigation of the value of the questionnaire by saying that unless and until experimental science relieves us of the need of human judgments, or removes from our minds an interest in unique events, this wayward child of science, feeble as it is, will remain an indispensable helper. It will thus be always needed, and we can but hope that it will curb its intrusive disposition and mend its unseemly ways.

CHAPTER III

WHAT PURPOSES ARE SERVED BY SCALES OF MENTAL MEASUREMENT?

THE units in which natural phenomena have been measured have been intimately connected with progress in science. It is not until we have units of measurement that we have definite measures of progress, but of course there must have been progress before this, or the units themselves would not have been derived. Undoubtedly units of weight, time, distance, temperature, volume, etc., grew out of felt social needs. The early developments of units in all of these fields antedates historical time, but we can imagine what might have taken place.

A Neanderthal man in hunting had wandered three sunsettings from home. With his mighty club he killed a wild boar, and gorged himself, but, even so, the major portion of the carcass was unconsumed. Fearing that he would be killed if he should fall asleep with all that wealth beside him, he shouldered it and set out and shortly came to a village of friendly tribesmen. Now property rights were well established in those days. It was agreed that if a stout tribesman entered a village with a dead boar over his left shoulder and a club in his right hand, the boar was his property. Our man was a cunning fellow. He thought, "This is a wonderful carcass, and I cannot take another bite, and I am sleepy and cannot stagger for another three days, so I cannot take it home now—and I remember having found that even a beautiful carcass loses its savor after three days' ripening in the sun; so I must negotiate a long-time contract."

Seeing a wife and many children gathered about a tribesman who eyed his trophy hungrily, he said, "My compatriot, you and your family need sustenance, and need it now. I shall need it three sunrises hence. I give you this choice pork if you give me back the same amount of prime fat and protein in three days. How now, is it a bargain?" "It is." Our sleepy hunter has not lost his wit. He says, "Come, red-haired urchin, and lift this portion for me." The boy struggles and can barely budge it, so the thing is weighed. Our hero pulls off a lock of the urchin's red hair and ties it in his own as a means of identifying his weighing scale three days hence, when he hopes to get back an equally heavy portion of a newer kill. The headman of the clan bears witness to the transaction, and almost in a jiffy snores proclaim that the deal is closed.

This contract in futures, in addition to principles of credit and underwriting, involves physical measures of time and weight. The time unit is fairly exact. Unless our man sleeps the sun around and fails to notice a sunrise, he can each day chew a mark on his club and have documentary record when the three days are up. His weighing scales lack something in precision. They are in fact not much better than our own scales of mental measurement today, but they are a beginning for the science of dynamics. Whence came the background that led our Neanderthal scientist to conceive of weighing an article? Did he know that two properties, mass and force of gravity, were involved in the weighing feature of his program? No, he simply knew that the thing done was beneficial to him. He was going to want, in the future, fresh meat and lots of it, and the urchin could lift. In other words, he has a measuring device of something, he cares not much what, but it is a something which it is to his advantage to measure.

How close the parallel to our mental measurements to-

day! Our mental tests measure something, we may or may not much care what, but it is something which it is to our advantage to measure, for it augments our knowledge of what people can be counted upon to do in the future. The measuring device as a measure of something that it is desirable to measure comes first, and what it is a measure of comes second. The amount the red-haired urchin can lift depends upon the mass, the slipperiness of the hold he gets, his immediate effort and state of being, etc. In the first crude usage of this weighing scale, which of these are relevant and which are not is not thought of. All that is considered is that on the whole it is a pretty good scale. Homo-Neanderthal may have been as tickled with his weighing scale as are my colleagues when they devise a new psychological test. The use of the scale is the quickest means of discovering its shortcomings. Neanderthal's great-great-grandchild may stipulate that the lifting is to be by grasping the hind legs of the animal, and no slippery hold is to be permitted.

In the matter of mental measurements we are in that stage where we know that we are measuring something that it is valuable to measure. Our measures work in giving us information that is good to have. Those standing high on the Army Alpha do, on the whole, make better officers than those standing low. Those standing fairly low do make better clerks in positions involving monotony and light behind-the-counter chit-chat than those standing high. Those scoring high on a number of our school achievement tests do succeed better in the work of higher grades than those standing low, etc., almost without end.

My discussion therefore starts with measuring devices that are valuable, and it does not need to start with any hypothesis that we know just what the valuable thing is that we are measuring. I will now venture an answer to a question which concerns all psychologists, "In what units

shall we measure intelligence and achievement?" The answer is "In such units as will be of most value to us in doing the work of the world." This answer of course requires further elaboration. If some philosopher argues that intelligence is a mental trait that shows itself in some carefully defined manner, I shall not take his statement as evidence that I should measure this thing. Only in case his definition involves in it the idea of greater utility in practical problems than some other definition, would I feel inclined toward it, and then the test would be whether this greater utility was actually present or not. For the Neanderthal man to have a measure of weight that was independent of slipperiness would be an advance. It would better serve the needs of commerce of that day, just as an international standardized currency would better serve the needs of commerce today.

I am not arguing for a unit that serves a commercial need as opposed to one that serves a scientific one. I am merely advocating a unit that serves a need of some demonstrable sort as opposed to one that meets a hypothetical mental standard. The mathematician can propose and develop most beautifully innumerable different units of time, but probably one only of them will well meet the objective needs that the astronomer finds inherent in his data. As a problem in abstract thought the development of these different units of time is highly profitable; in fact, not uncommonly suggestive of issues to be experimentally investigated. But, so far as adoption is concerned, that one only is to be adopted that most completely meets the temporal conditions imposed by life. The time relationships of life are generally imposed by a power beyond ourselves, by God, by nature, by necessity, or by whatever name you choose. Now as indicated in my last chapter, some of the mental relationships of life are not imposed by so invariable and just a hand. They are, however, none the less imposed, though by society itself, and

it is just as necessary that our mental scales meet the needs that society imposes as that our physical scales meet the needs given by the laws of the physical universe.

What are these needs imposed by society in the field of mental phenomena? Clearly, one need is that a term used be used with the meaning that society has already attached to it. If we define honesty in some manner, for example, as the ability measured by a certain specific test, not at all in keeping with the already existent consensus of opinion as to its meaning, then the units of the test are improperly called units of honesty. They may be valuable units of some mental capacity—that remains to be proven, but the value does not partake of the already existing value known to be attached to the concept honesty. Even if the order of individuals as given by the scores on the test called an honesty test agreed with the order of the same individuals judged for honesty by their acquaintances, still the test units might not be appropriate, for this would only affirm that the two rank orders were similar, and it would not state that the quantitative differences between scores were alike. Suppose the test places three individuals, A, B, and C, in this order, and such that the number of units from B to C is twice that from A to B, and suppose the judgment of acquaintances places the individuals in the same order A–B–C, but such that the distance from B to C is the same as that from A to B. Then, although the test preserves the rank order of the judgments, its units of measurement are inappropriate because they do not preserve the quantitative relationship of the criterion, which is honesty as sensed by competent acquaintances.

Another source of trouble may be mentioned. It is the location of the zero point of measurement. In some mental investigations estimates of ability from zero are frequently made, while in others they are seldom if ever employed.

In the case of honesty we may judge of the distance between individuals without in any instance judging of an individual's honesty above zero. We can then lay down two important rules for a test measuring honesty. First, the function measured by the test should be the same as that judged to be honesty by competent people in general, and, second, the differences between test scores of individuals should be proportional to the differences in honesty of the same individuals as sensed by competent people. A third condition, that dealing with the zero point, may or may not be of great importance. I will call any scale meeting the first two conditions a sensed difference scale.

Since people do talk about honesty, would it not be a fine thing if a reliable objective scale for the measurement of it existed, thus making it possible, by giving a test, to obtain fairly accurate information upon a trait which is now much less accurately revealed by such judgments of acquaintances as it is ordinarily possible to collect? The merit of such a measure would lie first in measuring just the same trait and in proportional units that judgments now evaluate under the same term, and, second, in doing it more accurately and expeditiously than is now possible by means of judgments. Such a scale would serve in the work of the world. It would simply make it possible to do better that which we endeavor to do anyway. We should not have to justify the objective of such a measure, for the objective is already socially sanctioned. A scale actually established to be of this sort needs no defense.

Of no other kind of scale can the same be said. No other kind of scale is, strictly speaking, entitled to be named by a word whose meaning is already established by social usage. Let us consider such a scale. We will call it a scale of "intellability." Intellability is not an English word already having a meaning, so I am treading upon no social

preserves in using the term. I can define it as the ability measured by my test, and thus I have a very objective definition, though not a definition with meaning to my hearers as yet. In fact, the term is like any other technical term coined to describe a new phenomenon.

The word "chromosome" is such a term. Only those becoming acquainted with certain life phenomena,—new as judged by older standards,—understand it, and profit thereby in their understanding of life. An entire set of relationships and meanings have been built up around the word. The important thing to note is that all these elements of value have been built up. Not one, without demonstration, could be adopted from earlier social meanings or habits of mind. This is a scientific procedure, and I shall shortly speak more of mental units, which cut loose completely from social concepts as represented by existing meanings of words. I wish now to point out that intellability has not a leg to stand on except as it grows its own. This term and this concept require justification, whereas honesty, meaning thereby present social consensus of opinion, requires no defense.

The proof that intellability should have a place in the sun is no small undertaking. As a mental trait its meaning is to be related to existing concepts of other mental traits which are not defined in terms of test scores, and it must be shown to have some unique property which is important in the lives of men. From the date of its birth it is on the defensive just as were such terms as "chromosome," "gamete," "ion," "proton," etc. These words have survived, but recall how each describes a characteristic of nature not earlier described, how each is involved in and helps to explain relationships not earlier understood. The same rigorous test will determine if intellability is to live. Now I believe that such new mintings are going to take place, and

that soon. They may even in time relegate to the scrap heap some of our fondest concepts such as intelligence, innate mental ability, and the still common terms of faculty psychology, just as "effluvium," "humour," and the "four elements, earth, air, fire and water" have been. These are no longer of social value, having been displaced not by new words meaning the same things, but by words descriptive of entirely new structures of thought. The periodic table of elements tells a story so much more interesting and forward looking than any bounded by "earth, air, fire and water" that these latter words as boundaries of matter have been allowed to crumble.

At present I do not wish to defend intellability or other new mental unit. Rather I want to indicate the benefits of, and the method of deriving, mental units which take their cues from present concepts. I have made an attempt to do this with results that seem very promising. The function I have dealt with is achievement in the elementary school. The ultimate source of authority upon what constitutes achievement in the elementary school is the consensus of opinion of American school men and women.

Some have argued that each of the questions of an elementary school achievement test should be accepted by such a consensus as appropriate, and should be so weighted one to another in the aggregate test score that the relative importance of the various parts of the test is in harmony with the consensus, and finally, no material elements should be omitted that the consensus would include. Now this is not sound doctrine—the consensus passes judgment on the grand total outcome. It asserts that the distinctions drawn by the test scores between children are or are not sound. The consensus is ultimate in the judging of people, not of test items. The preliminary stages in the selection of items for a test may depend upon the judgment of the individual or

group devising the test but no excellence in judgments at this stage is a proof of validity of the test. Ultimately and authoritatively, this depends upon the agreement between the measures of school achievement of children as determined by the test scores and as determined by the consensus.

An attempt was made when developing the New Stanford Achievement Test to make the units of the test substantially proportional to differences in ability as sensed by school teachers. In so far as this attempt was successful it aids in revealing other facts of mental development. For example, if the sensed difference between the mean scores of third- and fourth-grade pupils is called 1, that between second- and third-grade pupils is 2, and that between seventh- and eighth-grade pupils is 2/3.[1]

In short, teachers do sense the achievement differences between grades as progressively less as we ascend the grade ladder. This is not at all surprising. These lesser differences in the upper grades are due to demotions and double promotions, so that in the later grades groups are more homogeneous, and closer together in the teachers' judgments. The relationship just mentioned and others revealed when sensed difference units are employed contribute to a sound understanding of individual differences.

If I have shown that units proportional to sensed difference units have a peculiar claim to validity in connection with achievement, it must be obvious that they have no less a claim in connection with honesty, originality, or other mental function.

What are appropriate units in which to measure native intelligence? I speak of native intelligence, and not intelligence, for this latter term means to some intelligence as

[1] These facts may be deduced from Table E of Kelley, Truman L., *The Influence of Nurture upon Native Differences*, 1926, together with the published norms on the original Stanford Achievement Test, 1926.

determined by original nature, and to others the outcome of nurture, achievement, and original nature. I do not know what the consensus of opinion is as to the meaning of intelligence, but if I speak of native intelligence, I feel fairly sure that that fraction of later life acumen is implied that has not been determined by the specific nurture of the individual. This very statement supposes that there is such a fraction. We cannot tell until we attempt to measure it.

Now if one portion of later life capacity is due to one cause, say, original nature, then the rest must be due to some other, single or complex. Thus if we have a scale of native mental ability, we should also have a scale of something else. I believe it will be in harmony with general opinion to call this something else a nurture scale. Let us then first consider the determination of this second scale. If such a scale can be built up it will have value, very great value, upon its own account, and it will also have great value in showing what is not to be included in the native intelligence scale.

One aspect of the concept, native intelligence, is that it comes increasingly into manifestation with growth. Though present in germ at birth, it is not then full-fledged. Just as the physical changes that occur at puberty are considered matters of original nature, and not of nurture, even though they take place fourteen years after the gametic union, so mental phenomena may and probably do occur at late as well as early periods in life, which are in fact but the flowering of original nature. Thus one of the important characteristics of native intelligence is this phenomenon of delayed expression, or, if you will, is the phenomenon of growth.

On the other hand, nurture is timeless in the sense that the opportunity to learn a certain thing and the effort to teach it may occur at any age; that reading is not taught with equal emphasis from birth to adulthood is surely due

to limitations as to feasibility imposed by original nature, not due to lack of environmental agencies willing to teach it at any age. If, therefore, we desire to build a nurture scale, we must rule out of it all of those things that are highly correlated with chronological age from birth to adulthood. To select the elements which will enter into our measure of nurture we must find elements which do not correlate with chronological age, or, if there are none such, we must consider that a test element is partly nurture and partly nature in proportion to its tendency not to correlate with chronological age to its tendency to so correlate.

I can make my meaning clear by an illustration. I give as a completion exercise the following: "Little Miss Muffet sat on a tuffet, eating . . ." In giving this exercise to groups of one-year-olds, two-year-olds, etc., up to adults, we should find varying numbers of correct responses. None of the one-year-olds will get it, some of the two-year-olds, etc., but at no age shall we get 100 per cent correct response. Immediately we see that the Little Miss Muffet growth curve is not like the sexual maturity growth curve. A certain percentage of boys are sexually mature at 12, a larger percentage at 13, etc., until all are mature at 20. The ability to say the words, "Little Miss Muffet sat on a tuffet" is conditioned upon the maturity of vocal organs and cerebral connections which are innate and which develop in an orderly manner with increase in age, but the proper completion of this exercise with the words "curds and whey" depends upon something not so conditioned, and not in all cases happening in an orderly manner with increase in age. I doubt not that there are American adults who do not know of Miss Muffet's fondness for curds and whey. Because of people such as these, the correlation of ability to complete the exercise and age will drop down below what it would be if the completion were merely a matter of native

intelligence. This shortening or dropping down of the correlation is a measure of the extent to which the exercise is a function of nurture.

It only remains to find just the proper procedure to utilize this fact in determining just what portion of an outcome is due to nurture and what portion to nature. Such information can be utilized in selecting items as nearly 100 per cent nurture as possible to build up a nurture scale. If we have such a scale then we shall be able to clearly see what nurture does. It is manifestly unfair to judge of its rôle by our present scales of intelligence, because one and all have been built up with the idea of securing as high correlation with chronological age as possible, and not with the idea of securing as low correlation as possible.

Compare if you will nature to height, and nurture to weight. Suppose that I have a more or less effective set of measures of height—sticks gathered at the seaside, of different lengths. I gathered them because they were of different lengths, paying no attention to other features. Now of course it will happen that they will also be of different weights, but they may be totally unordered for weight, and totally inadequate as a weight scale. If I persist in using my set of sticks to measure all new phenomena, I will get the impression that the only essential characteristic of such phenomena is height, and come to believe that there is no other dimension. Has not something of this sort happened in the case of those who attribute most of mental difference to difference in native intelligence? These people have had at their hand a measuring device, a Binet test or any other mental test that you wish to name, the very elements of which were chosen because of high, not low, correlation with chronological age.

The nurture advocates are largely to blame for this situation. They have not developed a nurture scale. They cannot

effectively combat positions based upon measured individual differences by beating the air with verbal observations that there are other differences. They must demonstrate that there are, by measures based upon objective tests, or upon demonstrable and verifiable consensuses of opinion. Generally speaking, they have done neither, and in their criticism and ridicule of the advocates of the all importance of original nature, they have not uncommonly included all psychological measurement itself as an object for their attack. But intelligence and achievement measurement seems to have survived these valorous assaults, and I now fear that unless the nurture proponents hurry up, the measurement people will seize their Holy of Holies and measure nurture itself for them.

I have said that the devisers of mental tests have chosen test items on the basis of their correlation with chronological age. Cannot an even more accurate method be followed to build up a native intelligence scale? What are the properties of such a scale? It would correlate with chronological age to a certain high amount, and any inclusion of an item largely nurture would lower the correlation. Second, and this is the new criterion that I propose: The correlation between an individual's score at one age and the same individual's score at a later age should be perfect, or as nearly perfect as the chance factors in the test permit. Clearly, this would be so if the test is a native intelligence test, for one's native intelligence is given at birth, only requiring growth for it to blossom forth in its different aspects. Now growth results in a difference in age means, and the correlation method in which deviations of individuals are measured from their own age means would completely allow for this phenomenon of growth. Therefore the deviation from an age mean at one age and that at another are merely due to innate individual differences, provided of course the scale is, as supposed, a scale of innate mental ability.

I have used this second principle, not, as I here advocate, in the selection of items of a native intelligence test, but in testing units of measurement of one, probably the best, intelligence test—the Stanford Binet. I am convinced, and I am sure the author of the test is also, that the items of the Stanford Binet fall considerably short of being 100 per cent native intelligence items, uncontaminated by nurture. Nevertheless, it has seemed worth while to assume that they were such items, and then to see if the units of measurement were as good as they could be. Discarding now the consensus of opinion standard as to what is meant by native intelligence, and by units of it—in other words, by equal differences in native intelligence—let us adopt a criterion independent of human judgment. As there is no clear consensus of opinion as to the meaning of native intelligence, we are not taking any violent liberties with usage if we ascribe to it the following properties.

(1) Except for growth, it does not change in the individual as age changes. (2) Natural or correct units for its measurement are those which reveal this fact. As a corollary to this second statement, we can say that if the units of measurement are not these natural units, then the correlation when differences are taken from age means of an individual at one age and the same individual at a later age will be less than if measurements are in these natural units.

This immediately suggests the experimental device of finding the natural units of measurement by so determining test units that the correlation between early and late scores is a maximum. This has been done with an outcome that I will shortly mention. Also the correlation between sibs has been investigated. As there is an innate correlation dating back to the germ cells, between children of the same parents, we can use the same argument and say that the

natural unit of measurement will be that which will make this correlation a maximum.

In passing, let us note that one characteristic of the natural unit of measurement in the case of a nurture scale would be that which would make these same correlations minimal. The argument is straightforward, but the application of it meets difficulties because, as mentioned, our scales do not come to us, probably cannot come to us as pure nature and pure nurture scales.

The first population upon which I can report experimental findings consists of 521 pairs of sibs, tested with the Stanford Binet. Of these 262 were California children, for whom data were kindly supplied by Dr. Lewis M. Terman. The data for the remaining 259 were given me by the late Dr. Bird T. Baldwin, from Iowa Child Welfare Station files. All of the children were considered normal. An examination of both of these groups suggests that the populations were not quite typical, as there is an excess number, perhaps 5 per cent, of both low- and high-grade cases. It was ascertained after considerable labor that the effect of these cases upon the issue involved was small, so all cases have been included in the results that I shall report.

The correlation between sibs for this population of 521, taking as the raw scores deviations from smoothed age means determined from these same data, and then using the Pearson product moment formula, was .62. Of course, in getting this a year's mental growth, whether from 4 to 5, from 5 to 6, or from any other age to the year above, is in every case called *one*. Now it is known that at the upper end of the scale, say from mental age 15 to mental age 16, a greater mental difference is indicated by the difference of one mental year than actually takes place in median children as they develop from median 15-year-olds to median 16-year-olds. This is a disconcerting element for our

present purpose. Though I have not attempted to allow for it, I would say that as the bulk of my data involved mental ages below 14 years, this factor cannot be expected to seriously cloud the general relationship for ages up to 15.

The mental span from 0 to 4 years was given the literal symbol a; that from 4 to 5 was called b, that from 5 to 6 was called c, etc. Thus a child of mental age $5\frac{1}{2}$ was given a score $a + b + .5c$. A child of mental age $10\frac{1}{4}$ was given the score $a + b + c + d + e + f + g + .25h$, and similarly for other mental ages. As all the children were above mental age 4, and also above this chronological age, the first literal element, a, dropped out of the correlation computation. It was endeavored to ascertain the values that should be assigned to b, c, d, etc., that would make the correlation a maximum.

A further statistical condition was imposed to simplify the work and to prevent chance fluctuations from unduly affecting the values. This was the requirement that values of b, c, d, etc., should lie on a single curve capable of representation by a second-degree parabola. Even the requirement that they should lie on a straight line would have been sufficient to tell whether the early mental years were greater or less than the later, so the condition that these units lie on a second-degree parabola is quite general enough to yield as great refinement as our present state of knowledge can profit by.

Let me summarize the argument underlying this procedure. The correlation between sibs in the case of measures of ability not influenced by nurture is something that dates from the birth of the younger sib, and since it is given once and for all this correlation will not change throughout the life of the couple, if allowance is made for growth. The measure of this correlation will be weakened by any improper units of measurement. If therefore we start with

units as given by a test, and alter them so that a higher correlation between sibs is obtained, we shall be altering them in the direction of natural units of native ability in the function tested. The same argument holds so far as direction of change of unit, if the measure worked with is in part a measure of something other than native ability, something such as nurture, not having a high invariable correlation as between sib and sib.[1]

Since brothers and sisters 3 to 4 years apart in age have different school teachers, a common nurture influence between sib and sib showing high correlation can hardly exist in the matter of school subjects. In other fields not limited to family things (thus perhaps religion is excluded), there is still less reason to believe that sibs have a common nurture. The Stanford Binet is considered to be a test largely influenced by native ability and, to a lesser extent, by nurture. Then we investigate the Stanford Binet units and so alter them that the correlation between sibs is a maximum. In the particular sample worked with, this treatment raised the correlation from .62 to .72. I will express the results as they bear upon units in the same terms as I did those dealing with sensed differences. We will remember that we found that if the difference between the third- and fourth-grade means in general scholastic achievement is called 1, that the difference of seventh- and eighth-grade means is then to be called .67, in order that equal numerical differences be sensed as equal. Here in the case of the Stanford Binet it is found that if the distance between the mean Stanford Binet scores of third- and fourth-grade children is called 1, the distance between the mean Stanford Binet scores of seventh- and eighth-grade children is to be called .50, in order that the correlation between sibs shall be maximal. These

[1] A note in substantiation of this is given in Kelley, *Scientific Method*, edition of 1929, Appendix A.

two results are not identical, but they are of the same general order. There is no logical reason why they should bear any resemblance to each other, as the tests are different, the subjects tested are different, and, most important, the purpose of the investigations are entirely different. The one investigation aimed to secure units equal in the sensed difference meaning, and the other to secure units which would not weaken any intrinsic correlation which might exist between sibs. That the outcomes are similar is both surprising and gratifying. The prospect is that here in the mental field there is no great discrepancy between sensed and natural units of measurement. We know, according to Weber's law, that equal sensed differences in height are not equal differences as the physicist or the man with the measuring stick counts them. For such judgments of difference in height as one commonly needs to make, the physical measure of difference is not a bad unit to employ. It is with little difficulty incorporated into our scale of meanings, and no separate sensed difference scale of length is found in common usage. The prospects are that the same thing will be found to hold in the mental field. Though the sensed difference scale is in terms of meaning, it may yield to another, but slightly different one, in terms of which the phenomena of mental life can be more accurately and invariably expressed.

The argument underlying the procedure just described was based on the assumption that the Stanford Binet was a test of native ability and not of nurture. If so-called achievement tests are in truth mainly tests of native ability we should get very similar results by similarly investigating their units. I have investigated the units of the Stanford Achievement Test, just as I have those of the Stanford Binet.

Through the kindness of Mr. W. E. Wiley I was able to study the Stanford Achievement scores of some 230 pairs

of sibs attending the elementary grades of the Lodi, California, public schools. In the case of the Stanford Achievement Reading Test, it is found that if the difference between the third- and fourth-grade means is called 1, then the difference of seventh- and eighth-grade means should be called .43, in order to yield the maximum correlation between sibs. The result is quite in harmony with the .50 found for the Stanford Binet. This similarity may indicate that the assumptions made are about equally valid whether the Stanford Binet or the Stanford Achievement Reading Test is involved. In these same subjects it is found that for the Stanford Arithmetic Reasoning Test if the third- to fourth-grade difference is called 1, the seventh- to eighth-grade difference should be called .83 for the correlation between sibs to be a maximum. This result is a little farther away from the Stanford Binet result, and on the opposite side of it from that for Reading. Possibly the Stanford Arithmetic Reasoning Test is even more a native mental capacity test than is the more comprehensive Stanford Binet. Finally, for the same pupils it was found for the Stanford Computation Test that if the third- to fourth-grade difference is called 1, the seventh- to eighth-grade difference should be called .44 to yield the highest sib correlation. It has not been possible to calculate the probable errors of these results, so we must judge of their reliability by comparison one with another. They certainly do not wander over the conceivable range, which is from plus infinity to minus infinity. The smallest value we have is .43, and the largest .83, with values of .44 and .50 in between.

One further approach, again entirely independent as to population, though somewhat similar as to hypothesis has been made. Assuming that the correlation between sibs measured for a native ability is some intrinsic amount greater than 0 and less than 1, and remaining constant for life,

we have the argument already given. Now let us assume that the correlation between an individual measured for a native ability at one age and the same individual measured for the same ability at another age is perfect, no matter, of course, what the two ages, when natural units are employed and deviations of scores are taken from age means. This is an even more usable hypothesis than the former because the trustworthiness of our quantitative results are in large part dependent upon two things, one of which, nature, is the cause of high correlation, and the other, nurture, of low correlation. Surely one's original nature is perfectly correlated with his own original nature at a later age, so we clearly have one factor which leads to high correlation. The other factor, nurture, is probably not the cause of high correlation from age to age, for teachers change, and the incidents of child life are not strung upon a purposive chain—most children at the age of 14 do not know what vocation they will follow. An exception to this statement may be necessary in the case of religion and politics, for they may be the outcome of highly correlated elements of nurture.

Through the kindness of Mr. Cecil R. Brolyer I have been able to study the Stanford Achievement total scores of 348 California school children tested with one of the forms of the Stanford Achievement Test in elementary school grades above the third, and retested three years later with the other form. At this later time quite a number of the children were in high school, and the test may not have been quite hard enough to have adequately tested them, though the test is known to be a serviceable measure in the ninth grade. It was found that if the third- to fourth-grade difference is called 1, the seventh- to eighth-grade difference should be called .45, in order to yield the maximum correlation between the individual's first score and his second. This is in remarkable agreement with the earlier

findings reported. The function tested is a composite including reading, arithmetic, language usage, spelling, history and literature information, and science information.

