agement. Finally, the group returned to Detroit and, after spending a day exchanging ideas obtained from their plant visits, presented their comments and suggestions to interested members of our Corporation management, the personnel staff, and personnel representatives from our divisions and plants.

In order to give a greater number of educators an opportunity to gain this kind of experience so that they will be in a better position to present their students with a more realistic picture of what they can expect when they accept industrial employment, this experiment should not be confined to one company or one field of education. We should consider its application or the application of other methods to the fields of science, engineering, finance, and distribution, among others.

I firmly believe that by pooling the experience and "know-how" of employers with that of faculty members and administration of our colleges and universities we will find better ways of producing technical and scientific graduates who are better qualified to do their jobs and more interested in making these fields a career wherever they choose to accept employment. This cooperative attack from a long-range point of view will, it seems to me, help to insure more effective use of technical and scientific graduates in industry. This may be more important than producing a larger number of graduates, many of whom leave this field of work after they are trained for it. Let us put greater emphasis on producing quality rather than on just increasing the number of graduates in the technical and scientific fields.

The Scope of Science

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WHEN, SOME MONTHS AGO, I FIRST read the paragraphs in British publications which provide the text and motivation for this article, I was so amazed and dismayed by what seem to me partially erroneous and wholly misleading statements that I felt impelled to make immediate reply. But on further reflection, being of a noncontroversial disposition and much averse to giving my time to expression of disagreement, I inhibited the impulse. Recently there have been developments which brought the matter to my mind again, and this time with such sense of duty and opportunity that I am now offering critical rejoinder and alternative view. Unwillingly I write in the first person, since I may not assume that the British writers speak for their scientific colleagues or the British people, and since I have no mandate to represent my scientific colleagues and my fellow citizens of the United States of America.

The first half of my text appears in the following paragraph from an editorial entitled "The method and scope of science," published in "a quarterly review designed to record the progress of the sciences in the service of mankind," which is "made and printed in Great Britain." The journal is called *Endeavour*.

> It is, however, in the realm of qualities not susceptible of measurement of any kind that the scientific method—as far as it has yet developed—ought frankly to be recognized as *inapplicable*. An obvious example can be taken from the field of art. The scientific method can give a great deal of information on the chemical nature of pigments, on the wavelength of the light they reflect, and on similar factors, but *it is wholly unable to predict* whether a picture will have an aesthetic appeal to those who see it. *Nor can the scientific method be of help* in those problems relating to drama, literature, and the like, which involve *qualities that cannot be measured* and knowledge which is not communicable. In the wide field of human affairs *the scientific method cannot be applied*, even in the form of statistical analysis, to problems in which events are influenced by the philosophical values of goodness, truth, and beauty, and emotions such as patriotism, fear, or political conviction (*Endeavour*, 1946, **5**, 126). [Italics by R. M. Y.]

In this emphatic statement of opinion I should wish, on the basis of my knowledge of fact, to convert all the negatives—the impossibles and improbables—into positives. Thus "inapplicable" becomes "applicable"; "it is wholly unable to predict" becomes "it is partially able to predict"; "nor can the scientific method be of help" becomes "the scientific method can be of help"; "qualities that cannot be measured" becomes "there are no qualities of natural phenomena which cannot be measured, although at present there are many which are not being measured or are measured very crudely and inaccurately"; and "the scientific method cannot be applied" becomes simply "the scientific method can be applied to all natural phenomena."

Actually, what the writer of the editorial deems impossible I have observed being done in what chances to be my country, culture, and sphere of creative scholarship, on all sides and increasingly throughout my life. For nearly 50 years most of my time and intellectual resources have been devoted to efforts, sometimes recognized as successful, to do just what is termed impossible

or improbable. Under the circumstances, it would require much more than an exhibit of authoritarian belief or emphatic assertion of individual opinion to convince me—and I am sure that in this I may speak for hundreds of my scientific colleagues and associates—that what I have observed by way of research endeavor and progress is illusory, and that we who are working as biologists and social scientists are not achieving dependable and serviceable results in our varied studies of behavior, experience, social relations, and institutions. If the natural phenomena designated by such terms are not amenable to systematic study by procedures which are in principle the same as those employed in the physical sciences, hosts of us who now are classified as scientists actually are self-deceived workers who, unlike our physical science colleagues, are denied access to the truth concerning the natural phenomena which particularly interest us.

