CORRESPONDENCE

To the Editor of *Philosophy*

SIR ARTHUR EDDINGTON'S THEORIES

Dear Sir,

I am glad that Professor Reichenbach realizes that my polemic against Sir Arthur Eddington is not really personal. But since the personal element seems to have misled him (and therefore probably others) concerning the real issue, may I try to state it in strictly impersonal terms?

Can a distinction be made between the experimental and theoretical elements of physics? Professor Reichenbach will not deny that there is a difference between (say) measuring the spectrum of a substance and interpreting the measurements in terms of atomic structure; in practice the two elements are so distinct that they are often the work of quite different people. But he may deny that, as I assert, they are wholly separable. I admit fully that they are not actually separated by the prevailing use of the words "law" and "theory," and that therefore I may have been unwise to use those terms. I admit further that in all scientific propositions, as usually stated, the two elements are confused to some extent; in particular, theoretical terms are habitually used to describe experimental facts. But I maintain that the elements can be separated; that the experimental element can be isolated by stating all "laws" in the form that certain experiments, not necessarily describable in words, can be demonstrated; and that the part so isolated contains all of physics that has any practical "authority." I recognize that imaginary experiments (Gedankenversuche) present a difficulty, but I believe it can be overcome. I am not sure whether Professor Reichenbach would agree with me so far, but almost all physicists who have actual experimental experience would.

If the distinction is admitted, the question of the relation of the two elements arises. Sir Arthur Eddington would probably hold that they differ only in degree; that a theory concerns exactly the same "reality" as a law, but is a fuller, more profound, and truer account of it; that it differs from a law in somewhat the same way as an adult's account of some complicated event differs from a child's. (*Law* and *theory* are here used in my sense, of course.) On the other hand, I hold that they differ in kind, in somewhat the same way as the statement that Brutus killed Cæsar differs from the statement that Brutus was right to kill Cæsar, and that therefore they must be carefully distinguished in considering the philosophical implications of science. Which of the two views is right can be determined only by a careful study of the relation between the experimental and theoretical elements in some typical branch of actual physics. My own solution of this problem is contained in my *Physics: The Elements* (Camb. Univ. Press, 1919), though naturally now I should amend it slightly. My quarrel with Professor Reichenbach and most other philosophers of science is not that they have given a different solution, but that they have ignored the problem altogether.

Yours faithfully,
Norman R. Campbell.

To the Editor of *Philosophy*

SIR ARTHUR EDDINGTON'S THEORIES

Sir,

It is of much interest for me to hear from Dr. Campbell himself that my interpretation of his opinion was true, and that his article really was based on the conception of a precise disjunction between experimental and theoretical physics. Though this is only one point of my former letter, it seems to be the basis of the

difference between Dr. Campbell's opinion and mine, and therefore I may add some words now.

I do not deny that there is a difference between facts and theories, but what I deny is that laws are of the form of facts: they are nothing but theories of a more narrow form. And I must add that what a physicist calls a fact is a theory too: the real facts are sensuous impressions like blue and green and rigid, and to say that there are things of certain physical character is going further from facts to theories. Thus proceeding from absolute facts to propositions about "external" things is a way marked by the steps "physical facts," "laws," and "theories," but there is no sharp *frontière* between these steps. What can only be said is that the probability of the proposition gradually decreases as we proceed. The physicist is not always conscious of this: he takes an "observed" spectral line as a "fact," and a relation between observed lines as a "factual law," as for instance the law of Balmer. But what he could only maintain here as a fact is that he saw some dark and light spots on a photograph—he never sees spectral lines, but must deduce them from the observed spots by theoretical construction.

The instance given by Dr. Campbell is not of the form of transition occurring in physics. "Brutus killed Cæsar" is of the character of a "physical fact," that is, it is deduced from facts (like the sensuous data in reading ancient chronicles) by theoretical construction. "Brutus was right to kill Cæsar" is no statement at all, because it states nothing about the world, but only informs us about a certain feeling of the speaker, his feeling of justice. In the whole of physics there is no proposition of this kind at all.

This view of facts being a principal train of my theory of knowledge (*e.g.* in my *Ziele und Wege der physikalischen Erkenntnis*, *Handbuch der Physik*, Bd. 4, 1829. Verlag J. Springer, Berlin), why should I be charged with having ignored the problem of facts? I think every theory of knowledge must deal with the given view of facts, if it wants to give account of what an experimental physicist *does*, and not of what he *thinks he does*.

Yours faithfully,

August 1, 1931. HANS REICHENBACH.

NOTICE

THE first volume of the *Collected Papers of Charles Sanders Peirce*, scientist, logician and founder of pragmatism, has just been published by the Harvard University Press. This volume is entitled *The Principles of Philosophy*, and is composed mainly of papers previously unpublished. It contains his system in outline, and his more important papers on the methods and classification of the sciences, phenomenology, or the doctrine of the categories, ethics, and æsthetics. It will be sold at $4.50. The entire works will consist of about ten volumes; those subscribing to all the volumes will be entitled to a discount of 20 per cent. The second volume dealing with traditional logic, signs, methods of discovery, induction, and probability will follow very shortly. Nearly all the members of the Department of Philosophy at Harvard, as well as other friends of Peirce, have devoted much time to these manuscripts of these papers. The final work of arranging the papers and preparing them for the press has been done by Dr. Charles Hartshorne and Dr. Paul Weiss.