A similar investigation was made of a younger population—224 children in the second and third grades at the time of the first testing. The original Stanford Achievement Test comprises nine tests when given to grades 4–8, and 6 tests, 3 reading, 2 arithmetic, and one spelling, when given to grades 2 and 3. Utilizing the results of these six tests only provides a means for studying the relationship of units at the second-grade level to those at higher levels. It was found that if the third- to fourth-grade difference is called 1, the second- to third-grade difference should be called 1.66 in order that the correlation between the child's score at the first testing and that at the second, three years later, should be a maximum. The comparable value in the sensed difference study was 2.00, so in these results again we see much similarity, considering that they are based on entirely different hypotheses.

The data that I have presented tend to show that units which make the product moment correlation between sib and sib or between individuals at one age and the same individuals at another age a maximum are closely similar to units which are sensed as equal by teachers, and they are such that approximately, if the third- to fourth-grade difference is called 1, the second- to third-grade difference should be called 1.75, and the seventh- to eighth-grade difference should be called .55.[1]

The agreement between the sensed difference and the maximum correlation units is an important point in their favor, but I do not at this stage of our knowledge express the belief that one of these or the average of these is indubi-

[1] The statistics yielding the figures upon the Stanford Achievement Test units are given in Kelley, *Scientific Method*, 1929 edition, Appendix B.

tably the best unit to use for the scientific study of mental relationships. So far as present meaning of terms is concerned, a sensed difference unit certainly holds chief claim to consideration, but so far as new meanings are involved, some other unit may be more serviceable.

Worthy of consideration in this connection is a unit based upon a Gompertz growth curve as S. A. Courtis has shown, and also the unit suggested by the studies of L. L. Thurstone. This unit is such that the mean score for an age group divided by the standard deviation for a random population of the same age is a constant. It is a device that would naturally lead to the use of quotients in the interpretation of scores, whereas the sensed difference unit naturally leads to an interpretation by means of differences in scores. It may be that we shall need both units. Consider the temperature scale. At the present time two very distinct types of interpretation are present, the one involving straight differences in temperature—practically all of our common usages of the scale are of this sort—and the other involving the so-called absolute temperature scale, in which temperature is measured on the Centigrade scale from $-273°$. On this scale, when other things are equal, the pressure of a gas is proportional to its temperature, just as on Thurstone's scale group variability is proportional to group mean. In the case of temperature, the statement as to proportionality of pressure and temperature does not hold throughout the entire temperature range, but that does not prevent it from being a very useful relationship throughout much of the range. So with Thurstone's scale, though the relationship he pictures as being a possible picture for the hoi polloi may not hold for imbeciles or geniuses, infants or octogenarians, it may, however, be a very useful relationship for the description of the rest of us.

One further basis for the determination of units of mental

measurements has very great scientific possibility: If we have a number of mental traits—to make it specific, let us think of musical ability, mathematical ability, and memory ability—that, when measured in a certain manner, are completely independent of each other, then this manner of so measuring them has peculiar value. If the traits are not, intrinsically, in the mental structure of mankind, independent, no units can be devised which would make them appear so, except perhaps discontinuous units. Therefore if we can find any traits and units in which to measure them in terms of which independence is manifest, these will be the natural units, these will be the units in which all relationships will be evaluated in their true light.

The first steps in the building of a scale of measurement in which variability is proportional to mean, in the building of a scale in which the correlation of sibs or of early and late performances of the same individuals is a maximum, and the building of a scale in which the units are proportional to sensed differences, have all been taken with great promise. The first steps even have been taken in the determination of scales revealing independent mental traits, but here the task is much more difficult, though the benefits to be derived are correspondingly greater. When this latter is done—I do not say, *if* it is done, for it simply must be done, the needs of society and our demands for knowledge will not be satisfied until it is done—we shall have a tool so powerful for the understanding of individual differences and of native possibilities and of nurture needs that we shall wonder how we staggered along in our social adjustments and our uncertain educational efforts without it. To any who are just entering the field of psychological research, we can say that the best problems await you. You can be happy that you are not restricted by Wundtian boundaries.

CHAPTER IV

THE OBJECTIVE MEASUREMENT OF THE OUTCOMES OF THE SOCIAL STUDIES

BEFORE attempting to measure the outcome of the social studies we should know the nuclear themes around which they are organized, provided there be such.

What sort of coordination is there in the social studies in the elementary and high schools? Here are the social studies curricula for two quite random communities, for grades 2, 5, and 8. The first community is a small town in the West having small diversified commercial and industrial activities and a population which is mainly American born, but wherein is a scattering of foreign born. The second community is located at some distance from the first, but it can be described in just the same terms. Are their social curricula similar?

In the second grade in the first community the social studies are: Community and the Child, Primitive Life, and Special Holidays. In the second community there is one social study: it is Home Geography. From the statements given we might judge that perhaps half of the work in the two places was quite similar. In the fifth grade in the first community the social studies are: Colonial Life in North America through the Revolutionary Period and the Development of the New Republic to the Present Time, and also State History. In the second community is taught the Geography of the United States, North and South America, Europe, Africa, and Australia. Perhaps 10 per cent of the work is similar in the two communities. In the eighth grade in the first community is taught United States history, 1860

to date, with emphasis upon social and economic phases. In the second half of this year Community Civics is taught. In the second community we find United States history to 1840 and Geography. Perhaps 10 per cent is common to the two courses in this grade.

A comparison of the other grades would reveal no greater agreement, grade by grade, than just mentioned. A comparison of the curricula for the entire eight grades of the elementary school, neglecting order of courses, would reveal a greater similarity from city to city, but there still would be perhaps 50 per cent of the material given in one community not represented in the other, and only a small part of this would be local history, local geography, and local civic problems.

In short, there is little uniformity in subject matter offered or in time or order in which treated. The disparity in curricula here noted is, I believe, characteristic of that between any two communities chosen at random. Now, this is particularly interesting and important to note in connection with the claim seriously put forward that the social studies should constitute the core of the elementary and high-school curricula. It is, to say the least, unusual to think of a core which is itself so unstable. We are told—the "we" referring to such of us as accept history as taught—that in the Middle Ages the trivium was a real core of elementary education and the quadrivium of advanced education. Grammar, dialectic, and rhetoric represented a content substantially invariable, as did also geometry, arithmetic, music, and astronomy. For years there were additions here and there and furbishings up of the old subjects, but withal there remained an unchanging core.

Certainly, with our present knowledge as to the objectives of the social studies, as to their appropriate content, and as to the grade placement of materials, we cannot think of the

social studies as constituting such a core in the curriculum. It is not that we know less now than in the Middle Ages as to outcomes of study, but that, knowing more, we lack the self-assurance necessary to assert that a given material and method of approach is clearly the best and should be unchanging and imposed upon all.

If we search elementary curricula we find a certain core. It is represented by reading and arithmetic. Every child is taught both. The order of treatment and even, with rather small deviation, the specific grade placement of concepts developed is standard throughout the country. The methods employed in different schools are characterized more by their similarities than by their differences. There is no similar core to be found at the high-school or college levels. The outcome of the instruction in the core subjects in the elementary curriculum is demonstrable. As the result of the work in reading, children learn to understand the printed page. They extend their sensitivity beyond that of personal contacts to contacts with the thought of potent individuals of other places and of other times. As a result of arithmetic study they learn a technique which is the foundation of modern business and of the vast fields of quantitative physical and social science. These demonstrable and fairly universal outcomes are accomplished by a method of instruction which has much in common both from grade to grade and from school to school. The specific nature of the child's later reading habits and of his specific utilization of quantitative methods in business or scientific study are unknown and are considered irrelevant at the time of the early instruction. Such, then, is the nature of this core in our present educative process.

Do the social sciences—history, geography, citizenship, ethics—or does any one of them, have the characteristics of such a core? This would require core content and a core

method of instruction. So far as history is concerned, such a thoroughgoing believer in the value of elementary and secondary history study as Dr. Henry Johnson specifically disclaims that the chief value lies in any core content. I believe that thoughtful students, not only of history, but of geography, economics, and sociology, will agree with him. In short, there is not at present a foundation in subject matter and method of the social studies which can make them the core of the elementary or secondary curriculum.

Is there the possibility of a core upon some other basis, specifically, upon the basis of a common purpose? We can find in the educational processes of peoples sufficiently remote from our own either in time or place that we can view them in perspective, illustrations of the organization of educative processes around a continuing purpose. Less attractively put, we can say that we find illustrations of national indoctrination. The Fascist educational purpose in Italy and the Soviet purpose in Russia are striking contemporary illustrations. I judge it to be no extremity of statement to say that in Russia today it matters not what techniques of instruction are followed, what subject matter employed, if it results in a belief in the excellence of the Soviet system and in a belief in the damnable qualities of other systems. In Russia the social studies constitute the core of the curriculum. It is a core in terms of purpose,—the creation of good Soviet citizens. It would be a simple matter to draw further illustrations, for example, from France and Germany of the preceding two generations, showing the nature of a dominant purpose as a core in the curriculum.

If the social studies are made the core of the elementary and secondary curricula of the United States, it seems to me it must be in some such sense as is the case in Russia, the purpose here being to develop good American citizens. Let us not salve our sense of truth and of perspective—the

highest virtues of the historian—by asserting that our group consensus of a good American citizen is so superior that we have nothing to fear from its realization. We may, perhaps, though even this is a question, have less to fear from fellow-citizens as ideally conceived by the men and women of America today than as betrayed by the lives of the men and women of America today, but we should be very fearful of a point of view, the standards of success of which are based upon demonstrable progress in indoctrination. I can cite no national program of education covering more than fundamental processes—reading, writing, and arithmetic—that has not been a program of indoctrination. There have been individual efforts to so instruct as not to indoctrinate. Perhaps, historically, the greatest of these was that of Socrates, to teach men how to discover truth. In a very attenuated form this was indoctrination, for truth was set up as an ideal whereby to live. For his efforts Socrates drank the hemlock.

Can we set up the Socratic ideal and assert that the core of the curriculum is the purpose of helping men to learn the truth wherever they search for it? This would commit us to an outcome of which we are now unaware. For example, it may be true that social and individual happiness and advance would be furthered by breeding from a select 10 per cent only of the population. Are we of the United States ready to sign a blank check which might be filled out in this or some more extreme manner? We surely are not. We will not subscribe to a purpose such as that of Socrates whereby we might find ourselves unwittingly committed to a presently unpleasing outcome. Indoctrination of the Socratic sort is too little indoctrination, is too pure in its endeavor to meet with our approval. Speaking of "our approval," I, of course, mean the approval, after due reflection, of those who count, of those who constitute the backbone of America,

and not of any clique of intelligensia, however selected, but not representing the real thought of this basic citizenry.

I am forced to the conclusion that if there is a core in the curriculum corresponding to a national purpose, it takes on such virtues and vices as lie in indoctrination. As a practical matter, there seem to be two principles which should guide us: First, any curriculum core in terms of purpose should be in as broad terms as possible, that is, they should, wherever possible, lead to attitude of mind instead of specific conduct, should, wherever possible, lead to methods of determining attitudes instead of to attitudes themselves, should, wherever possible, lead to a consideration of whether any attitude was needed instead of to a building up of an attitude. Second, ample provision should be made for the support of educational institutions and of individuals not accepting or emphasizing the national purpose, in order that divergent practices may take place, that such of their virtues as become demonstrable may become widely utilized.

At the present time I do not believe there is any such purpose core, at least not any that we are conscious of, because of the lack of unity of educational philosophy and of educational administration, for both of which we may be thankful, and also because the subjects through which the duties of the citizen are supposed to be best presented, namely, sociology, problems of democracy, community civics, home economics, etc., are still so new in the curriculum as not to have yet become mummified in content and presumably still less so in purpose.

These remarks have been intended to make clear the very great difficulty that confronts one trying to measure the values of the social studies. In the foreign language investigation no one particularly questioned the view that for students to know many foreign words instead of a few was a good, to be able to speak with the inflection of the native

instead of the traveling American was a good, etc. Here a core in subject matter was tacitly accepted and the measurement of pupils, therefore, becomes relatively simple. If the important outcome of the social studies is not subject matter, but attitude, no such simple problem of measurement is presented, for the desirability of any specified attitude is open to question to the degree that knowledge of many foreign words instead of few is not, and because the attitude is a response to more indefinite and diversified cues than is specific knowledge. The simple question: "What is the French word for horse?" will bring a true response from pupils, that is, those who know it respond correctly, and those who do not either fail to respond or respond incorrectly approximately 100 per cent of the time, while a question: "How should a Democrat feel toward a Republican?" will bring a response characteristically unamenable to sound appraisal, for in addition to the subject's true attitude as it would be revealed by divine insight there are unmeasured differences in significance of the terms "Democrat" and "Republican" and there is a background of widely different intimacy of contact with these creatures. There are, of course, differences of contacts of pupils with horses and with written sentences referring to them, but such differences are commonly of a lower order than those that attach to emotional or attitude-arousing situations.

Consider the attitude of tolerance toward weakness of others. A certain acquaintance told me that he had been puzzled over his own mental state because, when an associate recited behavior little short of an assault upon one of the opposite sex, he at the time looked upon the conduct with tolerance. This seems to be a rather high order of tolerance of the mistakes of others. The point raised is whether tolerance is a virtue or an evil. Shall we endeavor to develop it or the contrary? If we measure it, what scale shall we

use and how shall we interpret the results? There may be discrete states of tolerance dependent upon the specific training and possibly even upon the original natures of the individuals in question. Is there a religious tolerance, a separate racial tolerance, a tolerance of the violation of the eighteenth amendment, a tolerance of the violation of traffic laws, a tolerance of stupidity, a tolerance of moral obliquity, etc.? We do not know whether these things are largely discrete or largely the same. If largely discrete, as certainly is not unreasonable, both the measurement problem and the educational problem as it concerns itself with tolerance are greatly complicated.

In spite of the complexities that are present, we must surely assert that one of the important outcomes of education is the establishment of what we think of as correct attitudes. A little white boy growing up with negro children has a feeling of friendliness toward them. There is no thought of racial antipathy. The feeling of friendliness is not a construct that has become established only after native antipathies have been suppressed; it has been the outgrowth of a normal friendly contact. Later in the life of the same white youth we may find strong racial prejudice. If this is the case, and if racial friendliness is considered desirable, the problem that then concerns us is quite clearly to undo something that an earlier educative process has accomplished. This would be a very uneconomical way to proceed. How much more direct it would be to scotch the first educative process, that which led to the attitude of racial intolerance. We would not think of teaching a child that 6×7 was 35 as a preparation to teaching him that 6×7 is 42. Now, in so far as attitudes of people become established by such a process as described, the educational problem is to exercise a euthenic influence very early in life in opposition to the cacothenic influence, if I may coin a

term, of the home, street, or gang. To wait until the undesirable attitude is full grown before its modification is attempted would seem the sheerest folly.

This situation creates a very interesting measurement problem, that of ascertaining when these cacothenic influences operate. We must measure something not a classroom product, something developed by specific experience that we know not of, but something characterizing an individual's response to cues which we can provide. I can ask a boy of five: "Would you rather play with a little Irish boy or with a pickaninny?" and I can ask the same boy when an adolescent: "Would you rather have an Irish boy or a negro for a companion?" If the answers are different, something has happened meanwhile, of this I am certain, though of its specific nature totally ignorant.

As with the racial attitude cited, so with most attitudes. A knowledge of what is taking place in the minds of growing children, whether engendered by school or extra-curricular activity, is of the first importance to the teacher and administrator ordering the social studies curriculum. All will grant that development of and changes in attitude take place as the school years roll by. Are these measurable? There are intangible values resulting from instruction and incidental contacts and always will be, but so brutal a thing as an attitude which at times is violent in its fierceness and crudity of manifestation is not, in its usual phases, one of them. There is no logical reason why we should not measure all attitudes that we can define, but for practical reasons we must limit our endeavors to the more important ones only.

In the case of a course in history wherein five hundred facts are taught, we measure for factual knowledge by testing for these specific facts. If our test involves but one hundred of the five hundred, we do not know that any single one of the four hundred not tested for is present in the case of any

child. It is true, we may, as a result of our sampling of the child's knowledge, be quite sure that a certain proportion of the four hundred are within his experience. The same idea holds with reference to correlated attitudes. If we test for a child's attitude toward the Chinese, we do not know his attitude toward the Japanese, but presumably, just as in the case of history information, if we quite thoroughly sample his attitude toward a number of races, we will probably have a strong indication of his attitude toward other equally well-known races. This seems to be about all that we can do, and it should be quite sufficient for the general broadening of our understanding of the child. If there are gaps in our measurements of attitudes, they will probably be no more serious than those in our measurement of information, and we have established by adequate investigations of validity and reliability that these are not so serious, but that our measures are highly valuable guides.

Let us consider the similarity or lack of similarity between specific attitudes and specific knowledges. In the fundamental school subjects, reading and arithmetic, specific knowledge is the aim. We know that we can teach this and can measure it. In the social sciences a rather common view of many scholars is that the specific facts involved are of small importance in comparison with the principles developed. For convenience, but not to convey the idea of a sharp dichotomy, I would divide these principles into mental outlooks operative in meeting novel problems and attitudes operative in meeting or remeeting old situations, *i.e.*, situations the essential aspects of which are felt as similar to those of earlier situations. In so far as these principles are attitudes, the question is, are they general or are they specific, just as is factual knowledge? A specific question put verbally or by the attendant circumstances, such as, "When did Lincoln live?" calls for specific knowledge. A specific situation,

such as a seat in a theater next to a negro, calls up one's attitude toward negroes. The attitude may be aroused by contacts in the theater or the street, by pictures or articles in newspapers, etc., which though constituting a greater variety in stimuli than those calling for the period when Lincoln lives, are nevertheless fairly specific cues. Shall we, therefore, conclude that the attitude is specific because its cues are more or less so, or that it is general in that it is a response to numerous situations? In the specific nature of its cues it has a characteristic akin to knowledge while in the universality of them it is akin to habit. As we can measure both knowledge and habit we should be able to measure attitude, and by techniques which are a cross between those employed in the measurement of knowledge and of habit. One such technique is the free association experiment wherein a certain word is the cue, presented in a situation not so specific as to call up a definite past, *i.e.*, knowledge, but rather permitting the subject to interpret the cue in the light of his individual slant on things. If I say the word "deer" (dear) the hunter gets one picture, the lover another, the miser a third—you may classify yourselves. The response "gun" would indicate the hunter, "girl" the lover, and "money" the miser. True in this case if the response word given is "fawn," I don't know whether hunter, lover, or miser is responding, but ordinarily, or at least many times, the response is indicative of mind set, or attitude.

Another technique for measuring attitudes is that employed by L. L. Thurstone.

In case the attitude has few cues its effect on life is generally small and of the order of importance of specific knowledge. For example, a city child's attitude toward snakes is probably not of great importance in his life. For the class of attitudes wherein there are innumerable cues, such, for example, as social mindedness, honesty, and independence,

the specificity of the mental trait must decrease and a general something remain which far transcends the single-cued response in its control of the individual and his various contacts. Though there may be different honesties, one for the school, another for the home, another for the playground, one for friend, and one for foe, still they can hardly, in general, be so specific as to give us an honesty in regard to grocery stores, another in regard to bakeries, another in regard to five- and ten-cent stores, etc. If they were so highly independent, would we not conclude, as many have with reference to specific knowledges, that they were among the less important outcomes of the child's education?

For the reasons just given, I feel justified in advocating the measurement of such attitudes as have the widest fields of application, as are set off by the largest number of cues, as have the most generalized cues, and as have the most serious racial consequences, rather than an attempt to measure all attitudes or to measure them in terms of their specific features. Specifically, had I not the study of Hartshorne, May, and Maller before me I would attempt to measure honesty and, if I failed, but not until then, would I divide it into parts, perhaps honesty toward friends, and honesty toward others, or honesty under surveillance, and honesty when alone, etc.

Let me mention certain quantitative measures of attitude. I think some of them can with profit be subdivided, but even as they stand they have a sort of coherence or unity which has been demonstrated.

Dr. L. L. Thurstone has some very pretty scales measuring attitudes. You should note that questions of interpretation are with us even after the scale of measurement is quite satisfactorily provided. Dr. Thurstone's attitude toward the church scale consists of sentences arranged in a haphazard order upon a sheet, each of which the subject

is called upon to read and either endorse or disapprove. Here are a few, not in the order as given, but in the order in which their endorsement indicates high valuation of the church:

The endorsement of the following statement indicated the attitude most favorable to the church that Dr. Thurstone secured a measure of: "I believe the church is the greatest institution in America today."

The endorsement of the following indicated almost as favorable an attitude: "I believe the church is a powerful agency for promoting both individual and social righteousness."

The endorsement of the following statements indicate successively less and less support of the church:

"I feel that church attendance is a fair index of the nation's morality."

"I think the church keeps business and politics up to a higher standard than they would otherwise tend to maintain."

"I do not understand the dogmas or creeds of the church, but I find that the church helps me to be more honest and creditable."

"I believe in the church and its teachings, because I have been accustomed to them since I was a child."

"I believe the church is fundamentally sound, but some of its adherents have given it a bad name."

"I am careless about religion and church relationships, but I would not like to see my attitude become general."

"I believe in religion, but I seldom go to church."

"I believe in sincerity and goodness without any church ceremonies."

"I think too much money is being spent on the church for the benefit that is being derived."

"I respect any church member's beliefs, but I think it is all 'bunk.'"

"I think the church is a hindrance to religion for it still depends upon magic, superstition, and myth."

"I think the organized church is an enemy of science and truth."

If one's attitude is, let us say, moderately favorable to the church, he will disapprove of such statements as the

first three or four read and also of the last three or four, and will endorse statements near the middle of the scale. He will probably endorse: "I am careless about religion and church relationships, but I would not like to see my attitude become general"; "I believe in religion, but I seldom go to church"; and "I believe in sincerity and goodness without any church ceremonies."

To each exercise is attached a numerical value. These values increase as we go up the scale as read. The average scale value of exercises endorsed by the subject constitutes his attitude-toward-the-church score. Dr. Thurstone has tried this out upon divinity students at one end and radical groups at the other. The divinity students do not pile up at one end of the scale, for seldom does one endorse the statement, "I believe the church is the greatest institution in America today," and no group that Dr. Thurstone worked with even approximated the attitude represented by, "I think the church is a hindrance to religion, for it still depends upon magic, superstition, and myth," and by, "I think the organized church is an enemy of science and truth." The average score of divinity students corresponds approximately to the endorsement of, "I think the church keeps business and politics up to a higher standard than they would otherwise tend to maintain"; of avowed Roman Catholics to endorsement of, "I do not understand the dogmas or creeds of the church, but I find that the church helps me to be more honest and creditable"; of avowed Protestants to endorsement of, "I believe the church is fundamentally sound, but some of its adherents have given it a bad name"; of Chicago University students to endorsement of, "I am careless about religion and church relationships, but I would not like to see my attitude become general"; and of members of the Chicago Forum to endorsement of, "I believe in religion, but I seldom go to church."

As it is possible to place groups upon this scale, so individuals may be placed. So also it is very feasible to measure the changes that take place in individuals as a result of, say, a year's instruction in some school subject—perhaps citizenship. I bring this scale to your attention to show the entire feasibility of measuring status and changes by means of it without raising or answering the question as to whether one should endeavor to develop high scores or low scores. This ingenious measuring device is given us, without prejudice, to be used for whatever purpose we see fit, just as are Fairbanks scales given us to weigh what we will, and with whatever interpretation of results we wish to make. In the matter of human weight we do not say, "the heavier the better," nor "the lighter the better," but we believe in an ideal purpose—the mean weight for a given height of a group of very healthy people and we judge with reference to this optimum. Clearly, some such appraisal should be followed in the matter of most, if not of all, attitudes. I will even venture to assert that the same principle holds with reference to scholastic knowledge. It may be heresy to intimate that the highest school mark is not the best for the individual to strive for. If so, it is a sort that should appeal to any who have suffered or witnessed a nervous breakdown.

This is an interesting aside, and I must return to my theme. I hope we may develop scales of the sort described for the measurement of attitudes more definitely within the province of the social studies in the schools than is attitude toward the church. We need measures of tolerance, of attitude toward law enforcement, toward a-typical individuals, toward malfeasance in public office, toward property rights, toward social obligations, toward capital and labor, toward racial differences, toward crimes of violence, toward public responsibility, etc. We need these things for

an understanding of what is taking place and of how it can be changed. For, unless we can in some sense measure a change, we do not know it has occurred. The value of any one of these instruments is scarcely altered whether the socially approved objective of instruction is to increase or to decrease the attitude in question. We need these measures in order that we may know if adopted objectives are possible of attainment, for, if not, the objective has no value but to give philosophical satisfaction to some recluse. Such value is negligible in comparison with the harm such an intangible aim has in deterring effort along more fruitful lines.

Before leaving this matter of attitude I should mention the work of Goodwin Watson and of Hartshorne, May, and Maller in their studies of deceit, service, and self-control. Deceit is probably a compound of limited understanding, attitude, and habit. It would seem that a knowledge of the consequences of dishonesty would, in general, lead toward the development of honesty, that habits of honest conduct would be of more than momentary benefit, and that attitudes of unselfishness would harmonize with honest behavior. Whether the line of attack be through the intellect, the emotions, or habit, establishing the optimum condition of the trait to be striven for and knowledge of deceitful tendencies in the individual is indispensable to any endeavor whose outcome with respect to deceit is known. From one point of view, honesty is the overcoming of resistance or of inducements to be dishonest, and the greater the inducement resisted, the greater the honesty, or otherwise expressed, the greater the trouble one will go to to deceive, the greater his deceitfulness. Now, Hartshorne and May have shown that children can be graded upon this basis. A child who when given a scoring key may add a mark to his examination paper, answered earlier, in order to improve his score, may refuse to do so if it involves the erasing of an

earlier pencil mark, and a child who will add a mark and also will erase an earlier check or cross in order to add the correct mark may refuse to erase the earlier mark if it is in ink instead of in pencil, and so on, up to the child who will do all of these lesser things and, in addition, erase an entire line written in ink in order to write in a correct line. Actually, children drop out in their overt acts of deceit at successive levels, so that we have a well-graduated measure of the tendency to deceive upon school examinations. A tendency to deceive is a continuous trait and not, as frequently assumed, of the nature of all or none. We have reduced it to the class of quantitative phenomena with which we can cope. Simpler and more reliable measures will help us still more.

I must not devote more space to the fascinating problems connected with the measurement of attitudes. A second quite as interesting a problem is the measurement of the extent to which principles or laws are mastered by pupils. In fact, this has much in common with the measurement of attitudes. A principle is an abstraction earlier arrived at and elicited in any particular situation by some prepotent element. As a thing earlier incorporated into mental life, it is like attitude and also as a thing brought into play by a selected element from a total situation it is like attitude, and only as a thing finding expression in further intellectual activity instead of emotion or in immediate conduct does it differ. We can sample the further intellectual activity set off by a question more readily than we can sample overt conduct.