My proof? As exhibit in support of the positive statements which I have made, I offer simply the literature of technical report and general discussion classified under functional biology, psychology and psychobiology, anthropology, sociology, and other so-called social science categories. I have in mind, as especially pertinent, the literature of the 20th Century. It seems idle to cite titles from our numerous journals, monograph series, and books. With regret I admit that I am a bit suspicious that the negatives of the editorial, and the writer's obvious lack of faith in the applicability of scientific procedures to psychological and social phenomena, indicate ignorance of this vast array of recent discovery and of technical applications in various branches of human engineering. Perhaps there is also a tendency to look backward in the history of science instead of examining current activities and, with that as vantage ground, trying to look forward optimistically and prophetically. Assuredly, history, and especially the history of science and of man, is invaluable for our guidance, but its making is far more exciting and fate determining than is mere recording or subsequent rewriting. One dislikes to admit that he appears to be living in another world of fact and striving than that of his opponent, and I earnestly hope that the writer of this editorial and I shortly may discover that we both are scientists in spirit and achievement and may come to agree that assertion or prediction of the impossible is an unprofitable exercise in our callings which not infrequently makes one appear foolish to one's successors. I cannot even be so charitable as to wish that I may come to accept the point of view or faith of this writer, for I am reasonably sure that he is mistaken, and I hope to convince him, and all who sympathize with him, of that fact!

As to the second half of my text, less words of comment are required since it is a specific exhibit which might have been used in *Endeavour* in support of the general negations. I borrow it from an important report of effort to further human welfare by application of dependable knowledge. *The Peckham experiment*, by Innes H. Pearse and Lucy H. Crocker, physician and social worker, respectively, commands my admiration and, apart from such exceptional disagreement as I herein note, my enthusiastic approval. It deserves to be widely read, and it should become influential. My interest in the book led me months ago to urge that it be made available in an American edition. Perhaps if I knew the authors as well as I now know their experimental outlook and services, I should not have the heart to criticize them! The paragraph to which I wish to take exception is entitled "The Place of Psychology in the Peckham Experiment." It reads thus:

> One last word. The Centre has often been accused of neglecting Psychology. In our opinion, however, there is as yet no psychology; only a knowledge of psycho-*pathology*. Indeed, how can there be a scientific study of psychology until an experimental field for the *study of the healthy* has been established? At present, knowledge of the physiology of the psychological mechanism lags far behind its pathology which fills libraries, for all knowledge so far available on this subject is derived from the disordered, and its theories and technique are devoted to remedy. Remedy is important for the sick, but the sick are not the primary concern of the Centre. Once however a medium is found in which we can watch the action-pattern of function in the family unit, we are presented with an experimental field in which we can begin to look for a psychology based upon health rather than pathology (*The Peckham experiment: a study of the living structure of society*. New Haven: Yale Univ. Press, 1945. Pp. 272–273).

The assurance and finality of statement in this case are devastating. As a psychobiologist whose affiliations and services for a half-century have been primarily medical, and as one who recognizes the existence and value of psychology as extension of physiology to include the whole of human behavior and experience, I am deeply sympathetic because of the disadvantages under which the authors of this report labored. By their own statement, it is in a land and culture in which, apart from psychopathology, there is no science of psychology. But in this matter, what of the world situation? And what, I apprehensively ask myself, will the thousands of American psychologists, my colleagues and co-workers, think and say when they read: "In our opinion, however, there is as yet no psychology." Will they respect the efforts of our British co-workers for human welfare, or will they instead say that such ignorance, narrowness of view, and provincialism are inexcusable? Science generally is considered international, and it certainly should be reasonably independent of local advantages and disadvantages.

