necessary only to experiment upon much thinner layers still, in order to obtain the vanishing amount of modification by scattering. This, however, was beyond the range of experiment.

What we have shown is that by experimenting on a suitable radiation, a perfectly regular development of the modified scattered radiation can be traced to the superposition of unmodified radiation from thin layers. This further illustrates what we have previously described as the coherence of superposed X-radiations. Neither quanta nor wave-trains within narrow limits of wave-length can be considered independent in their action; it is the whole stream of radiation which is effective.

C. G. BARKLA.

University of Edinburgh.

Philosophical Foundations of Quantum Theory.

IN his very lucid and interesting article (this is no empty compliment) in NATURE of April 16, Dr. Jordan makes two misstatements. On p. 569 he says that in C. T. R. Wilson's experiments the time of a single quantum jump is a measurable quantity. But those experiments involve no time measurements at all. Time enters only through the velocity of the particles; and if inquiry is made how it enters into the value assigned to this velocity, it will be found that the time measurements concerned are made on large aggregates of atoms and have nothing directly to do with quantum jumps. Again, he says that the experiments of Geiger and Bothe and of Compton prove that the interval between emission and absorption is exactly that of the light path between the atoms concerned. But all that these experiments proved was that the interval was less than 10^{-3} sec.; the interval of the light path was about 10^{-9} sec.

Of course Dr. Jordan knew that these statements were not wholly accurate; a limit to accuracy is always set by brevity, and he would doubtless reply that the inaccuracy does not affect his argument. But if there is anything in certain vague ideas which I have twice tried to present to physicists (NATURE, 107, 170; 1921; *Phil. Mag.*, 1, 1106; 1926), it does affect his argument very greatly. I must apologise if I am too pertinacious, but every serious writer on the difficulties of quantum theory gives me the same excuse as Dr. Jordan. They all develop their argument up to a point where (as it seems to me) they are bound to notice my suggestion, if only to reject it; they then make some statement about time that is patently false, and, without noticing it, proceed on some different line of thought. If only somebody would explain why the suggestion is too silly to be worth discussing, there would be an end of it, once and for all.

Briefly, the suggestion is that time is a statistical conception, significant only with regard to large aggregates of atoms; and that it is as meaningless to speak of the time interval between atomic events as of the temperature of an isolated molecule. If that suggestion is right, some of Dr. Jordan's questions are answered or become unanswerable. He asks: Will it ever happen that the time of a quantum jump is undetermined? Certainly, for there is no such time.

The conception of a statistical time is, of course, not easy. But the general nature of the influence which it would have on our ideas can be grasped by means of an analogy. If all 'regular' clocks were abolished from our laboratories, and we were forced to use radium clocks, in which the defining events are the disintegration of individual atoms, it would be very difficult to demonstrate some of the experiments on which our conceptions of 'continuous processes' and 'causal relations' are based. That difficulty, I think, is precisely the difficulty which we encounter when we proceed from the world of atomic aggregates to that of individual atoms.

NORMAN R. CAMPBELL.

I SHOULD like, first of all, to express my regret that up to now Dr. Campbell's most interesting papers have escaped my notice. The expressions which I used, and to which Dr. Campbell takes exception, would certainly have been more precise had I taken account of Dr. Campbell's work.

As for Dr. Campbell's idea, I should like in the first place to point out that the matter has been considerably advanced by two papers by Dr. Dirac (P. A. M. Dirac, *Proc. Roy. Soc.*, London) and by me (P. Jordan, *Z. für Phys.*, 40, 809; 1927) on the foundations of quantum mechanics, and by Dr. Heisenberg's "Über den anschaulichen Inhalt der Quantenmechanik," which is based upon them (W. Heisenberg, *Z. für Phys.*, in press). These investigations corroborate Dr. Campbell's opinion in certain respects; on the other hand, they indicate certain limitations. Heisenberg has explained how the Cartesian co-ordinates, *e.g.* of an electron in hydrogen atoms, can be regarded as exactly measurable; and correspondingly one must consider the 'fourth co-ordinate' $q_4 = ict$ as exactly definable and measurable. The difficulties of a measurement of t which are brought out by Dr. Campbell arise in the measurement of every physical quantity in an atom (*e.g.* energy). How and to what extent these difficulties can be overcome has been considered in detail by Heisenberg. In this respect, therefore, the quantum mechanical conceptions differ from those of Dr. Campbell.

In a certain respect Dr. Campbell's views are, however, confirmed by the quantum mechanics: for if the atom has specified quantum numbers, the time (and the co-ordinates) are statistically, and only statistically, defined. For the characteristic feature of the quantum mechanics is that one cannot specify simultaneously all of the $2f$ constants of integration of the classical dynamised system, and, in particular, that one cannot specify both a co-ordinate and its conjugate momentum. Similarly, one may specify the energies of the initial and final states of a quantum jump; then the time of the jump is indeterminate. But one can equally well specify the time of the jump, and leave unspecified the initial and final states; and within certain limits of accuracy one can specify both the initial and final states and the time.

Undoubtedly this discussion is too short and too inaccurate to elucidate the point completely. I should like, therefore, to refer Dr. Campbell again to Heisenberg's paper, in which these questions are treated in detail.

P. JORDAN.

The Law of Flame Speeds.

IN NATURE of Dec. 11, 1926, p. 837, Prof. W. A. Bone stated that he would at some future date publish the results of experiments on the 'uniform movement' of flame which disproved the law of speeds. The work referred to has now been published in the *Proceedings of the Royal Society* (A, 114, 420; 1927) and we can reply to Prof. Bone's letter. The principal mixtures he has chosen to test the law of speeds are of ethylene and acetylene with oxygen, and the choice is made because, to use Prof. Bone's own expression, these mixtures are so 'sensitive' (*i.e.* highly responsive to accidental changes in experimental conditions).

We have for some time been engaged in further study of the law of speeds with the view of ascertaining its meaning. We have not hitherto, in our experiments on the uniform movement, used mixtures of