The problem of measurement here is not, however, simple, because the situation presented in the solution of which some general principle operates must be novel; otherwise the solution is, or may be, due to memory and not to the operation of a principle in the subject's mind. For example, if I ask a child who is in a strange locality at 9 o'clock in

the morning what direction is south and he looks around until he finds the sun, makes a deduction, and then points correctly, he has solved the problem by means of a principle, but if I ask a child familiar with the locality and he points in the right direction, he has probably solved it by memory of specific earlier instruction. We certainly cannot test for knowledge of principles by means of old material which has earlier been organized or taught in the same way now called for by the principle. Most of the so-called reasoning tests of history, geography, and even of arithmetic are not such at all, but information or memory tests. We cannot test independent of subject matter, so in order to test for mastery of principles we must do one of two things: first, give, preliminary to our test question, all the necessary content, or, second, utilize such common content that we may safely assume it is known to the child, but present this content in such a way that a generalization or a utilization of a principle not before taught in connection with it is demanded. As an example of this last, I can ask a child if the Pilgrims encountered any icebergs during the first week of their trip across the Atlantic and can ask that reasons be given. Here I am assuming that the child knows the latitude of the point of departure, the time of the year of departure, the effect of the Gulf Stream, and the source of icebergs. Knowing these things and utilizing principles covering climate and icebergs, he can answer the question. If the child knows these things and the question never presented before, the correct answer depends upon a proper utilization of principles, whereas if he does not know them, he must fail, though his ability to generalize be excellent.

When we test for principles, assuming knowledge, we must be very sure that the assumption is reasonable. As we can assume but little historical or geographical knowledge, we must, in general, give such in the test situation itself before

we can be sure that it is knowledge of principles that we are testing for.

There are, then, genuine difficulties in the way of testing for a knowledge of principles that are not in the way of testing for a knowledge of facts. It is on this count, I believe, that most of our so-called objective tests are tests of information and not of the more far-reaching functions calling for generalization, correct utilization of laws and principles, and selection of relevant from irrelevant material. The usual essay type of examination occasionally taps these more important functions. In so far as the essay test called for information, we can surely test more economically by means of true-false, multiple-choice, and other objective type examinations; in so far as it calls for a knowledge of principles and laws, we should either preserve it or develop a new type of test which measures these things in a more objective manner. That the development, in the minds of students, of principles, laws, workable abstractions and generalizations is of the greatest importance is, I am sure, the opinion of leading historians, geographers, economists, and other social scientists. If objective measurement falls down in this regard, it fails upon a vital issue.

If a child, or for that matter an adult, is asked to write upon the causes of the Spanish-American War and does write in good form, repeating what the text or lecturer has given as the causes his paper is so agreeably superior to that of the ordinary pupil who haggles his sentences and betrays that he never has understood the text or lecturer, that the highest mark is given to it, though not a single idea in it reveals an independent judgment of the causes of war. Coherence, lucidity, and accuracy of statement of such facts as are involved is just about all that is measured by the essay type examination, and these are not measured under uniform conditions from pupil to pupil, for after the first sentence

or two each has proceeded to an elaboration of what he knows or thinks he knows oblivious to perhaps a score or more essential issues that various other pupils may write upon. After the initial response the stimulus has changed, no longer constituting a uniform stimulus to the various subjects. Thus the first principle of good measurement is violated. The principle I refer to is that the set-up and question should bring pupils as nearly as may be to a common mental outlook or starting-point, so that differences in responses then made may be attributed to true individual differences, which are what we are trying to discover.

A question of the following sort, *if never discussed in class*, "What principle of party loyalty violated in the national campaign of 1912 was also violated in 1928? Enumerate similarities and differences in two instances," would, I believe, measure the ability to induce principles and apply them. I have but seldom seen an objective type examination question which I thought measured this type of capacity. It is, of course, unusual as a thing measured by the question which calls for a short essay. I believe that objective measures of such abilities can be built up as soon as scholars set their minds to it. The omission of this type from objective examinations is on a par with its omission from the essay type, for, remember, the essential feature of the party loyalty question put is not in what is called for, but in the condition that it shall not have been discussed in text or class.

I have spoken about the measurement of attitudes, outlooks, and mastery of laws and principles and have thus far neglected entirely to discuss the measurement of historical, civic, geographic, and other information which constitutes about 95 per cent of our present attempts at measurement. In a sense, I have taken the measurement of information for granted. It can be readily done and in an objective manner, and certainly should be done. We cannot

learn principles or develop attitudes in a vacuum. For each pupil there must be informational content and the important question is whether this should be substantially the same for all or vary widely. Pioneer problems might be studied through the subject matter of the Western Movement or that of the Pilgrims of New England, and many similar principles correctly deduced. There are advantages in the varied approach as well as in the uniform approach. The former demands a unification of the thought of two people upon the level of principle since they have developed their concepts through different content. This may be difficult and if not accomplished there is no agreement between them. The common content approach readily secures unification of thought, but it may be upon a lower level than that of principle, and thus constitute an unimportant or even spurious unification.

There must be some happy medium between the two, which should be defined and insured in the case of the citizens of a common country. This happy medium will encompass certain specific facts of national history, geography, language, and present activity. The information test endeavoring to measure these seems to me to be an essential part of a comprehensive measurement of social studies program. I do not feel that I can define or delimit these informational elements, but I have no doubt that they exist and rightly play a large part in public education. Though I believe that knowledge that George Washington was the first President lies within this field and that he could not tell a lie lies without, I am not ready to propose a rule of evidence or of social importance which should apply.

CHAPTER V

THE SCIENTIFIC VERSUS THE PHILOSOPHIC APPROACH TO THE NOVEL PROBLEM [1]

IT has been common to differentiate between science and philosophy, crediting the former with reliance upon facts and a kinship with data and the latter with a trust in logic and an affinity for the pure and ethereal verities of the mind. The expression "science proves" carries a very different connotation from "philosophy establishes." If it is held that "science proves" by a sound process, but one that is different from a second sound process whereby "philosophy establishes," I assert that the contention is wrong. The thesis of this chapter is that there is but one method tending to establish truth in the world of phenomena. This issue can be reduced to that of the places of data and of judgment in reaching decisions in the ordinary affairs of life. I believe all will agree that if "science proves" data certainly are involved, and if "philosophy establishes" certainly there is a mind at work, but neither data nor the mind can operate alone. To draw a crude, though, I trust, not inaccurate parallel, we can say that life in the biological sense is the interaction of the atmosphere with the soil. Call the soil "data," the atmosphere "mind," then life is "fruitful thought"—the interaction of the two. Dust does not burgeon upon the surface of the moon or even fly into tornadoes, for there is no wind to stir it, and an

[1] Address of the retiring vice-president of Section Q—Education, American Association for the Advancement of Science, December, 1929. A part of this address in modified form was first given before Phi Delta Kappa, New York, 1929.

atmosphere does not swirl in space where there is no dust or other gravitational field to hold it. We cannot think without facts, even though we may have facts without thinking. No person or organization can operate without data, *i.e.*, phenomena of life which are at least to a degree measurable and verifiable.

Of course, I cannot prove that the soil on the moon does not burst into bloom, much as I may believe it. The scientific accuracy of that statement is not the point, so grant it for the sake of the illustration. Therefore let us investigate in greater detail the part data or measurable items of knowledge play in careful thinking. Any statistic or objective measurement which fits into any classification scheme whatever, *i.e.*, has meaning, is such a bit of systematized knowledge. In addition to this there are novel items, meaningless at the initial moment, but quickly given meaning by the interpreting mind, in short, their novelty disappears and they present themselves as systematized knowledge. Starting with the novel element in an experience we find it quickly taking on the common feature, or statistic, characteristic. Now let us start with the statistic and see if it migrates, in the mind that apprehends it, toward the novel. The objective measurement once gotten is used, and the specific way in which it is used is the matter of importance to us.

Let me picture a situation in which the measurement has apparently taken the place of judgment in order to raise the question of the place of judgment on the one hand and of the measurement upon the other. The activities of the train dispatcher serve our purpose. He receives over the wire information as to the place and rate of movement of the various trains operating within his division. Two trains moving in opposite directions upon a single track would crash did he not heed the objective measures reported to

him and see that one train takes a siding. If the expected communication is slow in coming in he must literally long for it, and when it does come grasp it with joy and confidence. There is no trace of rivalry between his judgment and the facts upon which it operates, though these are so potent in the mind of the train dispatcher that they may be said to dominate the situation. This is so because the executive, by a full and free exercise of judgment, *wills* to heed and trust the objective measure. Such importance as it has, has been given to it by an act of judgment based upon earlier experience adequately scrutinized with very similar measures. Thus we are dealing with a scientific conclusion—one impregnated with human judgment and not devoid of it. Because of this science can never be exact, because it is never free of the element of human appraisal.

Let us carry this thought over into school life. A third-grade child is given an ABC reading test and secures a score of 40 on it. It is also known that the average score on this test for all pupils of the third grade in which this child is located is 30 and that the average for the fourth grade is 40. What will the school executive do in this situation? Promote the child one term, promote him two terms, keep him where he is or demote him? The mere figures that I have given do not answer the question. Surely it is inconceivable that the few facts given, unsupplemented by important other facts and unoperated upon by human judgment, do adequately answer a question of promotion. They cannot in themselves be sufficient, and teachers and principals should so affirm. Such objective facts should affect a teacher's or principal's judgment, which is the ultimate arbiter, only in so far as the teacher or principal asserts it reasonable that they should. There are a number of things that should operate in the building up of a conviction of trust or distrust of the ABC reading test scores. First, is the

agreement, in one's experience, between the test scores and ability of pupils as otherwise ascertained. Second—as pertinent to the individual case—is any accessory information about the pupil that may be available. Third, is the confidence one places in the sponsors and critics of the test (authors and others). And fourth, and ordinarily by far the least important, is the confidence one places in the test as a result of a perusal of the test items. As a result of these investigations one gives little or great heed to the test score.

The point is that it is the executive that *gives* to the score such importance as it has. If, in his best judgment, based upon all the facts that he has been able to muster, the test is not entitled to an important position in determining promotion, then, in the executive channel that determines promotion, it does not and it should not hold an important position. Perhaps some of you disagree with my statement that under these conditions the test "should not" hold an important place. You may say, "Suppose the test is intrinsically an excellent one, then it should hold a high position even though the executive is unaware of its genuine merit." I disagree with this view, for the executive should be the responsible party in the matter and personally held to account for any mistakes. We may hope that it will always be impossible to shift the onus of poor classification to so inanimate a thing as a score on a test.

Unless the executive looks upon the test score as a friendly and serviceable item of information for his own understanding of the child he should not use it. If you are a teacher and learn that little Bessie Jones has weak eyes you will place her in a favorable seat. You are glad that you have this information. As a result of it you and Bessie are better friends—you a better teacher and she a better pupil. The measure of Bessie's eyesight is a friendly fact because you know its implications and it does not mislead you. Test

scores which do not mislead hold the same possibility, making for mutual understanding and friendship between teacher and pupil.

It has been my pleasant duty to test many children. Time and again I have had before me scores for some child upon tests in which I knew from long experience a certain confidence could be placed, and I have literally longed to meet the child in person. Trusting the test scores, not implicitly but to a degree, I felt that I already knew the child, that he was a friend of mine, and I wanted to know him still better—to know wherein the tests had not been quite fair to him and still more to know those reaches of his character about which the test scores had been annoyingly silent.

Treated in this manner the test score is never a substitute for judgment. It is merely an aid in making judgments. It never delimits character, capacity, or achievement. It merely helps in the understanding of certain limited portions of these things. It should never circumscribe one's field of effort to understand. While illuminating a limited field it should challenge one to explore the reaches of mental life that stretch beyond.

Suppose a man possesses a small rowboat enabling him to explore the ocean throughout a radius of twenty miles. This does not decrease his knowledge of the ocean entire. On the contrary, it challenges him to get a ship and move out further and to listen to what travelers have to say and to appraise their tales with a sounder judgment. Just so should the information given by one good achievement or mental test enrich one's consciousness of and interest in the subject's life entire.

Certain opponents of objective mental measures assert that tests have a deadening influence upon the curriculum by tending to limit teaching to the narrow fields represented by the tests themselves. This may be so if one becomes so

enamored of the test that he loses sight of the child tested. I fear there are such people. I would criticize them, not the instrument they use. In an earlier generation such people were fetish worshipers. Some talisman, some rabbit's foot, some hocus-pocus answered every need. If a storm arose at sea, abracadabra stilled the waters; if a child was sick, abracadabra allayed the fever; if a male heir was desired, abracadabra turned the trick. So today, in a somewhat refined manner, we find the believers in the alpha-omega omnibus test. It tells what is good for backward babies; it clears the fevered brow of the dean when the rough-house rowdies walk the carpet; it sheds a great white light when college recommendations are called for, and it does a score of other things as well. You can pick out the modern measurement fetisher by the multitude of widely different things which the test of his choice will do for him.

If a test is in truth a good test it is good for something, not everything. Do we find in any other field of scientific endeavor an instrument that is good for everything? Thermometers measure temperature, barometers pressure, ammeters electrical current, and so it goes. As a thermometer a barometer is a total failure.

I believe that we should approach any mental measure devised with the idea that its field of utility is limited, but I would be the last to attempt to limit the field by *a priori* considerations. We must by careful trial determine the limits of utility. If we find that they are broad let us keep the instrument with its broad implications. This is not fetish worship—it is knowledge. My criticism is of those who extend the field of application without knowledge. It should be obvious to everyone that, as with every other scientific instrument, the judicious use of a test is something achieved only after careful study and much experience. Be confident that the process cannot be shortened. Recently a student

registered in my beginning class in measurement called me to task for dealing with averages, medians, age and grade norms when all she wanted was to know how to use tests in vocational counseling.

Though my remarks suggest a limit to the field of utility of objective measures I hope they also show the reality of the value of such measures. The score of an individual does not operate of its own accord, but only *via* the mind of an executive who concludes that the case in question properly falls in the class wherein such scores are useful. I have taken much time to make this point, but I believe it is fundamental and, unfortunately, sometimes overlooked.

No matter how well fortified by a long past history a certain type of measurement may be, it must take on the characteristics of a novel event in order to be properly interpreted in a new situation. The breadth of view and caution demanded of the test devotee is of the same order as that demanded of any scientist working with specialized measurements of any sort.

To generalize: It matters not whether we think of the interpreting individual as viewing all the elements in the case as novel or as viewing all the elements as lying within his organized knowledge. Whatever the view, the so-called novel or the so-called old elements all call for the same critical appraisal. If the elements felt to be novel are not subsumed under some existing-in-the-mind system, terror, or the taking-a-chance type of decision, results, and if the elements felt to be old are subsumed under an old system without a new and specific vindicating judgment there results a decision characterized by formality and lack of adaptability. The adequate mental process, whether that of philosopher or of scientist, scrutinizes the felt old and the felt new as though each were both old and new.

Ordinarily a decision, though made in the light of an

executive's entire experience, must be promptly made and cannot wait to be verified by a time-consuming study. Herein we may look for a difference between the philosophic and the scientific mental process. I will quote the distinction that Dr. Kilpatrick draws in this connection.[1] He states that science ordinarily postulates the question, "If I do this what will happen," but that philosophy cannot wait to see what will happen. He says, "Philosophy in contrast faces a situation of necessary action. . . . Note that any situation confronting is actual and must be met, and that any choice or course whatever, including refusal to act, is *an* answer which carries with it its appropriate harvest of consequences. Philosophy then asks, 'In the light of all this what shall I do?'" I believe that Dr. Kilpatrick has here stated *the* essential difference between science and philosophy—other differences follow therefrom. Philosophy is willing to attack any problem, any time, anywhere, and give an immediate answer. Science is not. I would not cite this as to the credit of either the one or the other, but do cite it with Kilpatrick and Dewey [2] as a difference of great moment.

The philosophic question, "In the light of all this what shall I do?" may be paraphrased without inaccuracy as, "In the ignorance as to consequences that enshrouds me, what shall I do?" for, of course, the "appropriate harvest" is not known at the time the decision and resulting act is made. The philosophic answer to an issue is, "Do something and the best you can," while the scientific answer is, "If in doubt delay decision and investigate." Each procedure has its place in this life, this hurly-burly in infinite time. It is something of a travesty upon the mind of man that it

[1] W. H. Kilpatrick, "The Relations of Philosophy and Science in the Study of Education," *School and Society*, 30: 39–48, July 13, 1929.
[2] See Kilpatrick, *loc. cit.*

is philosophy, sometimes thought of as the enduring, that responds to the hurly-burly, and it is science, changing science, that seeks to be judged by the standards of the permanent. Let us note some of the consequences of these outlooks.

The scientist procrastinates decisions, is other worldly, is of little aid in time of stress. He functions where deliberation and experimentation are possible, and his method is that of experimental analysis, synthesis, and verification. On the other hand, the philosopher provides an almost immediate solution. He counsels in times of stress and rides every emergency. His method is that of inadequate analysis, because logical only and not experimental, and inadequate, but much synthesis. Knowing that he has a unique situation to deal with he makes much of "integrations" and "total situations." His total picture, to which he reacts, may be grossly at variance with the real [1] total situation present, but of this he knows nothing because no experimental synthesis of factors has been made.

The statement that [2] "the [philosophical] effort is, as far as may be possible, to find a course of action which will save all the interests, which will integrate all into one course of action that best saves all" is also an excellent statement of the purpose of multiple correlation and of any scientific attempt to explain total outcomes. Though science moves more slowly and with greater assurance here, not being free to synthesize except as experimentation gives warrant, still it moves with the same purpose as does philosophy. In connection with this issue Dr. Kilpatrick implies that science deals only with parts of situations, while philosophy deals with them entire. Now there is no logic that deals with

[1] Meaning, of course, not some "thing in itself," but a thing as conceived to exist after very careful study. "Reality" in an ultimate sense can have no scientific, *i.e.*, verifiable, meaning.

[2] Kilpatrick, *loc. cit.*

wholes as wholes. If a problem case involving a cross-eyed, untrustworthy, brilliant, crippled, butcher's boy presents itself, what technique can treat this as a whole and without analysis? A logical or experimental analysis must be incorporated in any reasonable attempt to arrive at a solution of the total problem. How will you ever get the case referred to the oculist and how, unless there is analysis, will you ever get the cooperation of the father unless you call upon the butcher, etc.? Philosophy at its best must involve very detailed logical analysis followed by equally careful synthesis. As practiced, and, one would think, even advocated by Dr. Bode [1] in his recent work *Conflicting Psychologies of Learning*, the chief emphasis should be upon synthesis. Now experimental science, or that which involves the checking of an hypothesis against an outcome, depends upon analysis as a major feature in the process of arriving at the truth. Why is there a short-circuiting of this step in the philosophic approach? Perhaps a parable is in order.

There was once a very wise man who put a cat in a box with mice outside, and every time the cat scratched its ear, lo and behold, the box opened, the cat jumped out, and the one mouse caught tasted good. As time went on the ear-scratch movement decreased, almost to the vanishing point, but the jump through the door continued to be followed by some plaintive squeak, "The brave cat caught me." The sad part of the tale is that the cat never learned that the wise man had so fixed things that in general the more efficient the ear scratch the bigger the mouse caught, for any squeak nearly convulsed him with joy.

Is philosophy satisfied with any outcome? Dr. Kilpatrick [2] thinks not, but upon this point I fail to follow him. He writes: "having answered, philosophy awaits the outcome to test

[1] Bode, Boyd H., *Conflicting Psychologies of Learning*, 1929, p. 231.
[2] Kilpatrick, *loc. cit.*

the validity of its answer as truly as does science." How can philosophy await the outcome as truly as does science? The philosophical act must be terminated at some time, just as must the scientific, and, as previously pointed out, philosophy cannot wait—no, its act is terminated when conduct commences. Let me give a case which might easily have grown out of the philosophy of but a few generations ago. Suppose that philosophy concludes that bleeding is good for anemia, a vein is cut and the patient dies, then surely philosophy is to be charged with the death. No spiritual apology will convince a regretful wraith that the philosophic act is still in process. True, had the case been given to science she might have shirked the task, pleaded ignorance, and kept her hands off and said, "Let philosophy have the credit." While admitting this penchant of science to procrastinate, still, when a judgment, whether scientific or philosophical, is passed resulting in decisive conduct the problem as originally set is terminated. I must conclude that philosophy does not await outcomes—it acts—whereas science does await the outcomes of its experimental set-ups before it acts in the non-experimental, or important, life situation.

If we run through the steps in the complete act of thought, much as given by John Dewey,[1] except that I have added a final step, number 8, we can clearly locate a difference between philosophic and scientific thinking. The steps are:

1. A felt difficulty.
2. A definition of the difficulty.
3. A tentative solution.
4. A mental elaboration of the solution, leading to
   a. Additional tentative solutions and elaborations, if felt necessary, finally leading to
5. The belief that the solution is all right.

[1] *How We Think*, 1909.

6. An experimental verification.
7. An appraisal of the experimental findings leading to acceptance of mental solution and a decision for immediate conduct, or to rejection and a reinstatement of a felt difficulty. The process is continued until a verified solution which is immediately serviceable is obtained.
8. A forward look, or mental picturing of future situations to which the present solution is pertinent.

The first five steps are common to philosophy and science, but the sixth step, experimental verification, requires appreciable time not available to one who must act. Science continues and carries through the complete act of thought; philosophy does not. The distinction here made is reflected in the difference in activity of acknowledged scientists and philosophers. A distinction which I have occasionally heard calling the thinking part of the scientist's endeavors philosophic and the measurement and manipulative aspects scientific is a distinction that would not occur to or appeal to a scientist, as it would chop him up into unrecognizable parts. There are three important consequences of the difference noted. The philosophic solution is timely no matter how urgent the problem; the philosophic solution is more likely to be wrong than the scientific, and third, the philosophic forward look should be one of misgiving and largely a query.

My mind reverts to the sad parable of the cat. To the philosopher any outcome of his cerebration suffices. There is no conceivable method of determining the real excellence of a proposed course of action in a novel situation at the time it is first proposed. The pragmatic test is how it actually works out, but by the time this test is made the solution given by philosophy is long past. We seem to have reached the conclusion that the only solution to the urgent novel problem is the philosophic one, and that it is no solution because its fitness is and must remain unknown until

it is too late to alter it, *i.e.*, until consequences, good or evil, have actually followed. In the strictly logical sense that every present moment is a novel one and that something immediately takes place in reaching the next moment, I believe that this is true, but do not draw the important conclusion that therefore, in immediate problems, we should not turn to science, but resign ourselves to the unverified speculations of philosophy.

That these are unverified Dr. Bode appreciates, for he says,[1] "The more we emphasize man's power to shape his own destiny, the more necessary it becomes to recognize the possibility that he will make a mess of it." Though he realizes this his only comfort is philosophy, for he writes:

> In any event the problem [of whither we are headed] calls for an interpretation and organization of values, which is not a problem for scientific research, but a problem of philosophy. It is not a problem for science because it is not a problem that lends itself to the application of scientific technique. The scientist has his own special devices for collecting and interpreting data, but these devices prove inadequate when the situation calls for a recreating or reinterpreting of old values and old ideals. . . . If the foregoing discussion is correct, then most of the industry called the "scientific determination of objectives" is on a par with catching birds by putting salt on their tails.

With this I utterly disagree, for the problem of our educational and social ideals is not a problem that must be solved upon the moment. If so solved it would be unsatisfactory even to the philosopher, for tomorrow brings another moment. Far better that a year, a decade, in some matters a generation, be spent in determining educational and social objectives and techniques found by trial to be in line with past progress than that we "settle" the problem by speculation. If one asserts that what constitutes "past progress"

[1] Bode, *op. cit.*, p. 300.

is beyond comprehension he is indeed a pitiable optimist if he nevertheless believes that he can define "future progress." We cannot settle the problem of objectives for long by any method, but the ten-year experimental study holds promise of fitness and permanence not to be expected in the cloistered solution. Philosophically every moment is novel and calls for a new interpretation. Practically, every moment is surcharged with physical and emotional settings that have sprung from the past and that have been characteristic of innumerable past situations. The novelty of the moment, though real, may be insignificant in importance in comparison with the non-novel elements that are present. One of the features of the moment is the novelty in the organization of non-novel elements. From the viewpoint of the gestalter this dominates the situation. This is just a point of view and a very unhappy one, for it can never be proved, for, according to hypothesis, no two situations are alike, and accordingly a testing out and verification is never possible. The scientific point of view is to look upon the novel situation as characterized by certain cue or critical elements, or critical combinations of elements, which can be reinstated. The beauty of this view is that it can be proved right or wrong, as the case may be, by trial. This viewpoint has engendered mighty advance in the physical and biological sciences, and surely psychological and social advance lies in the same direction, for only thus is verification of progress possible.

I have made no distinction between science and philosophy on the basis of remote purpose or outlook and of course subscribe to the idea that a mind-body dualism is not necessary to a scientific point of view, and also to the idea that there is great need of and value in a criticism of historic conceptions. Dr. Kilpatrick considers that[1] "the need

[1] Kilpatrick, *loc. cit.*

for the continual criticism of current thought assumptions in the light of their wider bearings would of itself, apart from all other considerations, suffice to give to philosophizing a permanent place among the higher services of thought to man." The stimulus to evolution consequent to this service can hardly be overestimated, but is it not above all a service rendered by science? From Roger Bacon to Einstein the great cues to a criticism of current thought assumptions have come from science. Philosophy has tagged along and consolidated these scientific salients, but she has not made them. Did the score of philosophical interpretations of relativity precede or follow the experimental findings? Of course they followed. Did the philosophy of Bertrand Russell instigate companionate marriage or did popular interest in it raise an issue that he attempted to place in a rational understanding of mankind. Undoubtedly it was this latter, and so it goes. The intense and keen, but orbital arguments of the Middle Ages illustrate the value of a criticism of current thought assumptions not leavened by new scientific facts. That science should be the leader in this is inherent in the process of science, for when the verification step of experimental science fails to yield a check with hypothesis there is, practically speaking, an inevitable reexamination of the premises, which of course are merely current thought assumptions. There is no comparable cue stimulating the questioning of assumptions in philosophy. The jolt that sets off the questioning process is from the outside—it is unanticipated experience.

My advocacy of experimental investigation in the attack upon problems of social value is because of the method of science, not because it is exact. There is no "exact science." A definition of science not permitting of error both in the data and in the judgments of human beings who interpret scientific facts is of no practical use. While upon this matter

of error we may say that it is scientific to know that there are different degrees of exactness inherent in different stages of an argument. Science recognizes this time and again when philosophy (as practiced) ignores it. Having discarded any thought of exactness in science we can then turn to the important idea of the reliability of measures, findings, and judgments. Philosophy has been slow, to its detriment, in following science in this. Until philosophy attaches probable errors to its concepts it will fall far short of its possibilities. In recent generations it has willingly accepted the findings of its younger brother, science, but it should go further and adopt concepts of methodology from it also.

I wonder if there is a conspiracy among philosophers to belittle science and claim its peculiar merit for themselves. Whitehead writes,[1] "There will be some fundamental assumptions which adherents of all the variant systems within the epoch unconsciously presuppose. Such assumptions appear so obvious that people do not know what they are assuming because no other way of putting things has ever occurred to them." Again, "A civilization which can not break through its current abstractions is doomed to sterility after a very limited period of progress." I take these words, not as Whitehead meant them, but as a fine argument for science. To discover errors it only requires that experimental setups involving hypotheses based upon these unknown errors be attempted. Then as the attempt fails the error is revealed.

To what fields must science be limited? Specifically should someone say that education is outside the pale, would he also say the same of law, sociology, economics, psychology, biology, geology, chemistry, and physics? The relationships of physics and those of man to man are of the same order in consciousness. I cannot see how one can deny dualism and

[1] Quoted from Dr. Kilpatrick's article. A. N. Whitehead, *Science and the Modern World*, 1925, pp. 69 and 82.

still draw a line in the list just given separating the scientific from the non-scientific. If one draws no line and concludes that physics can never become a science (a little surprise for the physicists) I for one shall agree, in the sense that as far in the future as the mind can dimly perceive physical issues will arise for which no scientific answer is available—*i.e.*, no answer experimentally and adequately tested out. In the same sense only do I grant that education can never become a science.