In this case also I offer as eloquent exhibit to disprove the authors' negatives the world's literature of psychological science and especially that of America during the current century. Reports of research progress are to be found in a score of technical journals and other serials. Our handbooks, manuals, encyclopedias of psychological fact, method, and generalization are many. In America psychopathology is recognized as special development

from, and an important part of, the science of normal behavior-experience. If the various divisions of biology, functional and structural, are entitled to acceptance as natural sciences of measurement and prediction, then, from the facts provided by these exhibits, we must grant that psychology actually exists as science. Moreover, whether or not we chance to be medically trained or interested especially in psychopathology, we psychologists and psychobiologists are working in the spirit, with the objectives, and, in principle, with the methodologies of the other physical and biological natural sciences.

Obviously, my disagreement with the authors of *The Peckham experiment* is absolute. Nevertheless, as scientists we all are committed to strive for progress, and most of us recognize that what today appears impossible may be achieved tomorrow. I should like to suggest, if it be not presumptuous, that either the Crown or the Prime Minister of Great Britain appoint a commission of experts to visit the United States of America expressly to inquire into the existence and status of the "impossible" sciences of human behavior-experience, social organization, and institutions, and to report authoritatively and informatively to the people of Great Britain. Belief and action should not depend upon the assertions of the editors of *Endeavour*, of the authors of *The Peckham experiment*, or of an American scientist who refuses to admit that it is impossible for any one person to be physicist, biologist, sociologist, all in one, and to seek always to advance knowledge and human welfare as *scientist*, sometimes as *specialist*.

On the Multiple Factor Theory of Respiratory Control as Outlined by Gray

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A RECENT ARTICLE ON A MULTIPLE factor theory of control of respiratory ventilation (*4*) carries certain wide implications for which the author did not present the necessary substantiating data. Furthermore, many of the statements seem highly questionable, if not erroneous; e.g. a categorical statement that all preceding theories of control have "consistently met with failure" is presented without evidence.

Gray states that Gesell's theory "avoided the objections to the arterial H-ion theory" and that "the improvement, however, is more apparent than real, for the H-ion concentration of the cells of the respiratory center is still beyond measurement." If this statement means that Gesell's theory is difficult to handle quantitatively, we must agree. Otherwise, the meaning is not clear.

There is a considerable body of evidence that respiratory center acidity and chemoceptor acidity run parallel to respiratory minute volume. Cerebrospinal fluid acidity (*2*) offers a first approximation of central nervous system acidity. These approximations have been abundantly supported by observations on venous blood acidity. Present information on central nervous system chemistry and metabolism permits related deductions concerning the acidity of the center (*7*). Whatever method is employed to appraise intracellular acidity of the respiratory center and chemoceptors, the premise that intracellular acidity plays an important role in the control of respiratory ventilation is supported (*2*).

Gray seems to have accepted the views of Gesell on this major issue of intracellular acidity, for he writes as follows:

> The second step in the application of the multiple factor principle is to decide at what point the concentrations of the three chemical agents are to be measured. Although ideally their concentrations should be measured in the respiratory center and the peripheral chemoreceptors where they exert their effects, this is not feasible.

This is a point which cannot be passed over lightly, for if one fact is established with reasonable certainty, it is the lack of correspondence between the concentration of H-ions and of CO_2 in the arterial blood and the volume of respiratory ventilation. Of all the points of concentration, arterial blood is the least informative. Yet Gray states that "the most feasible approach is to correlate respiratory ventilation with the arterial concentration of the three chemical agents, bearing in mind the possibility that difficulties may arise in transitory unsteady states and in conditions of seriously altered blood flow."

It is a well-known fact that venous blood is usually more acid than arterial blood and that the increase in