Let us sum up the place of philosophy in life and specifically in education. It seems to hold an actual position which I deem to be different from its legitimate one, so let me speak of its actual position first. I will illustrate by reference to a field with which I am familiar—that of test construction. An author devises a test of honesty, let us say. He philosophizes most armchairishly, not failing to consider the experimental evidence reported upon earlier tests having something to do with honesty. He counts his hour or two (or ten or twenty, if you like) of armchair agitation as high grade and adequate for the solution of the issues. So he publishes his original contribution for the use of an expectant world that will no longer wait. True, it may be only that fraction of the world represented by the author that will no longer wait. However, this inability to wait is definitely a part of the philosophical attitude that never matures into the scientific attitude. The decision by the author that his exercises test honesty has been reached by speculation in lieu of investigation. Here philosophy is merely a makeshift, quite unnecessary, for science. This is my lesser criticism of it.

If there is necessity for prompt action any sort of a shift is welcome, so it is not disparaging to say that philosophy is the best method for the expeditious selection of makeshifts. Surely, excepting habitual acts, the majority of the acts of life will fall in this class.

My major criticism is that the test author, having made his speculations and come to his final conclusions, feels very contented with himself—he thinks he has done something worth while and of lasting value. Unfortunately, as he writes fully and with utter sincerity, many of his readers think so too, and thus they also are content and may so remain for years if they attempt no experimental verification of the test. Philosophy is here the great narcotic, the soothing-syrup for author and readers, when in truth a gadfly is needed. It has played this rôle throughout history and it does so today.

What is the merit of the philosophical conclusion? Because it is a makeshift it has a place—just the important and unavoidable place of the expedient. Think of a forty-niner in his rush to the gold fields of California in his eastern Conestoga wagon, and suppose that he break a whiffletree out in the Nevada desert. If he is the kind of a man who will not use a makeshift and sends back for a new whiffletree, he might by some be called a scientist—I would use a less complimentary term. Whatever he is called would not apply for long, for he would soon be a scrap of dried bones. In this case the solution will not wait. The time necessary to carry out the complete act of thought, with its step of experimental verification, is not present. Something must be done promptly, and when done it may be called a philosophic solution. This does not state that it is a "right" solution, or even a serviceable one. If the forty-niner tries to mend his whiffletree with a piece of yucca he will probably waste time and increase his danger. If he break up his wagon seat and use the timber he may pull through. Whatever he does has one indubitable merit—that of promptness. There is no certain merit in it on the basis of long-time adequacy. This is characteristic of every philosophic solution. The scientific solution is, or rather aims to be, a more or less per-

manent solution. The philosophic solution frequently should not even aim to be this, for when such is aimed at there is commonly time for the try-out step, so that experimental verification can enter in and the solution become scientific.

Problems demanding immediate answers, or at least answers before adequate investigations can be made, will always be with us. We shall always need philosophy. There can be no issue here. Not only so—we are going to need it increasingly in the future. If the area of a small circle represents scientific knowledge its periphery may well represent unsolved issues which, when first met, will ordinarily demand a philosophic solution. With the increase of this circle as science advances goes an increase in the periphery. I neither hope nor look for a decrease in philosophy as science advances, but just the reverse.

We shall need more and above all better philosophy. It seems to me that the most adequate philosopher will have the following characteristics. He will be a man of wide culture, familiar with the arts and sciences, with the psychology of man and with the values of life. He will be an accurate thinker—a sound logician—and have an extensive acquaintance with the facts and the methods of science. There is a peculiar necessity that he be aware of the scientific method. This method aims to secure more permanent solutions to its problems than does any other. It accepts the fact that time and investigation are necessary to this end. Though philosophy acts where these things are impossible the philosopher should attempt to parallel in his thinking what the experimentalist does in fact—only so can there be a tolerably promising philosophic solution of the problem.

Let me illustrate this by a problem which arose during the war. It was necessary to select men for training as officers. It seemed evident that the best selection would depend upon possession by the men of certain traits such as

mental ability, physical stamina, moral courage, cooperativeness, leadership, etc. Ratings upon these traits by superior officers of men in camps could be gotten. The problem was how to combine them into single gross ratings which could be used in the actual selection of men. Time prevented an experimental investigation, so philosophizing had to be appealed to. An experimental investigation in which these various trait measures were used to estimate demonstrated success as officers would have yielded the weights that should be attached to the measures separately in order to get the most reliable aggregate measures of fitness as officer material. In short, the experimental treatment would have analyzed the data and then combined the separate trait scores into the most meaningful total ability scores. The concepts of total correlation and of partial correlation (not of course assuming any limited type of relationship, as that of linearity, between measures) here operate and they alone do operate. No logical treatment not paralleling this can be as adequate as one which does parallel it. The more completely the philosopher parallels in his thinking the analysis and synthesis which the experimental treatment would yield the better is his philosophical solution. The great endeavor of the philosopher here should be to ape mentally the steps of science. He cannot have a technique which is better (omitting the time factor) than the scientific technique. Just the moment that he demonstrably did have, science would claim it as its own, for true science has no fetishes that it clings to in the face of evidence.

CHAPTER VI

A DEFENSE OF SCIENCE IN EDUCATION [1]

I WISH to spend most of my time in trying to show what philosophy and science can do, but I should first mention some things that they cannot do.

In an article upon "The Relation of Philosophy and Science in the Study of Education," [2] Dr. Kilpatrick presents four situations as peculiarly the province of philosophizing: "First, wherever there is indecision or doubt or dispute regarding the 'good life,' that is, the life that we shall approve and seek." There is an assumption here that philosophy can tell what the "good life" is. Now I believe there are only two approaches to the question of what constitutes a "good" act in the case of a child, that need to be considered. The traditional and more common view is that a good act is one that is in conformity with some concept of goodness antecedent to the act and actor in question. From this point of view, adult standards, philosophers' standards, traditional standards, or what not—in all instances standards not created by participation of the child in question—are taken for granted. I would be willing to discuss the artificiality to the child of such standards, and the impossibility of knowing whether such "goodness" was in truth good or not,

[1] The chapter herewith was the Negative of a debate held at a meeting of the New York Society for the Experimental Study of Education, February 6, 1931, between Professor Willam H. Kilpatrick of Teachers College, Columbia University, and Professor Truman L. Kelley of the Graduate School of Education, Harvard University, on the following question: *Resolved, that for some of the vital problems of education philosophy, not science, is and must remain a guide to the solution.* Reprinted from *Harvard Teachers Record*, issue of November, 1931.

[2] *School and Society*, Vol. XXX, No. 759, July 13, 1929.

but I shall not do so, for I believe that Dr. Kilpatrick disavows the validity of such a standard of goodness as fully as do I, and that we both follow John Dewey in this. Out of very many quotations which could be selected from Dewey upon this point, let me give the following: [1] "As long as the notion persists that values are authentic and valid only on condition that they are properties of Being independent of human action, as long as it is supposed that their right to regulate action is dependent upon their being independent of action, so long there will be needed schemes to prove that values are, in spite of the findings of science, genuine and known qualifications of reality in itself. . . . If they [people] are forbidden to find standards in the course of experience they will seek them somewhere else, if not in revelation, then in the deliverance of a reason that is above experience." Again Dewey writes: [2]

"The philosophy of education neither originates nor settles ends. It occupies an intermediate and instrumental or regulative place. Ends actually reached, consequences that actually accrue, are surveyed, and their values estimated in the light of a general scheme of values.

"But if a philosophy starts to reason out its conclusions without definite and constant regard to the concrete experiences that define the problem for thought, it becomes speculative in a way that justifies contempt. As far as ends and values are concerned, the empirical material that is necessary to keep philosophy from being fantastic in content and dogmatic in form is supplied by the ends and values which are produced in educational processes as these are actually executed." The traditional view depends upon a belief in the deliverances of a reason that is above individual experience.

[1] *The Quest for Certainty*, 1929, p. 44.
[2] *The Sources of a Science of Education*, 1929, p. 56.

Turning from this view to the modern view, so cogently presented by Dewey, the good life is to be conceived as something that is self-initiated and propagated by the child, and that finds its justification in his personal experience. Whether the child grows up to be a day laborer or a college professor should make no difference as to the fitness of this standard. It, and not some implanted thing, should be his guide in life—should define the good life for him.

If, due to conflicts between individuals, social standards of the good life are called for, and we know that they are, they must find their warrant in the harmonious activity of many individuals, determined by the best technique available. The social standard should be the outgrowth of the growing social organism, much as the individual standard should be that of the developing child. Now this study of many individuals is a statistical and a measurement problem, and the best technique for doing it is the scientific method, simply because, in addition to incorporating within itself the questionings and elaborations of the philosopher, it also imposes upon itself the necessity for demonstrable and objective proof of the soundness of its hypotheses. We may talk glibly about the requisite features of the good life, but we shall never know that they are good except as tested in the crucible of trial and retrial today and tomorrow. To say that this calls for time, initiating of activities in controlled and experimental groups, and experimental examination of consequences, and that we must know today without such effort is merely to ask for the impossible. In truth, we can never solve this problem. We can only engage in the process of solving it, for each day's solution is but the setting of tomorrow's issue. The practical issue is, in the matter of what constitutes the good life, shall we accept the so-called "solution" of today's philosophy and be content, or shall

we initiate this endless process of discovering through tested outcomes what are the consequences of conduct, and in the light of them set the standard of the day and the problem of the morrow. To make it very specific: without asserting whether for a child to play the game according to the rules is a part of the good life or not, nevertheless we can say that to know the correlative characteristics of children who do thus play and of those who do not is important, in that it would lead to greater knowledge of, and social power over, the traits of individuals in their bearing upon social structure—gang, party, state.

A second situation cited by Dr. Kilpatrick as peculiarly the province of philosophy is "to be found wherever the school must make a choice among persons, or in the relation of person to person." I would distinguish between the occasions for settling personal issues. If the matter is minor, or if so urgent that a study of it cannot be made, then practical judgment, philosophy if you like, should settle the matter. These are not the vital problems of education. Even if they were, there is no known means of "solving" them. All philosophy could do is advise. It could not know that its advice was sound, for time to prove it before action takes place is lacking. If the personal matter is not minor, and time for study exists, then the scientific method can be employed. If the king's son, just born, is to be sent to Oxford or Cambridge eighteen years hence, and if ample funds are provided, much could be learned indicating the probable tendencies in character formation and intellectual development of attendance at the respective colleges. Though such a scientific, historical, and parallel group study would reduce the chance elements in the education of the king's son but little, still it would reduce them somewhat. Though I have chosen the king's own son for illustration, you should still note the really non-vital nature of the issue involved. It

is of course vital in the sense that accidents are vital, but that can hardly be what we mean by the vital problems of education. I take them to be things of broad applicability and capable of social control.

The third situation cited by Dr. Kilpatrick as calling for philosophizing is "where a principle, or even fact, established in respect to abstracted data is sought to be applied in general,"—whether, say, a method of memorizing found to be efficient in the case of college freshmen taking a course in psychology and using nonsense syllables, can be taken over in teaching first-grade children the alphabet. Sometimes there is no doubt, *e.g.*, directions employed with the college freshman might be incomprehensible to the first-grade children. If the teacher does not know this, she will learn it upon the very first attempt to apply the method. If she feels sure that the directions are too difficult without trial, she will forthwith alter them, thereby changing somewhat the method. Let us say that she does not know whether the changed directions, the different setting, etc., really alter the method or not. What will she do about it? Why obviously, try it out, that is, use the scientific method, and discover by test whether the altered method under the changed conditions works or not. There is positively no other way of knowing. In such a problem as mentioned only the simpler issues can be settled by philosophy, and even then some of them will be wrongly decided. Within the week my attention has been called to an exercise in a history test put in by a very capable supervisor of history and passed upon by eminent historians to test a certain ability which develops from grade to grade. Though most of the other exercises of the test behaved as devisor and critics had expected, this particular exercise showed percentages of correct response in Grades 3, 4, 5, and 6 as follows, 27, 19, 11, 8 respectively. The experimental try-out revealed this. I

know the probable errors of these numerical results and unhesitatingly assert that, all the eminent historians to the contrary, children behave substantially as the try-out indicated, and not as they had anticipated. The best available judgment as to the consequence of a given course of action is not final and is merely a makeshift for evidence. That the applicability of an experimental finding gotten in one connection, to a second and a different connection is open to question, I grant. That the philosopher can answer this question is not to be expected. Even the experimentalist familiar with all the details of the first connection and with those of the second will err, though none other is as competent as he to judge. The only solution lies in trying the matter out and noting what happens.

Dr. Kilpatrick's fourth situation calling for philosophizing has to do with questioning assumptions. Let me mention three types of examination of assumptions, one by the philosopher, a second by the scientist familiar with the full technical implication of all the assumptions as ordinarily he alone is, and a third really ultimate questioning, the try-out represented by the crucial experimental test of them. The first two, however good, are makeshifts. The last is not. Every assumption dealing with practical affairs carries with it certain implications, which a logical analysis will reveal, which point toward an objective difference of some sort, so that the validity of the assumption stands or falls with the reality of the objective difference as revealed by experimental investigation. We should be but little interested in a mere intellectual questioning of assumptions, but tremendously concerned with such a real questioning as involves an actual testing of them.

I fail to see that philosophy has any claim to priority in the handling of the four situations mentioned except that of time and effort required in reaching a decision. It does,

of course, take less time to reach a solution mentally satisfying, which is philosophy, than to reach it and then test it experimentally, which is science. Science is not a quick means to knowledge but it is a means. Let me quote from the internationally known chemist, T. W. Richards, upon what is called for in the scientist:

> First and foremost I should emphasize the overwhelming importance of perfect sincerity and truth; one must purge oneself of the very human tendency to look only at the favorable aspects of his work, and be ever on the lookout for self-deception (which may be quite unintentional). Next one should never be content with a conventional experimental method or scientific point of view; one should be open-minded as to the possibility that the procedure or hypothesis may be incomplete. Each step should be questioned, and each possibility of improvement realized. And then, patience, patience! Only by unremitting, persistent labor can a lasting outcome be reached.

These are heavy demands but they are the demands of science, both as stated by the great exponents of science and as demonstrated by their lives. We must grant to scientists the right to say what the scientific method is, just as we must grant to philosophers the right to say what philosophy is, provided in each instance performance is in line with claim.

I am unable to conceive how any of the vital problems of growth and development, of social living and progress can be solved by the employment of lesser talents and activities than this great chemist mentions.

The vital problems of education as well as all others are solved by research. All research has as its object the completing of a mental picture—either the rounding out of a concept, the bridging of a mental abyss, or the illuminating of a mental cavern. The purpose for desiring to complete the picture may be either for the personal satisfaction derived therefrom or for the utilitarian benefit resulting. It

is valuable to draw a sharp distinction between the standards of excellence of research, having as its aim the construction of a mentally self-coherent system, and second, having as its aim a thought structure that is pragmatic, that actually serves when subjected to the test of the world external to the mind of the thinker.[1]

"Curved space" was a commonplace with pure mathematicians long before the Michelson and Morley experiment led Einstein to call upon it to characterize physical phenomena. As a reality in the world of ideas it was well established before Einstein. Many beautiful properties of it had been discovered, and the rules of research were known —they were the rules of logic raised to high perfection and called pure mathematical thinking. The moment these concepts were advanced as descriptive of the physical world in which we live, entirely new tests of excellence, new tests of validity, and new requirements in thinking were brought into play. The difference here pictured between "curved space" as a mathematical concept and as a phenomenon of nature is exactly the type of difference that runs through all thinking and investigation. It must be obvious that the distinction drawn affects all types of mental activity, though some have a strong leaning in the one direction, and some in the other—the pure mathematician and the metaphysician build unified thought structures and reach into experience merely for a few props and analogies wherewith to furbish their satisfying and quite complete mental world, whereas the narrow opportunist and utilitarian utilizes all phenomenal material to meet the exigencies of the moment. He reaches into logic incidentally and unsystematically to rationalize the bumps and knocks the world about gives him. The most

[1] Psychologically there is, of course, no world external to the mind of the thinker. There is, however, a world felt by him to be external, and another felt not so to be. The distinction drawn is between these two.

material minded is not entirely without a unifying philosophy, and the most otherworldly is not entirely unaware of conditioning experience. Though it is not deemed sound to draw a distinction between people upon the basis of being all or none with respect to the poles mentioned, it does seem sound to characterize many of them as faced one way or the other, and it does seem sound to characterize research upon the basis of its fulfilling the standards of the one type of mental demand or of the other. Between the two types lie the scientists, and many others whose positions are poorly formulated but who are forever struggling to so think as to encompass a varied and changing world *via* a thought structure comprehensive enough to provide for the variety yielded by past experience, and anticipated in the future.

An essential characteristic of such a mind is that it is changing. We should not refer to it as a thought mold, not even as a thought structure, unless the idea of growth is provided for. Nor should it be called a thought process, for the unchanging interpretative elements ordinarily exceed the changing. I know of no term that aptly describes such a growing mental organism, for the growth is doubly conditioned—once by the nature of the human mind and again by the nurture of new experience. Perhaps it can be approximately described as a fructifying thought structure.

Let us now consider the tests of right thinking in the realms of pure thought, and of living contact. The one inviolable criterion for correct thinking of the first sort, which I believe is accepted as a standard by all philosophies, is unity or mental integration, or, to use a term employed by Dewey, "internal consistency." Consequences must inevitably follow from premises, and no matter what their number or variety they must be so grasped mentally that they are seen to be self-consistent. The ramifications of such thinking have no boundary. New consequences and more and more of

them may be deduced from the original premises, and as they are, a greater and greater mental grasp is necessary to see them all fitting together into a harmonious whole. The elaborate structures of modern mathematics are the finest illustration of this type of thinking, and one who has but dabbled with pure mathematics knows its limitless scope and may perhaps sense its inherent beauty. The philosopher who builds his system with the rigor of the sound logician makes a contribution to his mental life and to that of his followers irrespective of whether or not the system "works" in guiding one's conduct as a candidate for mayor, or in putting through a business deal. To repeat, the great criterion of philosophical thinking is coherence of thought.

What is the essential criterion of scientific thinking? It is conformity of premise, as expressed in its logically necessary consequences, with experience. This is the great test and when made it is found that the premise never conforms, that is, never exactly.[1] Philosophy alone cannot handle this case, for what can it do when its premises are uncertain? Science does handle it repeatedly, and in its handling it becomes a growing structure, a method whereby premise and experience grow closer and closer together. Science is a prophet primarily, admittedly somewhat fallible, and only secondarily a body of principles and generalizations, since these are continually changing, being added to, subtracted from, refined and expanded in their application.

I said that philosophy alone could not handle the case

[1] My attention has been called to the "exact" agreement between the number of electrons around a nucleus as observed and as postulated by atomic number hypothesis. If this agreement is of the sort "individual A is either a man or a woman," then it is not exact at all, for our present knowledge of intersexed individuals shows that the individual may be neither man nor woman. May it not be sound to think of electrons as perhaps varying one from another by such small amounts that we have not discovered the variations, rather than to think of them as absolutely identical: in other words to think of the agreement as "nearly exact"?

when the premise is uncertain. I should rather have said that ordinarily philosophers do not attempt to handle this case, and here I have a real grievance with philosophy as practiced. Its persistent refusal through the ages to incorporate principles of probability with reference to its premises is so serious a shortcoming that its utility in throwing light upon living issues is greatly circumscribed. Most of the heated problems of life depend from premises which are only in part or to a degree established. Herein the crux of the matter is far more in the validity of the premise than in the deductions therefrom. If philosophy were to incorporate statistical concepts in its method, it could be contributive even here, but until it does so its pronouncements in fields where premises are uncertain are those of one who is too proud to fight, or is too right to know that he is wrong.

This distinction between scientist and philosopher upon the basis of thinking in terms of probability seems to be warranted in view of actual practice, but it is not a logical and necessary distinction. Is participation in experimental investigation versus non-participation a valid ground for distinction?

So far as I have read, Albert Einstein has never done any experimental work in physics. In view of this, is he a philosopher or a scientist? He is outstanding as the creator of a stupendous thought synthesis, and this would entitle him to high rank as a philosopher provided it were all that he had done. But it is not. I believe every physicist will claim him as a scientist, and for one simple reason: his thought processes terminate not in broad conceptions but one step beyond, namely in crucial experimental tests, which tests must be made, though perhaps not in his life time, before the problem is resolved. Sir James Jeans, though an experimental physicist, is while speculating upon the death of the universe a philosopher, for his arguments terminate in

no crucial experimental tests. If you ask what would the scientist do in this matter of the death of the universe, I imagine he would do nothing at all, saying that the issue was trivial, not worth spending good daylight upon while the sun is still warm and the world is alive. He cannot solve the problem, for the crucial test is infinitely remote in time, and he knows that he cannot. Neither can the philosopher as Jeans himself recognizes in referring to his ideas upon this matter as "speculative and uncertain," [1] but lesser men, those who are not scientists, may not so clearly see the utter incapacity of science or philosophy to contribute an iota of light upon this problem, the test of which lies in the physical world but beyond the limits of time and space to make. The point is that philosophy is not solving problems that science cannot handle. She is merely muddying the water if she attempts to treat them as solely within her province. That the thought process of the philosopher terminates in a mental-picture-felt-to-be-complete, and that that of the scientist in a still-to-be-performed-crucial-experimental-test, is the normal consequence of the mind which on the one hand is seeking unity of outlook, and on the other hand, conformity of thought with experience.

I have made some sharp criticisms of philosophy, but I would not be understood as believing that there is no place for it in life or in education. The practical problems of education so greatly exceed those for which there is a scientific answer, even a first approximation, that some temporary rule is needed, and to provide this should be the service of philosophy. To do it with least likelihood of error and in such a manner that error does not become dogma, demands a certain kind of philosophy. In my judgment it should be modest, humble, cooperative with science, and imitative of

[1] See review by H. T. Stetson of Sir James Jeans' "The Mysterious Universe," 1930, appearing in *Science*, January 16, 1931.

it so far as possible. It should be modest in its endeavor, and should not attempt thought syntheses beyond the possibility of experimental verification. It should be humble, and should recognize that its conclusions, one and all, are tentative—to be supported, overthrown, or modified by future findings. It should be cooperative with science in the sense that it takes upon itself a part of the larger scientific process, that part represented by the mental elaboration of the tentative solution of a problem. It should be imitative so far as possible of science, in the sense that it should adopt concepts of measurement, concepts of probability, and most important of all a terminal questioning concept to be satisfied only by new evidence.

A philosophy of education having these characteristics should be of great service in providing the young and immature with guides and rules of action in the matter of the purposes and methods of education, which they can use pending their own experimental discovery of better rules, and the discovery of better rules by scientific educators of a later generation, which the modest, humble, cooperative, and imitative philosophers of that generation will quickly incorporate into the doctrine that they believe and teach.

CHAPTER VII

THE CONFERENCE METHOD OF FINDING THE TRUTH

THE casual observer of the processes of education of this age must feel that they proceed with an inexorableness which inclines one to believe that they have a certain and enduring warrant. One has but to visit a poor or backward community and note the hardships that are endured, the sacrifices that are patiently met, that children may get a schooling, and to note the unquestioning acceptance of the curriculum offered and the great faith in its value, to feel that such schooling either is a mighty blessing or a sad delusion. The blind acceptance of the traditional processes of education, which is so apparent in the struggling community, commonly becomes a quasi deliberative acceptance in the more favored communities. This is so characteristically the case that it is surely a misfortune if there is any systematic and inherent weakness in the social processes that yield this outcome. We may look to the school board, the responsible authority for the curriculum, as the channel whereby tradition in school matters wittingly persists into the present. The city, county, or state superintendent of schools may be the expert advisor of the board, but as this officer is ordinarily far more versed in the historical and contemporary practices of education than in the experimental verifications of educational outcomes, he may perhaps be an expert upon mores but hardly more than that. He may be considered a valuable coordinator and check upon a lay deliberative body, but ordinarily he does not change the nature of the processes

whereby such a body reaches conclusions. Accordingly the board usually carries on as its predecessors have done or it follows the recommendations of some representative committee brought to its attention by the superintendent in making a change in curriculum or method. It is particularly appropriate to inquire into the sources of knowledge and the nature of inspiration of such groups as influence the board and school superintendent. In the following discussion of conference action attention is directed to the deliberative committee issuing a set of recommendations, and not to the fact finding committee or the executive committee.

The sound developments in education ordinarily have not been sponsored in the first instance by deliberative bodies, committees, boards of education, university trustees, or legislatures. The deliberative body may write a creed, a hymnal, or prayerbook, but not inspire a great religion. In the origin of things some individual, some great idea has struggled for expression. Later the high priests congregate, accept the idea after formalizing it nearly beyond recognition, and pass it on and actually use it to suppress new ideas, called heresies, some of which express the highest wisdom of the new age. There have been potent deliberative bodies in education. A few recent ones may be mentioned, the Committee of Ten, appointed by the National Education Association in 1892, the Committee of Fifteen, appointed by the National Education Association in 1893, the Committees of Seven, of Eight, and of Five of the American Historical Association, and the Commission on the Reorganization of Secondary Education in 1918. What have these committees sought to do?

The following quotation from the report of the Committee of Ten (American Book Company, 1894, pages 11–12) reveals its own hopes. "In every conference an extraordinary unanimity of opinion was arrived at. The nine reports are characterized by an amount of agreement which quite

surpassed *the most sanguine hopes* [italics mine]. . . . The unanimity developed is very striking and should carry great weight." The Committee is entitled to define its sanguine hopes as hopes that everybody will agree, but when realized, agreement is just what is gotten. Had the hopes been for the discovery of some great truth, would the Committee have spoken of, or been concerned with, agreement? It is not that, but just the opposite, that is characteristic of newly discovered truth.

The Committee states:

> The Council and the public will doubtless be impressed, at first sight, with the great number and variety of important changes urged by the Conferences; but on a careful reading of the appended reports it will appear that the spirit of the Conferences was distinctly conservative and moderate, although many of their recommendations are of a radical nature.

For a distinctly conservative and moderate spirit to lead to recommendations of a radical nature is a contradiction in terms and actually, as the reader of the report can verify, it is the first view and not the last which does characterize the report. This is not only the case with this Committee but it represents the only type of valuable outcome to be expected from such a committee. More radical and more true ideas may be expressed in committee discussion, but they are not endorsed and it may even be defended that they should not be, for a brilliantly true item in a report without lay followers may destroy the public confidence in the larger number of moderate recommendations the adoption of which would have a certain type of genuine value.

One of the members of the Committee of Ten, President H. Baker, submitted a minority report. The occasion for it may be made clear by quoting from the Committee report and by quoting President Baker's comment. The Committee wrote:

> Every youth who entered college would have spent four years in studying a few subjects thoroughly; and on the theory that all the subjects are to be considered equivalent in educational rank for the purpose of admission to college, it would make no difference which subjects he had chosen from the programme,—he would have had four years of strong and effective mental training.

President Baker commented:

> All such statements are based upon the theory that, for the purposes of general education, one study is as good as another,—a theory which appears to me to ignore Philosophy, Psychology and Science of Education. It is a theory which makes education formal and does not consider the nature and value of the content.

As nearly forty years have passed since these words were written we should be in a position to know whether the majority was "right" in its view. The developments of educational psychology of the past thirty years have been such that we may doubt if any committee of ten eminent educational psychologists, or, in fact, any one of them, would today favor the Committee's view in preference to that of President Baker. The aberrant Baker of 1890 proves a better guide to sound progress than the Committee consensus, but this is easily understood. Even now the issue, though in the process of being settled by experimentation, is far from settled, for much necessary evidence is still lacking. In 1890 what could a committee do but argue about it, and what argument could be more than merely plausible? Certainly President Baker could present no air-tight case. As the arguments in favor of a *status quo* seem more cogent than those opposed, and rightly so, for the *status quo* does exist, and it remains a question whether a new order can or cannot exist, we may regularly expect that the proponents of the temper of the times will silence the advocates of untried practices as long as verbal arguments are the weapons employed.

The insidious way in which tradition operates in committee work is well illustrated by certain introductory remarks of the Committee of Eight on the Study of History in the Elementary Schools, appointed by the American Historical Association and reporting in 1909. It wrote (p. v) that a majority of the Committee "were in actual touch with the work of the elementary schools," and again (p. ix) "We are persuaded therefore in presenting this final report that it cannot be said that we have reached conclusions hastily nor that it is the result of the working out of fine-spun theories on the part of college men." This last statement is probably a bit of gentle ridicule of earlier committees composed of college men that passed upon elementary and secondary school problems, but it is a boomerang as well. If their report is not the elaboration of fine-spun theories such as lie at the root of all progress except the purely fortuitous, what is it? Why, clearly it is a sampling of traditional declarations as to purposes, and of familiar and non-revolutionary practices. The one other thing that their study might have been, namely, a carefully conducted experimental investigation of the nature of outcomes of history study actually realized and of methods whereby realized, is nowhere intimated. In fact the Committee urges (p. xv) ". . . that a scheme like the present one . . . be given a fair trial." Think of it! The Committee makes a proposal for country-wide adoption before it has even been tried out. If the recommendations were not so prosaic this surely would be sound occasion for ridicule.

With what we may judge is an accurate appraisal of their special talents, this Committee commends itself for its closeness of touch with contemporary practices and past tradition, not for clear thinking and insight into the subtleties of the question of education for citizenship, and not for ability to add new evidence that will help solve this vital

problem. The Committee is touching neither end of the process whereby new knowledge is attained, (a) the keen hypothesis, and (b) its experimental verification. What is it doing? It is setting a norm or standard for things as they are in communities and from points of view assumed to be up to date.

In spite of this Committee's fear of fine-spun theories it naïvely makes a far-reaching assumption in the following words (p. xi), "Pupils in this grade [6] are not prepared to study scientific history in its logical and orderly development. But they are prepared to receive more or less definite impressions that may be conveyed to them by means of pictures, descriptions, and illustrative stories, arranged in chronological sequence." No evidence is given in support of the assertion that children in the sixth grade are not prepared to study history in its scientific aspects and in its logical and orderly development. This, however, is not a fine-spun theory. Not at all. It is tradition, so hoary and so untested that the authors show no awareness that it has never been verified. The Committee that made this recommendation, as one might expect, feels no hesitancy in giving specific advice as to the detailed subject matter of the appropriate curricula for each school grade.

Supporting data other than references to past courses of study and to superintendents' statements are entirely lacking. This type of evidence from the very nature of its historical origin and the non-research channels through which it comes is heavily loaded with tradition. It is scarcely conceivable that it could be otherwise.

What are the results of committee recommendations? In so far as they are effective their influence is of the nature emphasized in the following words which are those used by the American Historical Association Committee of Five upon the study of history in the secondary schools, 1911,

in referring to the American Historical Association Committee of Seven of 1899 (p. 3): "From one side of the continent to the other courses were fashioned with deference to its recommendations. The report of the Committee affected not merely the curriculum but also the methods and even the aims of history teaching, and its natural result was to bring about, or help to bring about, the establishment of substantially similar curricula in a large portion of the schools the country over." This reveals committee effect at its best and also at its worst. It is a stabilizing agency and it is not a gadfly stinging contented school folk to new efforts to improve and to discover new truth.

In the give and take of conference discussion upon broad policies, original ideas are sharply criticized and then as a result a compromise subsumed under less original ideas which are less severely criticized, and the process repeated until such general "truths" are reached as "stand up under the penetrating criticism" of the committee process,—such truths as the committee writing "Cardinal Principles of Secondary Education," 1918, recorded:

> (Page 9) The purpose of democracy is so to organize society that each member may develop his personality primarily through activities designed for the well-being of his fellow members and of society as a whole.
>
> (Page 9) Consequently, education in a democracy, both within and without the school, should develop in each individual the knowledge, interests, ideals, habits, and powers whereby he will find his place and use that place to shape both himself and society toward ever nobler ends.
>
> (Page 10) This commission, therefore, regards the following as main objectives of education: 1. Health. 2. Command of fundamental processes. 3. Worthy home-membership. 4. Vocation. 5. Citizenship. 6. Worthy use of leisure. 7. Ethical character.
>
> (Page 19) Consequently we recommend that secondary schools admit, and provide suitable instruction for, all pupils who are

in any respect so mature that they would derive more benefit from the secondary school than from the elementary school.

These statements are all bold face or italic material in the published report. They are the high spots of the report, the reason for the Committee's existence. The nearest the Committee came to making a recommendation not universally approved is in the following statement, but it saved itself by the last half dozen words:

(Page 30) Consequently, this commission holds that education should be so reorganized that every normal boy and girl will be encouraged to remain in school to the age of 18, on full time if possible, otherwise on part time.

In short, so far as the writer can judge, this is a perfect committee report,—this last recommendation has an abandon about it that is the pinch of salt in the bowl of substantial gruel.

There is little reason to believe that committee action is as keen, as forward looking, as bold and stimulating, as the views of the members separately, except perhaps the views of such good fellows as get on committees because of their genial dispositions and inability to have provocative ideas. Why should the Committee of Ten pride itself upon having exercised great care to secure a representative *geographical* distribution of members of its nine conferences? Undoubtedly the argument was that if the Committee's deliberations were to be respected throughout the country, they must be the product of country-wide representation and must go back to the various geographical regions of the country, inspiring the confidence that local educational leaders arouse. This is a characteristic committee view, but what has it to do with truth? The important question as to whether the ideas which will later be agreed upon by the members of such a conference will be worth passing on or back to the

local communities is left to the future, or at least takes a minor place as the very existence of the geographical consideration proves. Even where the vision of some student, where the great passion of a mind or two to rend the veil of ignorance and then spread a glorious new gospel, has led to the conception of, and the calling of, a committee, still even here by the time the committee is actually formed it is likely to be dominated by the desire to spread the gospel, not to discover one. Sir Francis Galton voiced his confusion and regret that his ideas of eugenics became distorted and uncritical propaganda in the hand of some groups though composed of his friends. Though these are not his words, it seems fair to say that the evangelical attitude of the group conscience grieved him.

The larger the committee or conference and the more close it lies to reigning authority, the more does it tend to degenerate into a selling agency before it has anything to sell, believing with the usual assurance of salesmen that it really will have a very fine product. The seer, the sundry capable members of the committee, and we may expect a majority to be such, have their visions. These are worn down by attrition, and the committee mind appears: first the desire to sell an idea as yet unborn, then the travail of compromise that some idea be finally delivered, the mixed pride and shame of, but withal loyalty to, conferees, and finally trust and distrust in the outcome, perhaps even a growing distrust of one's own best convictions.

A committee called to standardize the sizes of bolts and nuts used in machine work has a specific and well-established purpose antecedent to its first session. Its success is to be measured in terms of the thoroughness with which its recommendations meet the needs of the present or the immediate past. The greater the prestige and authority for the question in hand of the conferees, and the greater their word

carries conviction, the better the outcome. Compromise,—a thread dropped here, an octagonal head there, etc.,—is serviceable provided always it is not so great as to alienate the affections of the most potent conferees. That such a conferences has value and that it is about the only way the desired purpose can be brought about seem rather obvious, but how different the problem here set this fact finding committee and that confronting, say, a committee called upon to discover new values and new procedures. If a group deliberates and attempts to make decisions upon the nature of the values of, and the best methods of, teaching Latin to high-school pupils, it can proceed just as the nut and bolt committee and act upon the assumption that past judgments of value and past practices are a safe guide to the needs of the future. Proceeding thus, it would probably reach a consensus that would carry weight, and influence future practice, but has it solved the problem set? Certainly not, for this problem is not to be solved by a consensus but by investigation. Even should some member of the Latin committee know the solution with an excellence quite beyond that of any other member, it is difficult to conceive of any process of committee action which would cause this man's opinion to stand out above the rest and thus become the committee's action. Will anyone claim that there is any correlation between the truth in such an issue as this and between the self-assurance, vigor, and the beauty of the verbal bouquets with which ideas are voiced in committee? Of several views upon this matter, the excellence of the more true in comparison with the less true lies in its ability to survive when tried out, to demonstrate its reality when put into practice and tested, and in nothing else, and this process of testing is, of course, not available during the sessions of a conference. It may not be available at all or not within any reasonable length of time if tests are not devised to

measure its sufficiency. For example, if the proposition that the study of Latin constitutes general mental development be accepted by a conference, and if courses of study are built upon this assumption, but if no experimental study be devised to measure the mental development consequent to it, the proof of the proposition cannot be said to be established by practice, though many years pass and though thousands of schools act upon it. It remains an undisturbed, but not a proved, hypothesis. Only occasionally is a committee proposal so cacothenic in the field of life as to lead to its own demise, and thus prove its falsity. Generally an exuberant society such as ours can tolerate much inefficiency and lost motion, and still survive, so that we may expect erroneous or non-tested elements in our tradition to persist almost indefinitely. They may be in fact the orchids of the mental life of a society which is non-cognizant with the difference between the parasitic and the self-sufficient in economic, political, and social life.

If it is charged that it is unfair to criticize conference action for failure to be original, the writer's purpose will have been accomplished: there are certain essential things that a conference should not be expected to do. It is hardly to be gainsaid that there remains an important function for the conference.

It presumably is valuable to establish standards differing slightly from average practice in the direction of the consensus opinion of eminent men, for such men do commonly sit upon committees. Certainly such a consensus is more likely to be valuable than one of men about whom all that is known is that they are not eminent. The method of creating committees is sufficiently varied that no generalization about deliberative committees should be expected to be without exceptions, but the reader familiar with committee reports upon education and social issues which have

flooded the country in recent years can judge if there are many exceptions to the following: committee reports give a conservative and average view of things from which divergent practices, except as they are anathematized, are rigidly excluded; they always have something to "sell"; they are indefinite as to the causal connection between purposes and procedures; where specific they are rather more likely to be wrong than right, as judged by the standards of a slightly later generation; they are the source of "inspiration" of non-original school administrators and public officials; they give evidence that their authors have employed in part and unsystematically the methods of historical research but not the methods of experimental research; and they are slightly nauseating with platitudes.

The greatest danger of deliberative committee reports is that they will be widely and indiscriminately followed, that they will blight local initiative, that they will hold up to scorn divergent practices and tend to lead to their suppression. This outcome is in a sense vouchsafed by a philosophy and social structure that proclaims that the majority rules. Why should the majority rule if the minority wishes to indulge in some harmless experiment of its own? Why it should not rule has been forcibly expressed by Supreme Court Justice Brandeis in his dissenting opinion, New State Ice Company of Oklahoma City versus Ernest A. Liebeman.[1] He defends the right of a state to determine its own policy and dissents from the majority decision in these words:

> . . . The discoveries in the physical sciences, the triumphs in inventions, attest the value of the process of trial and error. These advances have been due, in large measure, to experimentation—which for two centuries has been not only free but encouraged.
>
> Some say that our present plight is due, in large measure, to the discouragement to which social and economic invention has

[1] Quoted from *Time*, April 4, 1932.

> been subjected. I cannot believe that the framers of the 14th Amendment, or the States which ratified it, intended to leave us helpless to correct the evils of technological unemployment and excess productive capacity which the march of invention and discovery have entailed. There must be power in the States and the nation to remold through experimentation our economic practices and institutions to meet changing social and economic needs.
>
> To stay experimentation within the law in things social and economic is a grave responsibility. Denial of the right to such experimentation may be fraught with serious consequences to the nation. It is one of the happy incidents of the Federal system that a single courageous State may, if its citizens choose, serve as a laboratory; and try novel social and economic experiments without risk to the rest of the country. This court has the power to stay such experimentation. We may strike down the statute embodying it on the ground that, in our opinion, it is arbitrary, capricious or unreasonable; for the due process clause has been held applicable to matters of substantive law as well as to matters of procedure. But in the exercise of this power we should ever be on guard, lest we erect our prejudices into legal principles. If we would guide by the light of reason, we must let our minds be bold.

Just as surely as the Brandeis view becomes social policy and just as surely as experimental science comes into its own in connection with social issues, so surely will the place of the deliberative committee recede from its present position of dominance in establishing the purposes and methods of American education to that of subordination to investigation and to serving as a means of collecting and diffusing knowledge of extant practices.

CHAPTER VIII

THE BEARING OF RECENT SCIENTIFIC DEVELOPMENT UPON PROBLEMS OF EDUCATION AND INHERITANCE

EXPERIMENTALISTS in curriculum construction are as yet but few in number, so it is only a slight exaggeration to say that from the dawn of history to the present day curricula have been constructed solely upon the basis of the opinions of individuals or of groups in authority unsupported by experimental evidence. During the World War an army school for mechanics was established in Brooklyn. A person connected with the instruction in mathematics informed me that 90 per cent of the mathematical content listed as necessary was, upon investigation of the activities of army mechanics actually successfully at work, found not to be essential. The curriculum was radically modified, with the result that many more men were quickly made effective fighting units. Seldom do we have such an urge as a great war to stir our self-complacency with our current educational practices.

Someone may say the 90 per cent not needed for the immediate activities of wartime mechanics would be valuable for the activities and cultural life of similar people in times of peace. As to this I shall remain skeptical until proof is forthcoming. I cite this army school situation not to argue for more or for less theoretical mathematics in school work, but to illustrate two types of process which may be employed in the building up of curricula. A certain content may be included (1) because knowledge of it is demonstrably needed,

or (2) because it presumably will be needed, in the planned-for future activity.

It is surely true that intelligent and experienced adults can judge more wisely than children of what knowledge will be needed in later life. In granting this, does one grant the correctness of a curriculum drawn up upon the basis of the opinions of experienced adults? It is to be noted that this has been the traditional method. Has nothing happened enabling an improvement in it? Is it to be the method for eternity? In other fields of life, history records an expropriation from judgment and an appropriation by science of one domain after another. Medicine has changed from an exhortation of evil spirits to an experimental science, astronomy from the interpreting of horoscopes to celestial physics, psychology from philosophy and belief in obsession to a laboratory science and an experimental study of individual differences. At every step an earlier prerogative of speculation has been reluctantly given up to observation and experiment.

Carefully note if you will that this yielding of ground by speculation has not narrowed the field in which to speculate, for at every step new and greater vistas have been opened up. The reluctance to yield ground has been due to the fact that generally the particular individuals who have had their speculative wilderness expropriated have been different individuals from those who have appropriated it, charted it, and in penetrating it have found a deeper, darker, and altogether fascinating wilderness beyond. We need not fear when we turn a domain over to measurement and statistics that we have lessened the opportunity for the mind to reason and to ramble. Does anyone believe that ten thousand, one thousand, or one hundred years ago there was greater opportunity for these things than there is today? Why, it is but a few years that we have had that limitless domain of non-Euclidian geometry in which to roam.

Let us then fearlessly approach the issues of curriculum construction and, though we rob some educational patriarch of the speculative activity that is closest to his heart, we should note at the same moment that we are simply clearing away detritus and exposing an underlying vein offering more profitable and more extensive fields for development.

One who is familiar with certain recent enumerations of geographical and historical allusions found in current educational literature, will surely know of the invective directed against the use of such information for purposes of determining the proper content of the elementary school curriculum. Typical arguments are that such data would give Dayton, Tennessee, great geographical importance and Tom Heflin a great political personality, etc. This would be true if there is abrogation of the judgment, when one becomes aware of facts in a situation earlier devoid of them, but no such abrogation is called for. If one author, in writing a political history, believes that Elihu Root should receive more attention than Tom Heflin, does the fact, say, that Tom Heflin's name appears ten times as often in certain newspapers as that of Elihu Root, annul this judgment?

The issue connected with knowledge as to frequency in current usage of historical and geographic facts is the same as that connected with knowledge of word frequency. Thorndike's *Word Book* gives the one hundred, two hundred, five hundred, one thousand, two thousand, etc., up to twenty thousand most frequent words in current English usage, as determined from a very comprehensive investigation. There are also several other word counts which can be used. Does this information do away with the exercise of judgment upon the part of the author of the elementary school reader? It certainly does with reference to certain features of the elementary text. Without the *Word Book* the author would have to use his judgment as to whether a certain word was,

without specific instruction, within the range of comprehension of the pupils of a given school grade. With word counts at his hand this question is answered approximately by simply turning to the word in question and noting its frequency value. Thus one task of the elementary textbook writer has been taken away from him by the word count. This simply means that he can turn his attention to issues that are more worthy of his mental effort.

The old textbook not utilizing the word count is at a disadvantage in competition with the new one, and the author of the old book may perhaps ridicule the utilizing of word counts in constructing a reader. If this selfsame author would add to his present talents the knowledge provided by the word count he would be a still better author—wisdom will not hurt him—and he would maintain a place in the elementary reading textbook field; but as he seldom will do this a struggle ensues between the new and the old.

What has been said about word knowledge and reading is but illustrative of what is now taking place in every field of instruction from the kindergarten through the university. Though the outcome of the struggle is, I judge, certain, it may be long and bitter, and consume much of life's valuable time which should be devoted to more important matters. If we look at the matter from the viewpoint of earlier practice, the three following stages might be suggested: (1) All issues connected with the writing of an elementary school reader are settled by the judgment of the author. (2) An ever-increasing number are settled by scientific investigation, thus removing them from the province in which the author's judgment alone operates. (3) Finally a stage is reached in which nothing is left to the judgment of the author. These three steps would occur only to a man of narrow vision, but in such case it is no wonder that he will tenaciously cling to the first stage, fearing that any admis-

sion of experimental evidence is but an entering wedge which, if permitted, would finally demolish the ancient and venerable method that he stands for. Though I hold no brief for a method because of its antiquity, I should only be content to see such a method discarded in case a better arose in its stead. In this old method are to be found such good things as delightful fairy tales and classical stories, good English, good morals, and so forth. Would one substitute a word count for any of these, taking out, let us say, the infrequent but shivery word "ogre" and putting in the frequent good Republican word "prosperity?" Without answering yes or no, I will maintain that a better answer can be made with the frequency values of "ogre" and "prosperity" in mind than in ignorance of them. Here, as everywhere, to be afraid of knowledge is a cardinal sin against progress. With a mind awake to evolution we can readily see that the stages are not as given, but these: (1) all issues settled by judgment; (2) an ever-increasing number settled by experimental evidence, and as they become settled, an ever-increasing number of new issues arising which must be settled by judgment; (3) no third stage which is different in nature from the second. The problem has changed. That which in the first instance merely called for an act of judgment is now seen to be an intricate problem in science as well as judgment. The net social result of this change is an improvement in elementary readers with the resulting more rapid learning of school children.

Word knowledge was taken merely as an illustration. A similar story could be told involving individual differences, laws of learning, nature of growth, transfer of training, and so forth. How long has this fight between opinion and experimental evidence been going on? In the matter of curriculum construction it has hardly started. The determination of what constitutes an appropriate curriculum is a

problem of estimating the future—the issue being, what can be done now that will best serve the pupil in the future, immediate or remote.

In Chapter II, I made the observation that if the past bordered upon the future the only way to solve problems having future reference was by a study of the past. I then argued that the past does not border on the future, for the present intervenes, and the present is more than a point in time. It is a definite expanse limited by memory, aided by recorded fact or not, as the case may be, and containing verifiable elements of knowledge. This expanse is the home of the experimental method. Having located the issue we immediately see why it has been with us for so short a time. In the history of the race the experimental method in any field has been with us but a few centuries, and in the social sciences it is still an infant in swaddling clothes.

The first great need in providing a scientific foundation for curriculum construction is that we expand present time so that we may conduct experiments in learning, in individual differences, in continuity of interest, in transfer of training, and in the nature of pupil idiosyncrasies, over a long number of years. If our experiments of these sorts could be a generation in length, assuredly hundreds of educational problems now "solved" by judgment—it would be truer to say now embedded in ignorance and polished with opinion—would yield to objective treatment. In addition to lengthening the present-time span we should extend the range of issues attacked by experimental methods. As one example, consider the honor system, put in, modified, taken out of high schools and colleges, with never an attempt to determine in a demonstrable manner the effect upon the child, which is surely the most vital matter connected with the question. Our social heritage in this matter is voluminous, and it is of such long standing that we continue year

after year to attack this problem with opinion, it not even occurring to us that a new technique is possible and essential to intelligent progress.

The introduction of the experimental method into human thinking has as yet had a rather small effect upon the public school curriculum. What should be its effect may be properly considered under two headings: first, what should be its effect upon curriculum research, and second, what should be its effect upon the philosophy of the curriculum. The first has been briefly considered. The second is a matter of opinion and in this connection I wish to quote certain eminent modern scientists.

In order that my present emphasis upon the experimental method may not destroy a total view of human development, I wish to quote first the very fine view of history given by Henry Osburn Taylor in the following words: [1]

> Thus I have tried to set before you a layman's view, in which history shall not be mere narrative, nor merely the series of events forming the past; but shall incorporate and be the onward-striding thought, the interwoven tissue of event itself, the element of continuity without which nothing is or can ever have been. Every object in nature, every bit of science, every philosophic theory, every phase and kind of religion, and every constructive or destructive act of life, possesses the constituent of being and becoming which is time.

As elements in the "onward-striding thought" of man, history and experiment are united. Though judgment as the basis of values leans heavily upon a record of the past, it should not be thought of as synonymous with history, being rather in the mental field what custom is in the field of conduct. Just as custom is measurably less than past practice, so opinion may be a very inadequate reflection of the history of thought. Such contrasts as have been drawn between

[1] "A Layman's View of History," *Science*, Vol. 67, No. 1731, March 2, 1928.

judgment and experiment as bases of curriculum construction are not contrasts between history at its best and science. They are rather contrasts between general impressions of what the past has bequeathed, and experimental observation.

Perhaps an illustration will assist in making this point clear. A consensus of opinion of people who have thought about capital punishment would yield an average result with wide variability from this average. Probably somewhere, not at the median of this distribution of opinions, will be found a view actually best interpreting the forward-striding of mankind—this is the interpretation, at its best, of history. Ordinarily we cannot pick this one view out from the many others, and are compelled to use a median view as the best available standard, though it depart quite radically from the best interpretation. Expressed in another way this is equivalent to saying that the standard (the median view) for this epoch is difficult to ascertain because of the variability in view, and when ascertained is of transient value because of the rapidity of social growth. In the case of the objective experiment, the variability of the results is ordinarily small, so that the use of the mean result will be a close approximation to the true result.

Still another distinction between judgment based upon history and current impression and experimental observation is important. The two things serve different purposes. For example, let us gather a consensus of opinion as to the status of transfer of training between certain school subjects, and second, let us conduct an experiment upon this. The specific thought of the past upon this subject, leading both to the present consensus of opinion and to the recorded history of it are probably so different in their general scope from the issue attacked experimentally that comparison is difficult. The experimental investigation, by its change in emphasis,

may actually change the problem, undermining the historical and the judgment values just as was done when alchemy was superseded by chemistry. In addition therefore to the hazard in utilizing opinion, due to the variability of judgment, there is also a hazard due to uncertainty as to the relevance of the issues as determined by judgment. No comparable hazard as to relevance is present in the experimental method, which involves a testing-out step so that the experimental issue is supported or not supported by the experimental findings.

At its best judgment, unsupported by experimental evidence, is the future reference aspect of history. The connection between issues suggested by judgment and those arising from experiment is intimate and mutual. One important aspect of experimentation is a verification of the deductions of judgment, and as such it bears much the same relationship to judgment that experimental does to mathematical physics. Let me quote Dr. Edwin B. Wilson upon this point: [1]

> Let us for a moment consider what is the function of mathematical physics. In a certain sense we get out of mathematical physics only what we put in. This is a purely mathematical implication and means merely that if our mathematics is watertight every conclusion must follow deductively from the premises. But in another and more physical sense we may and we ordinarily do hold that when we interpret our mathematical conclusion as a fact of nature we get new physics. In this way Hamilton got conical refraction. . . . Many other instances of obtaining from mathematics new physics could be adduced, and also many, perhaps more, instances of obtaining too much or too little. It is the interpretations that determine the value of mathematical physics and that make it as Darwin pointed out in 1912 a more exacting science than pure mathematics.

[1] "Some Recent Speculations on the Nature of Light," *Science*, Vol. 65, No. 1681, March 18, 1927.

The relationship here pictured between mathematical physics and experimental physics is the ideal relationship to be brought about between judgment and experimentation. Mathematical physics becomes established after its deductions have been verified by experiment, and judgment as to appropriate processes, methods, and values should likewise become established only after experimental verification.

Until such time, judgment may determine conduct because no better method of determining it is available, but it should be realized that it does so on sufferance only. Just as mathematical physics is a more exacting science than mathematics, so judgment that stands up under the scrutiny of experiment is a more exacting type than judgment not called upon to do so, which latter is ordinarily understood by the term judgment, or opinion.

It has been mentioned that one important aspect of experiment is the inferences to which it leads. Of the innumerable illustrations of this, few are more striking than the discovery of X-rays by Roentgen. I quote from Lafayette B. Mendel: [1]

> [Roentgen] was not searching for a method of penetrating the interior of the body with light; rather following the studies of Lenard and Herz he was engaged in consideration of radiation from the seat of electric discharge.

Before the discovery of X-rays, it is inconceivable that any scientist, though fully informed of antecedent history, should set itself the problem of discovering X-rays. History, though complete, and judgment, though excellent, would have been inadequate for the attack upon this problem. The inferences that gave us this wonderful new tool in physical and medical science came directly from experiment and observation. Its discovery is not just a piece of good luck. It is characteris-

[1] "Some Tendencies in the Promotion of Chemical Research," *Science*, Vol. 65, No. 1693, June 10, 1927.

tic of the outcome of experiment by one who observes carefully and infers with sagacity. It requires a special ability and outlook to see the significant aspects of an experiment. As Lafayette Mendel has said: [1]

> Without a background of facts thinking becomes a difficult, if not a futile task. A rich store of fundamental facts is the indispensable equipment of what Pasteur so expressively termed the "prepared mind." Many persons are privileged to make chance observations: only the prepared mind profits by them.

When we attempt to incorporate scientific method as a subject matter into a curriculum, it is plain that facts, many and well ordered, must continue to constitute an appropriate content; but the handling of them, the reasoning with them, the drawing of inferences from them, and the devising of experimental tests of inferences is something new in the curriculum, something which cannot possibly in the world's history antedate the development of the scientific method. Is it not safe to say that whatever the value of a technique of instruction prior to the time of society's becoming self-conscious in the matter of scientific method—and we may ask if that time has yet come—the value is not that it develops a scientific type of thinking?

As an illustration of the inability of the average or above average students to observe, infer, and make a few simple generalizations about phenomena, I report an exercise in concept formation given to a university psychology class. The subjects were not inferior college students, but the process was so foreign to their mental habits that they were quite lost. Four shorthand symbols called "ray," "hay," "kay," and "lay" were presented, one at a time, on flash cards in figures or outlines with added circles, loops, and lines. As each card was presented for a few seconds, it was named by the experimenter "ray," "hay," "kay," or "lay,"

[1] *Ibid.*

depending upon which of these four shorthand symbols was present somewhere in the simple figure. The only instruction given to the students was that they were to observe carefully so that they themselves would shortly be able to name the outlines. Every little while in the process of experiment the opportunity was given for them to attempt to do so. Just as rapidly as a student abstracted from the figures shown the four essential features that led to the naming of them, he was dismissed from the class, having solved the problem. The essential lines were not deeply buried in a complex figure. Most of the outlines were the essential elements themselves, plus a single added figure, such as a preceding circle or a following loop. For example, "kay" was a straight horizontal line. Outlines called "kay" were this horizontal line with a circle on the end, or with a circle in front, or with a loop on the end. Just as soon as this one feature, or horizontal line, was abstracted out of the situation presented, and all outlines containing this feature called "kay," the problem was solved. It seems simple enough for the kindergarten. Perhaps it is. Perhaps, as John Dewey implies, we should get greater success before the child's natural reasoning tendencies have been blunted by school discipline. However, for this group of average or above average college students the problem was too difficult. It had been intended to finish the experiment and its discussion in a two-hour laboratory period. At the end of four such periods three-fourths of the class were still unable to name the outlines. If this seems unbelievable to one, I ask that he repeat the experiment and satisfy himself.[1]

The conclusion I draw is that the students in question had never had even the simplest training in careful observation, in the drawing of inferences, and in the verification of

[1] Considerable care must be taken to insure that those who have solved the problem do not inform the others.

hypotheses; in fact, that they had probably had training antagonistic to these things. I judge, therefore, that they were as ignorant of the scientific method as though their pre-college training had been directed toward the trivium and the quadrivium.

In view of the present state of the curriculum and the techniques employed in such slight modifications of it as occasionally take place, does it not seem that there has been a pitifully slight transfer from racial knowledge of the scientific method to elementary school practice? I have not intended to picture tuition in the scientific method as a simple matter. We shall not know how difficult it is until we try, and when we do try we shall have an entirely new set of standards whereby to judge youth and wherewith to pick our future men of science. It is just about as reasonable to think that training in music will pick good mathematicians as that training in book knowledge, dependence upon authority, and rule of thumb, will select observers of natural phenomena, ingenious devisers of hypotheses, and severe critics of what constitutes proof.

Some progress has been made. The philosophy of Jan Smuts, and to a somewhat lesser extent that of John Dewey, does support a training in scientific method as have no older philosophies. However, in considering the bearing of science upon education, the as yet unrealized benefits of a common knowledge of scientific method should be placed well in the foreground. A vigorous adaptation of the curricula of certain experimental schools to this end, followed by a thorough investigation of the results thereby attained, might revolutionize our teaching process in a generation or two.

Probably one result of a standard of measuring pupils which involved their level of attainment in the scientific method would be a realization, only dimly sensed at present,

that different pupils should be differently trained. Were it established, as I believe it will be some day, that native capacity and appropriate training for the man of science are radically different from those for the man of letters, society might adopt dual or multiple standards of excellence in adults and in types of training for youth. True, such an outcome would complicate our philosophy of life, but let it, if it leads to a truer picture. There is no virtue in preserving a simple philosophy at variance with the facts. The facts that bear upon this fundamental issue suggest that there are several different kinds of mental capacity, several different achievement growths, and an equal number of appropriate types of training, and even an equal number of psychologies of learning. To create a great mathematician and a great musician, we should probably start with two individuals of very different native talents. We should give each a training having, in addition to many common elements, a very considerable number of different elements, and the psychology of instruction of these different elements would probably be very different. The common elements in the two training processes are such things as reading, United States history, honesty, courtesy, and so forth. The different elements are ear training, vocal and manual production of music, musical appreciation and especially musical composition, and on the other hand, a training in the scientific method, in mental manipulation of spatial relationships and of quantitative concepts, in sustained attention, symbolic thinking, and logic. There is no *a priori* or experimental foundation for thinking that a single set of laws of learning would apply to these disparate activities, so that we should anticipate that different psychological principles, at least a radically different stressing of the factors conditioning learning, should be employed in the different educative processes.

On the basis of earlier work already reported in print,[1] I venture to assert that a half-dozen or more fields of life are as discrete in their mental demands as music and mathematics. Although we need much more experimental evidence as to independent mental traits, it is now time that we construct a philosophy of education permitting of them. The religion and philosophy of India are cordial to the idea of different goals for different people, but, generally speaking, Western philosophy and religion are not. Too many centuries have been devoted to teaching that all men are or are not directed toward one single goal, and further, this goal has been so remote—the Day of Judgment, or thereabouts—that it has been impossible to devise any experimental check to see if the doctrine is sound. It likewise is impossible to determine that it is not sound. There is no harm in believing in a single goal, providing it does not, as it has in the past, lead to a withdrawal of men's interests in the immediate issues of a present lifetime, and in the existing social structure.

It has always seemed strange to me that modern philosophers[2] familiar with the doctrine of evolution, aware of biological evidence that innumerable species that have lived in the past are now gone forever, and that the universal story of life is that of branching, growth, and then more branching, and so on and on, with no evidence of termination, should, in the case of man, reverse the process, and picture development as toward a single goal—perfection of some single sort. The idea that human perfection is a single thing exists only in the mind of man, for no experimental evidence supports it. Experimental and observational evidence can give no light upon an ultimate placed in the

[1] Kelley, T. L., *The Influence of Nurture upon Native Differences*, 1926. *The Interpretation of Educational Measurements*, 1927. *Cross-Roads in the Mind of Man—A Study of Differentiable Mental Abilities*, 1928.

[2] I would not include Jan Smuts in this criticism.

remote future, but it does give us much light as to a past and present tendency. It would seem wise to heed it in building up an outlook for the present, and the immediate future.

The complexity of modern life and the specialization required of leaders in most fields of activity are such that present society could not exist if the doctrine of uniform development were strictly adhered to. Require a child to develop equally in the various fields of science, in engineering, in social and cultural activity, and what would we have in the end? Though he were one in a thousand in ability and versatility, we would merely have a delightful jack-of-all-trades, who would make a fine patriarch for some lost and retarded tribe on a distant island, but he would be of little use in the work of a big city or in the defense of a nation in time of war. His chemical warfare, his prophylaxes, his art of communication, his military strategy, and so forth, almost without end, would be so inadequate that his quick annihilation would be certain, and a nation composed of such as he would promptly vanish from the face of the earth. The doctrine of uniform development is untenable, for the race that would adopt it would be no match in peace or war for one which developed specialists.

Generally speaking, the practice of the nations of the Western world is to reward specialists; and their philosophy, as reflected in their public school curricula, is to develop paragons of uniformity. It is time that the right hand knew what the left was doing. There is no need to believe that, once the philosophy of uniform development concedes a step, that there will be no stopping point short of the other extreme, complete specialization of training. The hazard of this for social stability is evident. It lies in the difficulty of securing unity of action from a heterogeneous group.

Let us, for argument's sake, suppose that the goal of human life is single, and that of the many mutations as

represented by specialization, one only is fit ultimately to survive, the rest being destined to die. Would it not be a calamity if in our endeavor to select the perfect man we chose the wrong mutation, eliminating the rest, including the one that in truth was the only one fit to survive? Shortly the one mutation that we fostered would die, for it intrinsically is not stable, and the human species as a whole would become as extinct as the dodo. The disappearance of a species in the past has been sufficiently frequent to suggest such a possibility for mankind. Mankind, however, has one great advantage over those forms of life that now no longer flourish. Man has a mind with which he can partially foresee the consequences of proposed conduct, and in the light of such pre-vision alter conduct before it is too late. Is not foresight his greatest blessing and promise of racial survival?

I have drawn certain implications for education from our knowledge of science, particularly such as pertain to individual differences. What are some of the implications to be drawn from it for racial betterment by eugenic breeding?

We should first recall some of the momentous items of racial knowledge bearing upon human generation, heredity, and improvement of racial stock. The first item and, I should judge, the most momentous of them all to human thinking, is the knowledge that sexual intercourse is the cause of offspring. When this idea first took form in the mind of primitive man, there must have been a feeling of kinship with God, of potency, of mastery over the very sources of life, that consciously put him above the brute. Some such kinship with the Lord of Creation must have welled up in the mind of man when he first controlled fire and became master of the living flame; such kinship as, in historic times, in the case of Newton, who was nearly overwhelmed with the idea that one law, and that within his ken, linked the motion of the heavenly bodies with that of objects upon

the earth about him. Truly the knowledge of conception and the part that man and woman play in it is a mighty story that every child should be told reverentially at his mother's knee that its initial coloring be not that of the lurid light of the alley.

Perhaps primitive man was disappointed in that he could not use this great knowledge for the immediate control of generation, but instinctive passions, long antedating his intellectual knowledge, continued their rule. Probably in his attempt to give the mind a better chance to be master of passion he put on clothes, and now, after an interval of untold thousands of years, he has learned that by judicious continence or the use of other contraceptive devices the old rule need no longer operate, and the clothes are coming off again, at least so it seems. At any rate the knowledge of contraception profoundly affects man's outlook. It gives him a sense of mastery, of ability to cope with human procreation not possessed before in historical times. Coupled with this knowledge of contraception is further knowledge in part ominous, and in part reassuring. It bodes ill for mankind to be assured, as much evidence indicates, that thus far contraceptive practices have been more extensively employed by that stratum of society that we might well most wish to increase—by college graduates, by the well-to-do, and by men and women in professional walks of life. Let us hope that some day, not too late, this nation will awake to the seriousness of this situation, and take the steps well within its power to change it.

The reassuring information is found in the very substantial body of knowledge explaining the mechanism of heredity. If the rules of breeding are known, then it only requires the will to do in order to progress in the direction the mind may dictate. In the next chapter I shall discuss the use of analogy in scientific work. In the matter of the mechanics

of heredity we can say "as in the animal kingdom, so in the human." There are now too many specific verifications of this view to make it reasonable to longer doubt it. Of course we shall wish to test it upon every important issue, but meanwhile the presumption with reference to an as yet not tested point is altogether in favor of the view that the same mechanism maintains in mankind as in the lower animals.

Francis Galton was the great leader in showing the application of the principles of heredity to mankind. He demonstrated the principles of regression and variability as characterizing heredity in the human species, and he found a regression coefficient between offspring and parents of about .5. Let me illustrate this in the case of height, and to make the illustration simple, let us suppose that the average height of men and women is the same, and is 5 feet 8 inches. Then Galton showed that the average height of offspring of parents 4 inches above the racial average is 2 inches above the racial average, or the average height of offspring of six-foot parents is 5 feet 10 inches. However, these offspring will vary considerably about their own average. Similarly the average height of offspring of five-foot-four parents is 5 feet 6 inches, and these vary considerably about their own average. Let us merely for the sake of illustration suppose that height is a desirable trait, that the more of it the better, and let us proceed to breed with the purpose of augmenting this feature. To start with we will say that the party platforms of the Republican and Democratic parties each affirm that the parents of the next generation shall be 6 feet tall, and even the Socialists agree, so it is law, lived up to and enforced. If you can imagine all of this you see that in a single generation, the mean height of the citizens of the United States is raised 2 inches—probably a greater change than has taken place in the race in historical time. There is no more reason to doubt that this would be the

outcome than to doubt that dogs give birth to puppies, and cats to kittens. Further, this increase in height is permanent. If, after the first generation, all bars to mating are removed, and random mating takes place, the mean height of succeeding generations will remain 5 feet 10 inches.

No one familiar with genetics will doubt this statement, but some opponent to eugenics may say "All well and true, but you cannot continue the process indefinitely, raising the mean height of the race every generation by 2 inches, and thus obtaining in five hundred years a race of 8 or 9-foot giants." For reasons too intricate to explain here, this criticism may be taken as based on a sound conception of inheritance in a stable population. I would, however, give three reasons why it should not lessen one's trust in the possibility of eugenic breeding.

First: to take the first step, that represented by raising the mean height 2 inches, has been, according to our hypothetical illustration, an unmitigated good, whether anything further is done or not. There are hundreds of such first steps that society now would endorse; for example, people are agreed that a physique able to resist tuberculosis, as contrasted with one not so able, is good; to have keen vision rather than faulty is good; to have a capable mind rather than a feeble one is good; to have a sound body rather than a deformed one is good; and so on. Such first steps as these, if realized, are sufficient in number to remake society. There would be a greater difference between this new society and the society we now have than between the Greeks in their glory and the Eskimos in their squalor. Thus the first step alone is worth the taking, though it should lead to nothing further, but this is not to be expected.

Second: after the first step has been taken, society can reappraise itself and consider of the next step. After the height of the race has been raised from 5 feet 8 inches to

5 feet 10 inches, it may decide that the eight-foot ideal is not a worthy aim after all. The next generation is more competent to decide than the present, so leave it to them.

Third: if they decide that continued increase in height is desirable, they can look for it in two ways. In the first place, by the further application of the selective processes, which produced the first increase. This would tend to raise the mean, but not beyond a certain upper limit, if simple Mendelian principles, as they affect regression and variability, hold in the matter of height. In the second place, they could raise the mean by seizing fortuitous mutations and by breeding future generations from them. Now we do not know very much about the rules governing mutation, but is it not axiomatic that a mutation from a six-foot race is more likely to yield genes that produce a six-foot-six race than that a mutation from a five-foot race will do so? In short, whether from the viewpoint of immediate benefit or of progress toward a remote goal, the taking of the first step is justified.

I have used height in my illustration because it lends itself readily to quantitative discussion, and not because I think it is an important eugenic feature. I believe that one ground for criticism of eugenics is the fear that the criteria used in the selection of parents will operate against certain genuinely meritorious types—a belief, perhaps, that the movement would be administered by "mental testers," and that a high I.Q. as determined by the Binet test would be essential to selection. If one holds that many hare-brained freaks score high on the Binet test, and many gifted in one way or another score low, then the matter looks serious. I shall shortly give reasons for not thinking the I.Q. a very good measure to use as one of the measures in the selection of the eugenically fit. But let us now ask if any real danger lurks therein. For someone to show that a few "odd ducks"

score high on a test is not sufficient to prove that the device is poor. To establish this, it is necessary to show that the proportion of such among those scoring high is greater than the proportion in the breeding population at large. In short, it becomes an experimental and statistical matter. Investigations of this sort will surely be made just as soon as eugenics attains such a status that devices for use in selecting parents are actually considered.

How about the geniuses of one sort or another that score low on the Binet test? Here again it is an experimental matter to determine if the type of genius in question constitutes a larger proportion of those scoring low on the test than of those scoring high. If it should be found on thorough investigation that selection of people with high Binet scores tended, let us say, to eliminate musicians (such partial data as are at hand indicate the opposite), then I venture to assert that present test users themselves would practically unanimously consider it an improper device to use for the selection of the eugenically fit. If, as is more likely, it is found that high Binet standing has little effect one way or the other in selecting musicians, it would be considered inadequate upon this issue, and a supplementary measure covering musical ability should be employed, if it is desired to breed musicians. In short, knowledge about the instrument employed, readily though not inexpensively obtained, can sustain or disperse all fears as to cacogenic tendencies of a technique.

Though I know a mathematician of high order in a crippled body should I conclude that we should not eliminate if possible crippled bodies, for fear that we would thereby eliminate mathematicians? I should come to this conclusion only in case it is shown that mathematical capacity of high order is relatively more often found in crippled bodies than in sound bodies. The issues involved are statistical matters

in just the same sense as are those in life-insurance experience tables. Some progress has been made in the building up of tables giving the probability of death at various ages of people having different traits, a sound heart, tubercular ancestry, etc. Similarly we can build up tables giving the probability of certain traits in offspring born of people showing different conditions. Though time of death does follow certain rules as expressed in the life-insurance table, there is a large chance element in it, represented by exposure or lack of exposure to contagious disease and to accidents. Probably no similarly large chance factors operate between the mental and physical status of parents at the prime of life and that of offspring at prime of life. We may accordingly expect a more accurate forecasting of mental and physical equipment of offspring, knowing that of the parents, than is now the case in forecasting time of death from data contained in the physical examination blank filled out at the time an insurance policy is applied for.

When the time does come that fertility of the able is encouraged, and sterility of the incompetent rewarded, what may we expect to be some of the bases of selection of the eugenically fit? We may be sure that there will be many bases, and not one, for we know that different genes or genetic elements mediate different traits. We cannot influence eye-color by selecting the genetic element that has to do with the number and perfection of the digits of the hand. Many physical traits should be attended to, those having to do with the soundness of the sensory, motor, and visceral organs coming first. The independence of genetic elements is such that, generally speaking, selection of a genetic unit because of its known good effect in some regard carries with it no detrimental effect in another regard, nor for that matter, any beneficial effect, either. Though we select so as to improve some physical organ, say the quality

of the lungs, we shall not thereby run a risk in some other respect, say, the condition of the eyes. This statement may need some slight qualification as more knowledge is obtained, because single genetic units do affect more than single organs; still, it surely is not far from the literal truth. We therefore see that improvement of one feature by breeding is altogether to the good, and is not the occasion of any alarm with reference to other features about which little is known. This permits a piecemeal attack of the problem and a continual increase in effectiveness as knowledge grows. The very extensive body of knowledge now extant covering animal breeding serves in the development of desirable physical features in the human species.

Undoubtedly much the same situation will maintain in connection with mental traits. Here the difficulties of ascertaining independent mental organs and still greater difficulties of tying them to genetic structure warrant a less specific approach. Without knowing what genes condition musical ability, we can, nevertheless, improve it in offspring by breeding from musicians. This method can be effective, demonstrably so, though the genetic mechanism is unknown.

Our first need is to determine what are the independent mental traits. We should not think of a single I.Q. or a single intelligence score, but of ability scores, as many in number as we can experimentally justify by showing the existence of an equal number of independent mental traits. High standing in each of these is what society judges to be high standing, and it is a good thing to have, for it means expert ability such as high musical talent, high mathematical talent, high manipulative talent, high literary talent, and so forth. If we breed for these separately we shall in each case confer a good upon society, and perhaps in the distant future some superman can be bred combining all of these

talents to a high degree. It is beyond our present knowledge to even surmise whether this is possible, for the genetic relationships involved are quite beyond our grasp to picture, and we have no experimental evidence bearing directly upon this issue. Whether a single type superman or several types of supermen or even an indefinitely large number of types are the goals of mankind and thus of scientific breeding is a question that we may well let the greater wisdom of the future with its more perfect knowledge of evolution ascertain. In any instance, the first steps are clear and only await the social will to be put into operation.

The plain truth of the matter is that the racial knowledge about inheritance and about how to produce healthy, capable offspring has already outstripped the racial spirit of self-sacrifice, cooperation, and tolerance that is necessary to make the knowledge effective. In mentioning tolerance, I do not so much mean willingness to permit the dissemination of knowledge and views of minority groups, though that is of great importance, as I do willingness to tolerate, even to aid, those differently endowed. The odd child, though something of a genius in one line, is suppressed in the elementary school and made to conform. The university professor of law has, and that not infrequently, belittled and ridiculed the talents of the engineer, and so it goes. Feeling somewhat ill at ease in the presence of the differently endowed seems to be followed by dislike for and antagonism to him and finally by an effort to make him conform. Intolerance is confined to no stratum of society, and wherever found it is a potential stumbling-block to genetic advance.

An unselfish devotion to the nation is called for. Many a married couple today, knowing themselves to be above the average in physical and mental equipment, are childless by preference, because they love their freedom and their pleasures, or because they are ambitious for advancement in

their chosen line of work, which is not that of rearing children, and because they feel no sense of responsibility for the genetic structure of the coming generation of the nation to which they generally are proud to belong. Many men and women today are not fit to be parents, and know it, but they do have children because of incontinence or of sheer indolence in securing knowledge of and in the use of contraceptives, which practice they perhaps defend by hiding behind the cloak of religion; or they have children because their passion to have them leads them to suppress their better judgment. Such passion may pass under the guise of parental love. It is a veritable Judas kiss implanted indelibly upon the brow of the defenseless child.

Though no religion will openly defend the propagation of the feeble-minded or the congenitally diseased or insane, many do actually support these by the sin of silence, and by placing their taboo upon the discussion of the matter. We need a militant religion that will arouse mankind to as great sacrifices for the welfare of the coming generation as they have made for themselves. Of course the selfishness that leads a man to circumscribe his life by the hope of personal salvation has no place in this new religion. Self-sacrifice, planning, preparing, and, above all, using God's greatest gift, one's scientific knowledge, in behalf of those to come, is the way the spirit of Christ can be expressed in the evolving world wherein we live.

In conclusion let me say, if a curriculum, adapted to individual differences, committed to the development of the world's new tool—the scientific method—and dedicated to the nurturing of original talents, seems remote, recall the growth of the last three centuries and be optimistic. If the possibility of racial betterment by the conscious breeding of a nation's citizens seems fantastic, recall that less vital issues than this have been the bases of epochal religions;

and recall also that cacogenic tendencies less potent than the self-imposed sterility of a nation's leaders have undoubtedly led to the decay of nations. Darwin tells us little of the speed of evolution, but of its certainty he leaves no room for doubt.

CHAPTER IX

MENTAL TRAITS OF MEN OF SCIENCE

ONE who approaches research with a desire to follow wherever the spirit moves, with a willingness to use a new mechanism and master a new technique, even if need be to discard an old problem for a broader underlying one, will find that a study of what research has meant in the world and some acquaintance with its process and with the characters of the men who have made it mean what it does, gives not only vision but also specific suggestion. In such a study we must distinguish between prime movers in scientific research, and hangers-on. These latter are not only the great popularizers of science; they may be great philosophers as well, who incorporate new knowledge along with old into a comprehensive system. A very energetic group of such philosophers is now engaged in incorporating the concepts of Einstein into the earlier world of Newton.

Not being great investigators themselves, these men may fail to see something in the heart of the process, and they may overemphasize and oversimplify some aspect, to the obfuscation of some other more recondite essential. Two of the greatest men of this class of all time were Herbert Spencer and Francis Bacon. The benefit to society of their endeavors may well be great, but this benefit lay in arousing confidence and a will to do scientific work and to trust its findings, not in technique for its actual accomplishment.

What discoveries do we owe to Bacon, the modern sponsor for the inductive method? Not only did he give the world no great discovery, but further, his writings did not reveal

a sympathy with the great work of Copernicus, or familiarity with the great discoveries of Kepler. We must consider his method in the light of his own slight accomplishment with it.

Though Herbert Spencer was better informed about the scientific accomplishment of his time than was Bacon about that of his time, and though Spencer did draw upon a wider knowledge both of the limitations and of the accomplishments of science, still he was withal a philosopher, a man who told what science could do for the world rather than just how it was to do it. Spencer has given us no noteworthy experimental contribution.

Shall we look to Francis Bacon for the key to scientific genius such as welled within Galileo, Pasteur, and Newton? Listen to some of his words. He criticizes Aristotle in the following manner: [1]

> . . . having first determined the question according to his will, he then resorts to experience, and bending her into conformity with his placets leads her about like a captive . . .

The correctness of this judgment is open to much doubt, for Aristotle states: [2]

> Let us first understand the facts, and then we may seek for their causes.

And again, in speaking of the parthenogenesis of bees, he says: [3]

> There are not facts enough involved to warrant a conclusion and more dependence must be placed on facts than on reasoning, which must agree with the facts.

[1] Spedding, James, and Ellis, R. L., eds., *Works of Francis Bacon*, 1889, Vol. IV, 1st book of Aphorisms. Aph. 63, p. 65.

[2] Lewes, G. H., *Aristotle, a Chapter from the History of Science*, 1864, p. 110.

[3] *Ibid.*, p. 110.

And still again: [1]

> We must not accept a general principle from logic only, but must prove its application to each fact, for it is in facts that we must seek general principles, and these must always accord with the facts.

Aristotle was quite clearly aware of the primal and compelling nature of facts, but he made few observations himself, and his followers of the Middle Ages forgot this part of his teaching, so that in Bacon's time it was customary to "expect general principles from logic only," and fact and the experiment as the basis of knowledge was to be found in no circulating philosophy. Preceding Bacon's time many true scientists did return to these first principles. About 100 years earlier Leonardo da Vinci had said: [2]

> The interpreter of the artifices of Nature is Experience, who is never deceived. We must begin from experiment and try to discover the reason.

Though not the originator of the idea, Bacon emphasized the basic nature of the fact and the power lying at the individual's command in the control of experiment as none other had done. Bacon would have one observe, make inferences, and on the basis of these plan experiments, observe further, make further inferences, guarding every step of the process, and proceed from minor generalizations to those of greater and greater generality, building the structure of scientific knowledge as it were brick by brick, by the inductive method.

History has no place in the process. He says: [3]

> . . . we should at once and with one blow set aside all sciences and all authors; and that too without calling in any of the ancients to our aid and support, but relying on our own strength.

[1] *Ibid.*, p. 112.
[2] Abbott, Edwin A., *Francis Bacon*, 1885, p. 335.
[3] Spedding and Ellis, *op. cit.*, "Novum Organum," Vol. IV, 1st book of Aphorisms. Aph. 122, p. 108.

Again,[1]

> It is idle to expect any great advancement in science from the superinducing and engrafting of new things upon old. We must begin anew from the very foundations unless we would revolve forever in a circle . . .

There is no great subtlety in Bacon's method. According to his statement he has just happened to discover a rule of thinking whereby all knowledge may be attained:[2]

> . . . my way of discovering sciences goes far to level men's wits and leaves but little to individual excellence; because it performs everything by the surest rules and demonstrations.

Of course some mills operate more smoothly than others, and Bacon considered his own mind quite definitely disposed to know the truth. As I give his own words describing his talents, do not let his complacency turn your attention from the fine list of qualities which he mentions as conducive to knowledge. He wrote:[3]

> I found my own nature a special adaptation for the contemplation of truth. I had a mind at once versatile enough for that most important object, I mean the recognition of similitudes . . . I possess a passion for research, a power of suspending judgment with patience, of meditating with pleasure, of assenting with caution, of correcting false impressions with readiness, and of arranging my thoughts with scrupulous pains. I had no hankering after novelty, no blind admiration for antiquity. . . . For these reasons I considered that my nature and disposition had, as it were, a kind of kinship and connection with truth.

A man with the wit of Bacon cannot write on a subject without illuminating it. Certainly many of his observations are true and fine, but his own attempt to demonstrate his method by analysis of heat was rather futile. He missed

[1] *Ibid.*, Aph. 31, p. 52.
[2] *Ibid.*, Aph. 122, p. 109.
[3] Abbott, *op. cit.*, p. 27.

a connection somewhere—I believe it was in his failure to build on the past, in his belief that research is a mechanical process, and in his lack of sufficient definition of the scientific issue. Bacon then lies a little outside the field of most promising investigation. Let us turn to men who did greatly extend our knowledge of the world, to find, if we can, the nature of the actual steps employed.

Of these none is more satisfying to study than Charles Darwin. Men like Pasteur, Newton, Willard Gibbs are so brilliant, so in tune with nature's mysteries, that they leap to conclusions across gaps that we cannot follow, but Darwin, as great a benefactor to the world, as true a scientist as history tells us of, does not seem to sense nature's mysteries, and then gather confirmatory information, but by the very rigor of his research he compels her to reveal them. Darwin walks in no seven-league boots. Each step is foreshadowed by the one before. Upon closer examination we may find that this distinction between a Pasteur and a Darwin is not a real one, but in the process of comparison the life of Darwin provides a better key to that of Pasteur than does that of Pasteur to Darwin.

Darwin was not a brilliant youth, and seemed to delight in boyish pastimes in the open air. In this he did not differ from half the other boys of his or any other generation. His passion for collecting was revealed very early in life, and it is somewhat exceptional. It found expression in gathering beetles, shells, coins, minerals, and other things. The collecting tendency, or, as others would say, instinct, though common, is very unequally developed in different individuals, and Darwin may well have had it to so pronounced a degree that not more than five in a hundred would equal him. This interest must, of course, be taken into account in the attempt to understand his life, but of itself it is insufficient to account for his success, else we should

have had thousands of Darwins whereas we have had but one. Darwin loved the out-of-doors, was a keen observer and collector, and readily turned toward science.

Speaking of his scientific endeavors, he said: [1]

> I worked on true Baconian principles, and without any theory collected facts on a wholesale scale.

Are we then to cite Darwin as an exponent of the Baconian method? We are not, and because of what seems to be just a chance incident in his life. Darwin found in the middle of England what he thought was a tropical shell, and was greatly moved when Sedgwick was not pleased. What was the tropical shell doing in England? Well, whatever it was doing, it aroused no resentment in Darwin, but it had no place in Sedgwick's scheme of things. Darwin wrote as follows in his old age, though that need not lead us to distrust the reality of the youthful experience: [2]

> . . . I was then truly astonished at Sedgwick not being delighted at so wonderful a fact as a tropical shell being found near the surface in the middle of England. Nothing before had ever made me thoroughly realize, though I had read various scientific books, that science consists in grouping facts so that general laws or conclusions may be drawn from them.

Here, then, is where Darwin broke with Bacon. This incident in Darwin's life marks an epoch. Was it essential to his scientific development, or lacking it would some other incident have arisen leading to the same deepening of his view? Certain it is that such an incident in the lives of most men would not disturb the even tenor of their ways. Whether necessary or not, it did happen most opportunely, for it was essential that Darwin work to a purpose, and discard pure induction as the key to scientific work.

[1] Darwin, Francis, *Life and Letters of Charles Darwin*, 1891, Vol. I, p. 68.
[2] *Ibid.*, Vol. I, p. 48.

Pure induction would lead a man upon the seashore to observe indiscriminately skies, water, sea-life, land-life, human life, color, temperature, odors, physical phenomena, intellectual phenomena, and perchance moral phenomena. In short, all of life would be his field, and from its richness he is to induce principles. The task is impossible, and Bacon's comments give only such comfort as a man drowning in Lake Michigan could get by thinking of his plight if in the ocean. Bacon wrote: [1]

> . . . let no man be alarmed at the multitude of particulars . . . For the particular phenomena of art and nature are but a handful to the inventions of the wit.

We must be concerned with the multitude of particulars, and we must seek a method that is discriminating, that is a guide in this wilderness of facts.

When Darwin ceased to be interested in collections of all sorts he became a more effective collector and interpreter of certain things. This limiting of field of effort is necessary, but it also has its dangers. If we take history as a guide and limit our effort to fields described in the past, new fields are not investigated. Also if we take *a priori* hypotheses as guides, there is no progress if our hypotheses are incorrect. Darwin's position seems to be that of the perfect scientist, having in it little lost effort and effective in finding and establishing truth.

The idea in connection with which Darwin is most frequently mentioned was well phrased by Herbert Spencer, who coined the expression "the survival of the fittest," and it was suggested to Darwin through an earlier thinker.[2] Writing of his work in 1838 he said:

> . . . fifteen months after I had begun my systematic enquiry, I happened to read for amusement Malthus on Population, and

[1] Spedding and Ellis, *op. cit.*, Aph. 121, p. 101.
[2] Darwin, Francis, *op. cit.*, Vol. I, p. 68.

> 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 favourable variations would tend to be preserved, and unfavourable ones to be destroyed. The result of this would be the formation of a new species. Here then I had at last got a theory by which to work; but I was so anxious to avoid prejudice that I determined not for some time to write even the briefest sketch of it. In June, 1842, I first allowed myself the satisfaction of writing a very brief abstract of my theory.

He spent four years mulling it over before he would permit himself to become sufficiently attached to it to write it down!

Clearly realizing that an hypothesis might become a prison, Darwin immediately provided a way of escape. In his biography he wrote: [1]

> I had, also, during many years followed a golden rule, namely, that whenever a published fact, a new observation or thought came across me, which was opposed to my general results, to make a memorandum of it without fail and at once . . .

Writing in his old age of his early life and, we must believe, of a period following his experience with Sedgwick, he said: [2]

> From my early youth I have had the strongest desire . . . to group all facts under some general laws. These causes combined have given me the patience to reflect or ponder for any number of years over any unexplained problem . . . I have steadily endeavored to keep my mind free so as to give up any hypothesis, however much beloved (and I cannot resist forming one on every subject), as soon as the facts are shown to be opposed to it . . . I cannot remember a single first-formed hypothesis which had not after a time to be given up or greatly modified. This has naturally led me to distrust greatly deductive reasoning in the mixed sciences.

Darwin's mind was master. Though he gave himself loose rein to form hypotheses, once formed they did not control, but became subject to rigid tests of adequacy.

[1] Darwin, Francis, *op. cit.*, p. 71.

[2] *Ibid.*, p. 83.

The reading of Malthus was fortunate in his life, but of course we cannot say indispensable, for his mind was ripe for the idea of variation, and survival of the fittest. If Malthus had not provided the cue to a valuable hypothesis, something else most surely would have. Darwin himself clearly recognized that it is no more in the hypothesis than it is in its modification, elaboration, and verification or rejection, that the scientist makes his contribution. Scientific method as exemplified by Darwin seems to possess the following attributes:

(a) Delimitation of field upon the basis of antecedent endeavor, whether of oneself or of others.
(b) A wide acquaintance with as many facts in this field as possible.
(c) The tentative construction of a hypothesis by the inductive method to account for these facts, and
(d) The elaboration of the hypothesis and a collecting of additional facts bearing upon it, leading generally again by the inductive method to a modification or rejection of the hypothesis. These additional facts are collected in a systematic manner by observation of selected features of nature or by carefully controlled experimentation.

This last step (d) is repeated as many times as may be necessary to reach the degree of agreement between facts and hypothesis called for by the investigator himself.

Darwin could not say of this process as Bacon had said of his, that it tended to make all men equal. Too much of breadth of view, too much of originality, too much of rigor, persistence, and utter honesty with oneself are required to lead one to think that this process can be standardized and will lead to a "levelling of men's wits."

Darwin has been most kind to the world in his biography in permitting us to see him unadorned. The candor with which he describes himself is on a par with that with which he notes difficulties arising in connection with his own hy-

potheses. Seldom does a man appraise himself so dispassionately that one feels no necessity of modifying his verdict. Darwin was one of these rare self-appraisers. I wish to quote him upon those traits of character that seem to be an intimate part of his scientific life. He said: [1]

> The passion for collecting which leads a man to be a systematic naturalist . . . was very strong in me, and was clearly innate . . .

Of his school work in mathematics, he said: [2]

> [It] was repugnant to me, chiefly from my not being able to see any meaning in the early steps in algebra. This impatience was very foolish, and in after years I have deeply regretted that I did not proceed far enough at least to understand something of the great leading principles of mathematics, for men thus endowed seem to have an extra sense. But I do not believe that I should ever have succeeded beyond a very low grade.

Darwin had the good fortune to get an assignment as naturalist on board the *Beagle*, upon its extended trip in Southern waters. This was without pay, but he also had the good fortune to have independent means. His greatest work of collection and observation was done on this trip. Of it he says: [3]

> The voyage on the *Beagle* has been by far the most important event in my life . . .

Of his native talents he writes: [4]

> I have no great quickness of apprehension or wit which is so remarkable in some clever men, for instance, Huxley . . . My power to follow a long and purely abstract train of thought is very limited; and therefore I could never have succeeded with metaphysics or mathematics. My memory is extensive, yet hazy. It suffices to make me cautious by vaguely telling me that I

[1] Darwin, Francis, *op. cit.*, p. 26.
[2] *Ibid.*, p. 40.
[3] *Ibid.*, p. 51.
[4] *Ibid.*, p. 82.

have observed or read something opposed to the conclusion which I am drawing, or on the other hand, in favour of it; and after a time I can generally recollect where to search for my authority. So poor in one sense is my memory, that I have never been able to remember for more than a few days a single date or a line of poetry.

Again he said: [1]

. . . I think that I am superior to the common run of men in noticing things which easily escape attention and in observing them carefully. My industry has been nearly as great as it could have been in the observation and collection of facts . . . My love of natural science has been steady and ardent.

He writes of his success in a very modest and probably in a truer tone than did Bacon write of his: [2]

Therefore my success as a man of science, whatever this may have amounted to, has been determined, as far as I can judge, by complex and diversified mental qualities and conditions. Of these, the most important have been—the love of science—unbounded patience and long reflecting over any subject—industry in observing and collecting facts—and a fair share of invention as well as of common sense.

Let us take Darwin at his own valuation, which gives us a good, wholesome statement of his talents and of his weaknesses.

His good friend Huxley referred to him as [3]

. . . something bigger than ordinary humanity—an unequalled simplicity and directness of purpose—a sublime unselfishness.

And George J. Romanes writes: [4]

No one in this generation is able to imitate Darwin, either as an observer or a generalizer.

[1] *Ibid.*, p. 83.
[2] *Ibid.*, pp. 85–86.
[3] Darwin, Francis, *More Letters of Charles Darwin*, p. 71, footnote 3.
[4] Romanes, George J., *Darwin and After Darwin*, 1896, p. 8.

Darwin would not countenance such claims, and we need not believe him unequaled in any talents, sublime in any virtues, or bigger than humanity. He was one with us, and what he did is within our ken. At least let us believe so, so that we may endeavor to follow in his footsteps.

Of the superhuman virtues that Huxley cites, that of unselfishness comes as near to being justified as any. Think if you will of a young man of today, of independent means, giving up his country club and an easy pleasant social life and burying himself upon a tedious ocean trip in a small boat, wherein every wind that blew, or perhaps every meal that the cook brought forth, caused sickness, because he had an idea and wished to find if it were true. Let this same young man feel before the start that he may be chasing a will-of-the-wisp, but let him carry on withal. Finally, when the trip is over, the data in hand, and the sick body attempting to recuperate on *terra firma*, the man, now nearly thirty years of age, suspends his judgment for another twenty years, and devotes his time to penetrating scrutiny and to verification before he reports in print the outcome of his labors. Here is an unselfish devotion which of itself explains why every age and every land has not a Darwin.

A particular combination of talents, of virtues, and of circumstances made the man. Many of these circumstances were favorable, but one at least, his health, quite otherwise. Some of the world's great men of science have risen through circumstances of life that have seemed almost uniformly unfavorable. Roger Bacon and Galileo Galilei were such. Religious leaders have commonly thrived under adversity. So we must not attach too much importance to favorableness of environment. If there is a spirit of revolt abroad, a feeling that the arcana of life are being withheld by authority, there arise, singly and in small groups, those who dare greatly. The resulting stimulus to research may be even

greater than is the case wherein full social approval accompanies an undertaking.

In the time line that is our history is the record of great courage, great ability, and great accomplishment. I wish to name just a few who have carried the torch of science down the ages. Other names could be mentioned who have had a greater effect on human life, great rulers of the bodies or of the hearts of men—Alexander called the Great, and Jesus called the Christ—but few, if any, who have kept more pure and undefiled the channels whereby men acquire new knowledge of the world they live in.

Pythagoras, Hippocrates, Euclid, Archimedes, Apollonius, Galen. Even among these in the dawn of science are to be found lines of demarcation. Pythagoras, Euclid, and Apollonius were masters in pure reasoning, in mathematics; Hippocrates and Galen in living matter, in medicine; and Archimedes in physics, tied then as now to mathematics. Galen, who flourished in the second century A.D., was the latest of these. He was an experimentalist, and a keen observer of human phenomena, but at about this time in the world's history something seemed to happen. For the next thousand years thinkers turned their thoughts inward to philosophy and religion.

Logically, pure mathematics requires no physical phenomena to operate upon, but actually it has been inspired to new advances by every physical discovery, and as these were few in the Dark Ages, mathematics also languished, though it did not attain quite the depravity of the physical and biological sciences. When the light began to break again, it was, of course, feeble. Seen in this dimness is Roger Bacon, who flourished in the thirteenth century, a great man who dared to look about him for truth, who dared to observe the life immediately before his eyes and within his touch, and whose simple honest utterances caused him great

physical distress. The feeble flame nursed by Roger Bacon was held aloft by Leonardo da Vinci, Nicolaus Copernicus, and Galileo Galilei, and by an ever-increasing number. Of this increasing number there are a few who have fanned the flame to so white a heat that we may believe it will never die again. First in this list is Isaac Newton, who tied physics so close to mathematics that none has tried to extricate it since, though Michael Faraday did show with a brilliance that probably can never be equaled again what the mind of man, little tutored in mathematics, can grasp of nature's hidden physical processes. Of Faraday's great contemporaries, I will mention only Charles Lyell in geology, and Charles Darwin; a generation later are Louis Pasteur and Willard Gibbs. There may be one or two men living and in their prime today in a class with these that have been mentioned. It would be no kindness to any living man to have the world say to him while his labors are still in process, "We expect as much from you as we got from Newton or Darwin."

I have mentioned several mathematicians, physicists, and astronomers, several medical men and biologists, one geologist, and one chemist. There is no clear reason for stopping the list here. Though these men whom I have named are in the front rank of men of science for all time, they do not stand alone, and others of their level or very nearly of it could be mentioned. None, however, of equal rank will be found in the field of social science, none who have given new foundations to human thought upon demonstrable and verifiable bases. The Ptolemaic system of social science has yet to be shaken by some economic Copernicus.

So much of myth, so little of fact, so much relatively of outcome and so little of method, have come down to us from the early Greeks that it is difficult to investigate the issues connected with scientific outlook and procedure. We

do, however, in the case of both Greek scientists and philosophers, find a spontaneity and an independence not characteristic of later workers who have a genuinely valuable history upon which to lean. Even Galen, who flourished in the second century A.D., drew heavily from Hippocrates. He did, however, much that was original, and he has told us about it in his writings.

He was the son of a well-to-do architect, who saw to it that his son had a liberal education, and he lived in Pergamos, which had an excellent library. He started the study of medicine when seventeen, and continued it during his travels, which included such centers of learning as Smyrna and Alexandria. The controversial nature of medicine at his time probably meant much in his development. We may illustrate this by a modern instance. Mendel, who developed laws of heredity, had nothing to combat, and his work was lost for twenty-five years, and he himself dropped it and turned his energies to administering a monastery. When Lamarck and Weismann and others take opposing sides on the question of heredity, Mendel's work becomes vital, is found, and takes its place as a masterpiece. Another instance is illustrated by the life of Da Vinci. His very original work in mechanics aroused little comment in his day. He himself devoted his time to painting and to other things, and his physics was almost lost to mankind for three hundred years, the great scientists of the fifteenth and sixteenth centuries being apparently unfamiliar with it and not profiting by it.

To be ahead of one's time means more than anything else that the points one makes are not appreciated, and thus are not doubted by others. New knowledge is the spark that springs to life between the flint and the stone of claim and counter-claim. Most of the medical practitioners of Galen's day diagnosed and treated on the basis of logical

deduction, operating upon specific evidences of disease. They might be called part-treaters—separatists—because of the specificity of their point of view. Galen, taking his cue from Hippocrates, thought of the whole organism as a unit, and based his arguments upon this concept. He might be called a unitarian. Both the separatists, and Galen the unitarian, were great dialecticians.

In the wordy conflict that was carried on between him and his opponents, Galen appealed to a more ultimate authority than deduction—namely, experience and experiment —controlled experiment. He used a genuine experimental method as a shield and buckler in his fight. However, it is doubtful if we should credit him with realizing the true dignity of the method as a guide in the building up of a philosophy. He did not quite use the method as did Galileo, or in fact Archimedes, as a great beacon light to knowledge, as a thing more ultimate than doctrine. Galen was no such anatomist as Vesalius, the next great light in medicine, who flourished fourteen hundred years later. With his philosophy of unity of the human organism, he could not be, for the anatomist turns his attention successively to very limited portions of the whole. Galen's point of view was a great advance in his day, but it became a heavy drag in the centuries that followed. The reason for this is not far to seek. Galen was a ready writer, and an expert in argument. He did not change the method of investigation from that of mere logic to that of experiment, and observation. His arguments, more than his facts, upheld the banner of Hippocrates, and they were forceful arguments, as are generally those that subsume much of life under a single principle. Further, this practice in thought was current for at least fifteen centuries, so that Galen quite naturally belonged to the followers of Aristotle and the church.

Let me give one sample of Galen's success in making

generalizations, such generalizations as for centuries preceding the Renaissance filled the churches and the halls of learning, and darkened the windows to the life about. He adopted the principle that there is a specific attraction between the tissues and the things that give them life. He proceeds then to show that this principle explains why the lodestone attracts iron, why the stomach receives food, why the kidneys receive urine, and why the uterus receives semen. Now is not this fine—one principle accounts for all of these things, and for many more, and having once noted this principle one need look no farther, for his knowledge is complete.

This principle just cited was and still is descriptive of something that takes place between every body tissue and the food that nourishes it. It is a brilliant generalization characteristic of a great thinker. That Galen extended it beyond its useful field does not detract from his keenness in appreciating its importance in the living organism. Galen's experiments were very numerous and remarkably sound in the sense that conditions were controlled, and his observation of what happened most excellent. He almost discovered the circulation of the blood. I quote from Brock's translation of his treatise on the natural faculties.[1]

> Now . . . the following is sufficient proof that something is taken over from the veins . . . to the arteries. If you will kill an animal by cutting through a number of his large arteries, you will find the veins becoming empty along with the arteries; now, this could never occur if there were not anastomoses between them.

Galen's hypotheses were superior, his experimentation thorough, and his presentation precise, argumentative, and logical. Had his immediate followers accepted his hypotheses as hypotheses, and had they continued and trusted

[1] Brock, A. J., *Galen on the Natural Faculties*, 1916, p. 321.

experimentation instead of being content with his logic, the gap between Galen and Harvey might have been one generation instead of fifty.

Some of Galen's personal traits may be inferred from his writings. He had a high sense of personal responsibility. He exercised great self-control, and forswore sensual pleasures. He had a strong sense of what might be called righteous indignation, and he was a man of great energy and industry. From what he does not write, we must conclude that he was unconcerned with the suffering caused by vivisection. It is in fact quite impossible to conceive how he could have been concerned, and observe phenomena that he did observe, and have the thoughts while doing so that he has recorded. As the geologist must trace in his mind the fault line, and forget the cloud upon the mountain, so vivisectionists before the days of anesthetics had to attend to physiological processes and not to psychological.

After Galen, no really great figure in the history of science occurs until we come to Roger Bacon, who flourished in the thirteenth century—seven hundred years ago—seven hundred years, the span of life of modern science. Because of the fragmentary and at times contradictory evidence, one today can scarcely fully understand Bacon. The authority and hold of the church upon all channels of learning and upon rules of thought was such that only a peculiar combination of devotion to the church, of belief in the evidence of the senses, and of skill in relating the two, could gain a hearing. Today a scientist can ignore any organization, if he so chooses, and continue his way not seriously hampered. In the thirteenth century intellectual development simply did not take place in the sense that it affected history, except under the aegis of the church. Unless this is borne in mind, we can scarcely understand an Abelard, a Roger Bacon, a Galileo, or a Voltaire.

Voltaire's protestations of religious faith do indeed seem insincere and those of a man who played upon the stupidity of his spiritual superiors. How about Roger Bacon? Could he believe, as he affirmed, that the Scriptures contain all true wisdom, and at the same time argue that experience is the true way to knowledge? In this third work he maintains that experimental science holds a pre-eminent position because conclusions are verified by direct experiment, and accordingly truths and secrets of nature are discovered such as no other method would permit of. Whether sincere in his dual affirmation or not, is it not true that any other position on his part would have made him either just another school man of the Middle Ages, or an unknown short-lived scientist, neither of which outcomes would have stirred the slumbering world as did the life he led?

If he knew that supreme trust both in the divine authority of the church and in experiment were incompatible, but professed belief in both, we can honor him for the courage he displayed in tempting a fire that could extinguish him in a moment; and if he truly thought them compatible, we can thank him for his dullness in not seeing the danger that he ran. Though Roger Bacon defended the divine authority of the church, he also investigated the magnifying lens, gunpowder, and particularly the possibility of defining phenomena in exact mathematical terms. His name means nothing to us because of this first activity, but it is emblazoned in the halls of science because of the second. Some of his personal traits of character can be surmised. He was of good family, precocious, a scholar of high order, of tremendous energy—he practically touched as many sides of life as did Aristotle—an excellent arguer, a man of spirit. The record of his later life is somewhat obscure, but the evidence seems to indicate that after the condemnation of his works he led a restricted life (though the reported imprisonment of 14

years is open to question) and that he died, an embittered broken man. With his own last days in mind, think of the truth, the terrible personal truth of his statement that the four great causes of error are (a) authority, (b) custom, (c) the opinion of the unskilled many, and (d) concealment of real ignorance with the pretense of knowledge. As Bacon proclaimed and could bear personal witness to, the last of these was the most vicious. It is fitting that the awakening of science should be ushered in by so great a martyr. Others followed in his footsteps.

We can mention three great astronomers: Copernicus, perhaps saved from the fate of Roger Bacon by death just as the first printed copy of his revolutionary work appeared; Kepler, harried by the high priests of the Reformation; and finally, Galileo, whose life was a hard struggle against poverty, a bitter fight with authority, and an irrepressible urge to know nature at first hand.

Up to the time of Galileo's invention, or reinvention and development, of the telescope he was an original thinker who greatly furthered the development of physical science by his experiments and demonstrations, but whose influence did not, at least in the minds of the people of his day, greatly disturb the cosmography of Aristotle as sponsored by the church. He believed in the Copernican theory, but was willing to hold this view as an hypothesis against which there was no interdiction. He wrote to Kepler in 1597: [1]

> . . . many years ago I became a convert to the opinions of Copernicus, and by his theory have succeeded in explaining many phenomena which on the contrary hypothesis are altogether inexplicable. I have arranged many arguments and confutations of the opposite opinions, which, however, I have not yet dared to publish, fearing the fate of our master, Copernicus, who, although he has earned immortal fame among a few, yet by an infinite number (for so only can the number of fools be measured)

[1] Fahie, J. J., *Galileo, His Life and Work*, 1903, p. 40.

> is hissed and derided. If there were many such as you I would venture to publish my speculations, but since that is not so I shall take time to consider of it.

Here is an illustration of the necessity of an understanding audience. Do we not also see in the last sentence an awareness upon Galileo's part that he must soon enter the lists? His entrance came a decade later with his perfection of the telescope. He writes to a friend in 1610: [1]

> I am . . . staying in Venice for the purpose of getting printed some observations which I have made on the celestial bodies by means of my spyglass . . . and which infinitely amaze me. Therefore do I give thanks to God, who has been pleased to make me the first observer of marvellous things unrevealed to bygone ages.

In particular Galileo saw the satellites of Jupiter making their orderly rounds, and he knew the full significance of this as the final incident in the proof of the Copernican theory, and as the evidence that would confound the followers of Aristotle and Ptolemy. Who has communed with God as did Galileo with his telescope pointed toward Jupiter, and to himself has later denied him? Certainly not anyone with Galileo's honest mind. The die was cast. Galileo was the knight errant of the worlds that rotate about the sun, but in the years to come it was a battle of wit to preserve his position, and at the same time his personal freedom from imprisonment and from the rack.

The trials he had to face in his struggle for a livelihood, in his later blindness, and in the opposition of the church, were enough to tax the courage of any man, but there was another trial which must have been greater than all of these combined,—the incompleteness of his knowledge upon certain crucial matters.

Galileo discovered and accurately described the four chief

[1] *Ibid.*, p. 85.

satellites of Jupiter. The rebuttal of the Aristotelians was ludicrous and vicious. They claimed the phenomena were visionary, due to aberrations of one sort or another and caused by the instrument of observation. Galileo's defense to these charges was adequate, except in one instance. He had observed Saturn and found three stars instead of one, having interpreted the ring about Saturn as constituting two added stars, one on either side. When these two, which were in fact but part of Saturn's ring, vanished entirely from the heavens, as the ring of Saturn does vanish when viewed through ordinary telescopes, twice every twenty-eight years because it is then seen edge-wise, Galileo was non-plussed. Nature was playing tricks upon him. She was giving aid and comfort to the enemy by her fickleness. Of course Galileo did not believe this of her, but how could he, who had asserted that Saturn was a triple planet, defend himself from his critics?

This was serious enough, but of course more serious was the fact that Galileo could not understand the situation. Seldom is scientific knowledge so necessary to a man's well-being. The following letter to a friend, written in 1612,[1] is the cry of distress of a strong man who has no sword to wield as his enemies draw near:

> Looking at Saturn within these last few days, I found it solitary without its accustomed stars, and, in short, perfectly round and defined, like Jupiter, and such it still remains. Now what can be said of so strange a metamorphosis? Are, perhaps, the two smaller stars consumed like spots on the sun? Have they suddenly vanished and fled? Or has Saturn devoured his own children? Or was the appearance, indeed, fraud and illusion, with which the glasses have so long mocked me and many others who have observed with me? Now, perhaps, the time is come to revive the withering hopes of those who, guided by more profound contemplation, have fathomed all the fallacies of the new

[1] *Ibid.*, p. 110.

> observations, and recognized their impossibility. I cannot resolve what to say in a change so strange, so new, so unexpected. The shortness of time, the unexampled occurrence, the weakness of my intellect, the terror of being mistaken, have greatly confounded me.

Galileo took hope, guided apparently by the disappearance of Jupiter's satellites behind Jupiter himself, and he prophesied that the two smaller stars would reappear. True, they did, but Galileo's telescopes were not powerful enough for him to resolve the difficulty, though he did record quite accurately the phases of Saturn as revealed by his instrument. Saturn's behavior must have been a veritable sword of Damocles in the days that followed. Fortunately, his enemies were not wise enough to cut the thread, and Jupiter and other matters remained the important subjects of discussion. Galileo never deceived himself in this matter. He did not make the error Roger Bacon found upon every hand, and "conceal his ignorance with the pretense of knowledge." Twenty-four years later, blind and feeble, writing about Saturn to a friend, he says: [1]

> It will be for the future and for others to make observations, registering the times of mutation, so as to determine accurately their periods—that is, if there will be any persons curious enough to do what I, from the same motive (not knowing how to do better), have done for so long a time.

This is a sorrowful lament but withal that of a man who stands by his guns. He cannot carry on, his mind has grappled with a problem thus far beyond him, but he trusts it still, and he accurately points the path to knowledge. It is the path of science and none other.

This problem was easily resolved by Christian Huygens half a generation after Galileo's death, using a much more powerful telescope.

[1] *Ibid.*, p. 114.

In the conflict with the church three stages may be mentioned: the first that in which Galileo had sufficient support from those high in authority to enable him to teach and write unmolested, except for the bitter denunciation of his doctrines by sundry Aristotelians. This period is practically coterminous with his professorship at Padua, under the aegis of the Free State of Venice. The second stage followed this both in time and place. It turned out to be nothing short of a calamity for Galileo to forego the protection of Venice and subject himself to the intolerance of Rome, as he did when he left Padua for Florence. He pitted his wit against that of the censor and won, but it was a costly victory. His work was approved and printed by papal authority, but the resentment in Jesuitical and other circles was so intense, organized, and powerful that ultimately he was subjected to the Inquisition, vehemently suspected of heresy, forced to recant, and was pledged to silence. The third stage is that of his advanced years, an aging body, a mind alert and resourceful, but cramped by the interdiction that he never discuss or publish matters relating to the motion of the heavenly bodies. In this period he gave birth to his treatises upon dynamics, cohesion, and fraction of bodies. These are the great foundations upon which Newton built. They give evidence of an observing, thinking, analytical mind at work not surpassed by his astronomical contributions.

Galileo was inquisitive in the finest sense, always experimenting and always thinking, seeing the common things of life, but seeing immeasurably more in them than his fellowman. He realized as perhaps none before him the nature of an hypothesis. It was not a belief, a doctrine, a dogma, or a fantasy, but a formulation of a law of nature to be investigated and held in case it better (not perfectly) explained phenomena than alternative or prevailing opinion. By observing so closely as to be aware that his hypothesis did not

perfectly explain phenomena, Galileo left the door open to further investigation, and thus from the outset his science had a different effect upon men's minds than that of Aristotle. Aristotle's erroneous law of falling bodies shackled thought for centuries. Galileo's erroneous explanation of the tides bound no one.

I believe the reason is more in the matter of mental attitude toward the issues in question than due to a difference in times. Galileo was absolutely honest with himself. He never glossed over a difficulty or found it convenient to forget certain inexplicable aspects of a situation. It is true that under pressure he did accommodate his expressed views to those in authority, but we should not condemn him for this. Far better that a living and acting Galileo should express things that he positively believed as though they were merely ingenious bits of speculation, than that they should not be presented at all, which was the alternative. If it were not for these free thinkers who tricked the priesthood, the cardinals, and the Popes, and thereby gained an audience for their doctrines, we might today still be living in the darkness that Roger Bacon found. Great, free, scientific thinkers sprang up upon many hands, but probably none so close to the temporal seat of spiritual power as Galileo. One need not dwell upon the suffering this caused him, but we can regret that he could not know in his days of trial the esteem, reverence, and gratitude of later generations.

Like Galen and Roger Bacon, Galileo was master of an incisive mode of presentation, and redoubtable in debate. He was a man of great energy, having a mind so active that it many times interfered with his sleep. He showed talent in music, drawing, mechanical construction, and manipulation. He had an hypothesis for everything, and a test for every hypothesis. He could reserve judgment, if need be, for years, as in the case of the problem of Saturn. He had a

remarkable ingenuity in devising experimental tests of an issue, a rather overdeveloped sense of responsibility to importunate relatives, an abiding love for his serious students and friends, and, finally, a keen discernment of sham and a facile pen with which to tell of it.

The year Galileo died, Isaac Newton was born. During the life spans of these two men were many others, each of whom did his bit in the remaking of a world, in providing a new focus—experience—about which to revolve: John Napier, inventor of logarithms; Francis Bacon; William Harvey, discoverer of the circulation of the blood; Tycho Brahe, careful astronomical observer; Johann Kepler, interpreter of these same observations; Rene Descartes, mathematician and student of dynamics; Robert Boyle, chemist; Pierre de Fermat, early worker in the calculus of probabilities; Brook Taylor; Colin Maclaurin; Abraham de Moivre; Roger Cotes, and the Bernoulli brothers, all mathematicians of high order. Toward the end of this period we find our own Benjamin Franklin, forming a sort of link between the students of mathematics, dynamics, and celestial motion, and the prime movers in the new sciences of chemistry, electricity, geology, and biology.

Of all of these none took such giant strides as Isaac Newton. He was a precocious youth, and well-educated, and in a span of ten years provided the foundations of modern physics and astronomy, and of that most powerful analytical tool of all science, differential and integral calculus. His contribution in any one of these fields is epochal. He laid the foundations of optics, of spectrum analysis, and discovered the law of gravity. Almost as mere incidents in the orderly march of his mind in the interpretation of the quantitative facts of physics, with many of which he was familiar, he developed the binomial theorem and the method of fluxions, that is, of the calculus. To this day his contribution is a closed book

to most of us, but to the minority engaged in mathematical and experimental science, it is the open sesame to knowledge. The great astronomer Halley is quoted by Voltaire as saying of Newton, "It will never be permitted to any mortal to approach nearer divinity." [1]

We can never understand the recesses of his genius, but we can note many things wherein he differs from the ordinary run of mortal men. Let us sit with him under the apple tree and watch an apple fall. What thought arises? "It might have landed on my head." "I wonder if it is wormy." These would not be unusual thoughts, but what came to Newton's mind? As a matter of speculation, we will say that it was about like this: He thought of Galileo's experiment with falling bodies, which resulted in the idea of uniformly accelerated motion. Then he supposed that there was a hole right through the center of the earth, and an apple, in falling, started down this hole. Suppose a vacuum in this hole. The apple goes faster and faster, but not forever, for as Newton knew, an apple started at the other end of the hole would fall the other way. It seems certain that after passing the center of the earth the apple would slow down. Good. The law covering this retardation is the same as that covering the earlier acceleration. Further, right at the middle of the earth, the attraction of gravity would be zero, though the apple is the same size; in short, we must think of a new property of matter, its mass. The apple's tendency to fall is a variable, depending upon its position with reference to the center of the earth, but this new property, its mass, is constant. We need a principle or two to cover the situation, so Newton formulates his first law of motion: every body persists in its state of rest or uniform rectilinear motion unless compelled to change by forces impressed upon it.

[1] History of Science Society, *Sir Isaac Newton*, 1928, p. 95. In paper by Michael Idovrsky Pupin.

We now see that our apple is shuttling back and forth in this long vacuum tube forever—yes, forever, and without a change. Well, this is a rather fantastic picture, because we have no tube and never can have it; but hold, we do have motion going on forever. It is not oscillatory, but circular or elliptic, for we have the planets revolving about the sun, and satellites about the planets. Can we add to our first law so as to include this circular motion? As a consequence of the first law, a satellite would go off on a tangent unless something prevented it. The thing that prevents it is clearly the body around which it revolves, so there must be a force acting at right angles to the tangent which forever operates and just compensates for the tendency to fly off. We clearly need the following, which is the second law of motion: the alteration of motion is ever proportional to and in the direction of the motive force impressed.

At this stage of the thinking process the apple and the satellites such as the moon are subsumed under the same principle. The line of thought here given, though not of the commonness of the sort, "Is the apple wormy?" is not at all beyond our comprehension. The next stage is more difficult, for in the elaboration of the hypothesis it is found that a new mode of mathematical treatment and statement is necessary. Newton knew Kepler's laws that the orbits of the planets were elliptical and that the cubes of the mean diameters of these ellipses are proportional to the squares of the times of rotation. But the reverse problem: assume that the momentum of a body, or its tendency to continue at the same velocity in a straight line, is some function of its mass and velocity, and assume that the attraction between two bodies is some function of their masses and of the distance between their centers (and not only this but some function of the distance between each particle of the one and

each particle of the other), then what are these functions in order that the path of the smaller body shall be elliptical? Apparently, assuming that the masses of the bodies were concentrated at their centers, Newton solved this problem in a matter of months and reached his tremendous generalizations that any two bodies attract each other with a force proportional to the product of their masses and inversely proportional to the square of the distance between them. But at this time, 1666, Newton was not himself completely satisfied with his experimental verification.

He did not announce this law for nearly twenty years. Three reasons have been advanced: first, his distaste for argument; second, his attempt to verify it by showing that both the moon and a body at the surface of the earth obey it, was unsatisfactory because of a wrong value for the diameter of the earth; and third, that this attempt was inconclusive to himself, because he was unable for nearly twenty years to solve the problem involving attraction between each particle of a first body and each particle of a second body, as opposed to that between two points—their centers. It is not unlikely that all these reasons operated.

It was nearly twenty years after his first discovery that he established that attraction between two spheres each outside the other is the same as it would be if their masses were concentrated at their centers. Some years before this Jean Picard had provided a new and more accurate measure of the diameter of the earth. Fortified with these new data a recalculation was made and when a check between reality and theory was indicated, Newton was so perturbed that he had to call in an assistant to finish the calculations. Was there ever a greater moment in a man's life than this? Mathematics, physics, the smallest particles on the earth, the entire solar system, and knowledge of God's intention all snapped together at once.

I include a knowledge of God's intention because Newton was a deist—a very serious deist, who wrote even more extensively upon religion than he did upon science. He took law and order in the world as personal evidence of a God. There was no part of his colossal understanding or of his deep religious faith that was not stirred to its depths by this tremendous discovery. Is not an experience of this sort the greatest intellectual satisfaction that the world can give, and is it not reserved for the scientist alone?

I cannot detail other discoveries made by Newton, but I must mention his lucid statements upon scientific method. He lays down four rules: (1) Parsimony: we are not to assume more causes than sufficient to answer for explaining the observed facts. (2) Similar effects must be assigned to the same cause. (3) Properties common to all bodies within reach of our experiment are to be assumed as pertaining to all bodies—*e.g.*, extension. (4) Propositions in science obtained by wide induction are to be regarded as exactly or approximately true until phenomena or experiments show that they may be corrected or are liable to exceptions.[1]

Newton was very modest and retiring. He looked upon himself as a child upon a seashore, picking up a small pebble here, a bright shell there, "while the great ocean of truth lay all undiscovered before him." He so disliked controversial matters that he delayed publication of important findings, printed theological observations anonymously, and contemplated posthumous publication. His labors seem to be roughly equally divided between the scientific work that more than that of any other man is the foundation of modern science, religious exposition that probably has added nothing to speculative thought, and alchemy, which is the occasion of actual regret in the minds of not a few modern

[1] Sedgwick, W. T., and Tyler, H. W., *A Short History of Science*, 1917, p. 295.

scientists. His alchemical range of information was great, and his experiments very extensive and fruitless.

Is this not an excellent illustration of inability to get blood from a turnip? The trouble seemed to be that in his modesty he followed in this matter the old masters, of whom there were many, as he thought, but, as they were not masters, he went astray with them. In physics much less was written. He built upon Kepler and Galileo and struck forth alone with the result that we all know. As Kepler labored long and fruitlessly with astrology, so did Newton with alchemy. Neither seems to have had the acumen in separating the chaff from the wheat that was possessed by Galileo, who revered no man but Archimedes as his master.

Newton was ill-adapted to hold his own with the philosophers. From experience and from his heart he said: [1]

> Philosophy is such an impertinently litigious Lady, that a man had as good be engaged to lawsuits, as to have to do with her.

The sort of criticism that must have tried him sorely is illustrated by that of the great Bishop Berkeley, who said: [2]

> He who can digest a second or third fluxion, a second or third difference, need not, methinks, be squeamish about any point in Divinity. And what are these fluxions? The velocities of evanescent increments. And what are these evanescent increments? They are neither finite quantities, nor quantities infinitely small, nor yet nothing. May we not call them ghosts of departed quantities?

Bishop Berkeley was not presenting the view of a narrow churchman, but of a broad humanitarian, a view that is found in cultured circles today. Why should one be squeamish about transubstantiation or the immaculate conception if he accepts the "ghosts of departed quantities" and builds

[1] History of Science Society, *op. cit.*, p. 176. In paper by Florian Cajori.
[2] Sedgwick and Tyler, *op. cit.*, p. 298.

a vast superstructure upon them. The very statement of the problem shows the gulf between the two points of view. Had Bishop Berkeley experienced the thrill of Newton when he proved the law of gravity, he could have answered his own question. He would have known that the trustworthiness of science was so incomparably greater than that of authority that he would not even have put the question. Faraday knew the difference. He said: [1]

> Ultimately facts are the only thing which we are *sure* are worthy of trust.

Contrast this statement of Faraday's with that of a leader of the opposing school today, Dr. Nicholas Murray Butler. Let me quote from his 1927 report as President of Columbia University. He writes:

> Part of the difficulty may be found in the fact that science has been suffering from what may be described as a superiority complex which has prevented it from realizing its true place in the scheme of things. There is certainly no region or realm into which science does not or ought not to aim to penetrate, *on the plane in which science moves*. But that plane is, as every scholar in the field of human thinking must realize, a subordinate one. It is the plane upon which the world appears as made up not of definite and independent objects, but of indefinite series of changing units whose inter-relations and inter-dependences are all-important and all-controlling. To science no object is independent. Each depends on every other and dependence—relativity—is the controlling principle of the universe. There remains, however, that still higher plane upon which the universe appears as a self-dependent, self-related, self-active totality. It is on this plane that philosophy lives and moves and has its being, and on this plane that art and music and literature find the inspiration and the motive of those insights, aspirations, and intuitions which pave the path to beauty.
>
> . . . [Further] science has been in large part badly taught and in large part is badly taught today. The sole reliance upon

[1] Jones, Bence, *Life and Letters of Faraday*, 1870, Vol. I, p. 306.

the laboratory method for introducing students to an understanding of scientific method, scientific fact, and scientific accomplishment is well nigh disastrous.

Dr. Butler goes on to describe what he conceives to be the proper method of instruction in science. It is the lecture method, and its outcome would be that the study would "take hold (of the student) . . . with redoubled power because he sees himself dealing with a vast and continuing human interest." In other words, it finally attains the value to the student that is exactly that of the classics. Its long and respectable history gives it weight. The view is that of Bishop Berkeley over again. True, science has a history that is eminently worthy of study, but this is not the heart of science. Until one has devised an experiment to test a hypothesis, has carried it out and observed carefully, and had the outcome break upon him as new knowledge, he does not know what science is. Had Dr. Butler looked through the telescope in Galileo's stead and seen the satellites of Jupiter and known that at that moment a new universe was given to mankind, would he have advocated the lecture method as a means of inducting students into science? It is inconceivable. Dr. Butler refers to philosophy, art, music, literature, as operating upon "that higher plane upon which the universe is a self-dependent, self-related, self-active totality." This is a fantasy. Enjoy it if you can, but believe it never. The sensed totality is but the shaky structure of a feeble mind. After all "facts are the only thing which we are sure are worthy of trust."

Dr. Butler argues for the interpreting of science. Now this can be done in two ways. One is by a very careful logical analysis of the implications and consequences of established scientific principles and the other is by analogy: as in the universe so in the atom; as phylogenetically, so ontogenetically; as in the mind of man so in that of God.

No one doubts the scientific and cultural value of the first type of interpretation, but of the second type, that which leads to extensive similes, grandiloquent expression, and which makes the universe *appear* to be united in all its parts, a very different tale must be told.

Galen saw but half the truth because his God was complete and a unit, and, reasoning by analogy, so must also be the body of man. Kepler knew the perfect numbers of the mystics and fixed the numbers of planets and satellites accordingly; and even the great Newton, reasoning by analogy from sound, saw in his spectrum seven colors only, and missed not only the continuity of the infinite gradations between them but also the ultra-violet and the infra-red. It seems no overstatement to say that argument by analogy has been the greatest cause of error and the greatest restriction of vision of scientists of all time. It undoubtedly has been a great asset, too. We cannot do away with it, but it should be employed only in the tentative solution phase of a problem. Its fickle nature is attested by the fact that there are no rules of logic which apply to it. Who would attempt to lay down a set of principles telling when analogy is sound and when it is not?

Reasoning by analogy seems to have been in the past a marked weakness of scientific men. It is so today. Let me quote from Michael I. Pupin.[1]

> [The spiritual world and the physical world] are both governed by similar laws which are the precious extracts of human experience. Just as the activities of physical forces have been summed up in Newton's dynamics, Maxwell's electro-dynamics, and Carnot's thermodynamics, so the activities of the spiritual forces have been summed up by Christ in his spiritual dynamics. He formulated its two fundamental laws when he said:

[1] Pupin, Michael I., "Creative Coordination," *School and Society*, Oct. 29, 1927, Vol. XXVI, No. 670. (An address at the inauguration of William Mather Lewis as President of Lafayette College, Eastern Pennsylvania, October 20, 1927.)

> Thou shalt love the Lord thy God with all thy heart, and with all thy soul, and with all thy mind.
> Thou shalt love thy neighbor as thyself.
>
> Employing the language of science, we can say that these two commandments are the message from Christ's spiritual dynamics informing us that creative coördination rules supreme in the spiritual just as it does in the physical world.

The advice for conduct given in this quotation seems to me so fine that I would in no way suggest a modification of it, but I must insist that as an argument the quotation is fallacious. Let me condense the statement. Though it will spoil its beauty, it will reveal logic or lack of logic within it. Pupin states that just as Newton provided laws governing physical forces, *so* Christ provided laws governing spiritual forces. Newton drew his inferences from observable measurable relationships, and every step of his processes has been duplicated many times by subsequent investigators. Whatever was the process of Christ in reaching his spiritual laws, no one has attempted to establish that they can be objectively observed, measured, and verified. Surely it would be a calamity if Dr. Pupin could take faith, hope, and charity out of the process. However beautiful Christ's spiritual laws, they do not rest on the same basis as do the laws of experimental science. It detracts from the peculiar and unique dignity not only of science but also of Christianity to say that they do.

I believe that we need religion, that we need social standards of right conduct, that we need the stabilizing effect of history, custom, and consensus of opinion. But to say that the one source that we have for obtaining new knowledge, namely verified inferences from observation of the world about us, takes an inferior position to these is a position in which I cannot agree with Dr. Butler. Science need doff its hat in subservience to no other discipline of mankind.

When a scientist attempts to reason by analogy in his own work, he does so at great risk; and when he does it before the public, it may become the occasion of distrust and ridicule. Listen to the following abstract of a paper given at a serious scientific meeting and quoted by a serious journal: [1]

> Dr.——extended Sir Arthur's history of mankind back to pelagic times, saying: "We owe our appreciation of dancing, poetry, music, and our sense of rhythm to the actions we made when we were only tiny blobs of jelly flagellates, millions of years ago."

Here is a bit of interpretation. Does Dr. Butler approve of this, and if not, upon what basis does he draw the line? I maintain that the idea here quoted can serve no purpose in the planning of an experiment or in the understanding of man. If it is meant as a joke, it is out of place in the setting in which it appeared, and if meant seriously its only effect is to hold science up to scorn and derision. It is true that the history of science reveals that many eminent scientists have at times drawn no clear distinction between inferences deducible from facts and unsupported speculations, but such confusion is less and less defensible as the corpus of scientific knowledge grows.

An attempt to form a composite picture of mental traits of men of science will show many ill-defined features. There are at least two important reasons for this. First: the traits demanded in one scientific field differ from those in a second. Second: the traits facilitating success in one generation differ from those in a second having different social restrictions. With these two conditioning factors in mind we may still make a few generalizations. My study of men of science, a sample of which I have given here, leads me to think that there are no exceptions to the following:

[1] *Time*, Sept. 12, 1927.

(a) The great man of science is industrious, shows great mental energy, and is persistent on the trail of a discovery.

(b) He questions authority, at least in the one line of his greatest achievement.

(c) He is apt at drawing inferences, and is therefore ingenious in making hypotheses.

(d) His sense of logic is sound, so that he is perspicacious in making deductions.

(e) He is a keen observer of natural phenomena.

(f) He is dependent on observed facts.

(g) He is inventive in the matter of techniques.

(h) He is rich in his variety and number of hypotheses.

(i) He is not "inspirational" in his chosen field, i. e., his feet are always on the ground—his hypotheses are always amenable to some test of a factual or observational sort. But he is inspirational in the sense that a vision not the common property of fellow-men urges him on.

The following are common, but not universal, traits of the scientific man:

(j) He lacks personal attachment to a hypothesis—he will slay his own mental offspring, or if he does not actually kill an erroneous hypothesis that he has given birth to, he will let it die from inattention.

(k) He is disputatious.

(l) In his person much transfer of training takes place, for he adopts to one field a device, method, or hypothesis drawn from another field.

(m) He is versatile in his interests, and even in his native abilities.

(n) He is tolerant.

The following are traits which are not uncommon:

(o) In his person erroneous transfer takes place, for he reasons by analogy without warrant.

(p) He has a good memory.

(q) He has good mathematical ability, even though mathematics is not his major activity.

(r) He has excellent motor coordination and manipulative ability.

(s) He has a thorough knowledge of antecedent work.
(t) He has a deep religious feeling.
(u) He is generous.
(v) He is precocious in his development.

The following are traits which are not unknown in men of science:

(w) He is egotistical.
(x) He is modest.
(y) He has hobbies.
(z) He is not sociable.
(aa) His ancestors are of more than ordinary distinction.

I have endeavored to draw a bird's-eye picture with the hope of a better understanding of the racial development of science and of its expression within individual scientists, and I have also considered certain things that have held back the race and the individual. The tugs that facilitate forward motion are (1) talents, (2) training, (3) facilities—the first of these is quite likely as difficult to secure today as in the past. The hold-backs that slow down progress are (1) individual stupidity, prejudice, and preconceptions, and (2) social taboos. The first of these are probably as ubiquitous today as in the past. Three things, however, are encouraging for rapid advance in the future: (a) it should be possible to adapt training so as to encourage a scientific type of thinking; (b) it should be possible to provide better facilities for the conduct of scientific work; and (c) it should be within reason to expect that society itself will further lift its taboos upon what men shall think and do.

INDEX

Abelard, Pierre, 209
American Historical Association
  Committee of Eight, 153, 156, 157
  Committee of Five, 153, 157
  Committee of Seven, 153, 158
Analogy, 182–183, 224–225, 227
Apollonius, 204
Archimedes, 204, 222
Aristotelians, 213, 215
Aristotle, 193–194, 216
Association experiment, 108
Attitude, 104–113
Attitude toward the church, 109–112

Bacon, Francis, 192–196, 197, 198, 217
Bacon, Roger, 133, 203, 209–211, 214, 216
Baker, H., 154, 155
Baldwin, Bird T., 89
Berkeley, George, 222–224
Bernheim, E., 17
Bernoulli brothers, 217
Bias, 15–17
Bode, B. H., 128, 131
Boyle, Robert, 217
Brahe, Tycho, 217
Brandeis, Justice, 163–164
Brolyer, Cecil R., 94
Butler, N. M., 223–227

Cardinal Principles of Secondary Education, 158–159
Commission on the Reorganization of Secondary Education, 153
Concept formation, 175–177
Conference method, 152–164
Consensus of opinion, 37–38, 43, 45
  See also Questionnaire
Copernicus, Nicolaus, 193, 205, 211–212
Core curricula, 99–103
Cotes, Roger, 217
Courtis, S. A., 96
Credit, Giving, 37–71
Culture, 7–8
Curriculum construction, 32–35, 165–171, 177, 180, 190

Darwin, Charles, 13, 25–26, 196–205
Darwin, Francis, 173
Data, 120–125
Descartes, Rene, 217
Dewey, John, 23–24, 126, 129–130, 139, 140, 147, 176–177
Differentiable mental abilities, 96–97, 188
  See also Individual differences

Einstein, Albert, 133, 146, 149
Error, 36, 211
  See also Probable error
Euclid, 204
Eugenics, 181–191
Evolution, 179–185, 187, 191
  See also Eugenics
"Exact science," 133
Experimental method, 9–12, 20–35, 73, 169–175, 177, 190, 200, 203, 207–208, 210, 216, 219–222, 226

Faraday, Michael, 205, 223
Fermat, Pierre de, 217
Forecasting, 30–32, 181
See also Future time
Franklin, Benjamin, 217
Free association experiment, 108
Fructifying thought structure, 147
Future time, 9–12, 22, 25, 30
See also Forecasting

Galen, 204, 206–209, 216, 225
Galileo, Galilei, 2, 16, 193, 203, 205, 209–217, 222
Galton, Francis, 160, 183
Gibbs, Willard, 196, 205
Growth, 84–88, 90

Halley, Edmund, 218
Hartshorne, Hugh, 109, 113
Harvey, William, 209, 217
Hippocrates, 204, 206, 207
Historical method, 9–20, 24–25, 43
History, 8–9, 171–172, 194, 198
Huxley, T. H., 201–202
Huygens, Christian, 214
Hypothesis, 198–200, 215

Independent mental traits
See Differentiable mental abilities
Individual differences, 177–180, 188–189, 190
Indoctrination, 102–103
Induction, 192, 194, 197–198
Intolerance
See Tolerance

Jeans, Sir James, 149, 150
Jesuits, 215
Jevons, W. S., 20
Judgment, 30–38, 120–125, 171–174
See also Questionnaire

Kelley, T. L., 83, 179
Kepler, Johannes, 83, 179, 193, 211, 217, 219, 222, 225
Keynes, J. M., 20
Kilpatrick, W. H., 126–129, 132, 139, 142, 143, 144

Lamarck, 206
Langlois, C. V., 20
Learning, 178
Literature, 18–19
Logic, 10–11, 147–148, 206–207, 225
Lyell, Charles, 205

Maclaurin, Colin, 217
Make-shifts, 135
Maller, Julius B., 108, 113
Malthus, T. R., 200
May, Mark A., 109, 113
Memory, 25–28
Mendel, Gregor J., 2, 206
Mendel, L. B., 174–175
Michelson, A. A., 23
Mirrielees, Lucia B., Preface
Moivre, Abraham de, 217
Moseley, H. G. J., 16
Mozart, Wolfgang A., 16

Napier, John, 217
National Education Association
Committee of Fifteen, 1893, 153
Committee of Ten, 1892, 153, 154, 155, 159
Native ability
See Nature and nurture
Nature and nurture, 27–29, 83–87, 91, 93–94
Newton, Isaac, 181, 192, 193, 196, 205, 215
Nurture, 217–223, 225–226
See also Nature and nurture

Pasteur, Louis, 175, 193, 196, 205
Philosophy, 10–11, 119–151
Picard, Jean, 220
Present time, 8–12, 25, 30, 170
Principles, Measurement of, 114–118

Probable error, 22
Pupin, Michael I., 225–226
Pythagoras, 204

Questionnaire, 24–25, 35–74

Ranke, Leopold von, 16
Raphael Santi, 16
Regression, 183–185
Research activities, Classification of, 6 ff.
Research, methods of, 1–8; subsidized, 3–5
See also Experimental method, Historical method, Forecasting, Logic
Richards, T. W., 145
Romanes, G. J., 202
Russell, Bertrand, 133

Science, Biological, Physical, Social, 6–9, 119–151
Scientific method, 157
See also Experimental method, Historical method, Forecasting, Logic
Scientific thinking, 148–150
Sedgwick, A. 197, 199
Seignebos, C., 20
Sensed differences, 80–83, 91–92, 95
Shambaugh, C. G., Preface
Sidgwick, Henry, 20
Social studies, 98–118
Socratic ideal, 102
Spencer, Herbert, 6, 193, 198
Stanford University, Council of Research in the Social Sciences, Preface
Stetson, H. T., 150

Taylor, Brook, 217
Taylor, H. O., 171
Terman, Lewis M., 89
Tests, Construction of, 82–83, 86–87
Thorndike, E. L., 167
Thought, of complete act of, 23–24, 27, 199
Thurstone, L. L., 96, 108, 109
Tolerance, 189
Traits of men of science, 195–197, 199–204, 209, 210–211, 214, 216–217, 221–222, 227–229

Uniform development, Doctrine of, 180
Units of measurement, 75–97

Values, Future
See Values, Mental
Values, Mental, 32–38, 43
See also Judgment, Forecasting, Questionnaire
Values, Social
See Values, Mental
Variability, 183–185
Vesalius, 207
Villari, P., 20
Vinci, Leonardo da, 194, 205, 206
Voltaire, 210, 218

Watson, Goodwin, 113
Weismann, August, 206
Whitehead, A. N., 134
Wiley, W. E., 92
Wilson, Edwin B., 173
Woodworth, R. S., 27
Word knowledge, 34, 80–82
See also Values, Mental