

appears impossible that sufficient heat from atmospheric friction was transferred to the interior of the tektites to allow them to melt and take on the observed shapes.

For the reasons given above we consider that, as yet, no satisfactory meteoritic theory has been advanced to explain the origin of tektites.

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June 10.

¹ C. Fenner, "Australites", Part 1. "Classification of the W. H. C. Shaw Collection", *Trans. Roy. Soc. South Australia*, 58, 63 and 64; 1934.

² A. Lacroix, "Les Tectites de L'Indochine", Extract of the *Arch. Muséum National d'Histoire Naturelle*, 6^{me} Ser., 8; 1932. C. Fenner, *op. cit.*, pp. 62-79.

³ A. Lacroix, *op. cit.*, p. 211.

⁴ W. J. Fisher, Harvard College Observatory Circular, 385, 1934.

Philosophy and Modern Science

I AM sorry if my psychological knowledge fails to reach the standard required by Dr. Dawes Hicks¹. My book² has, as a matter of fact, been the subject of favourable notice in three psychological journals, though I cannot claim the universal agreement that would apparently be necessary to make it convincing to Dr. Dingle. I have done my best to explain what I mean by the terms used.

What Dr. Dawes Hicks calls a 'sensus' seems to be what Russell called a 'sense-datum'; and I do not use sense-data, partly because most of the alleged existing sense-data are not perceived and therefore are not part of our fundamental knowledge, and partly because some inference and refinement are necessary before we can pass from our immediate knowledge to even the perceived sense-data. I am not sure whether I accept Dr. Hicks's other definition; his words 'mental act' seem to presuppose that we know what we mean by mind and that there is some kind of activity in merely having a sensation; the former seems to me to be posterior, not anterior, to sensation, and the latter is I think untrue. When my eyes are open I am aware of various patches of different shapes and colours, which disappear when my eyes are closed. Those are what I mean by a sensation. They seem to be immediate and not the result of any activity on my part; they just happen. In the case of sensations of sound even the minor preliminary activity of opening my eyes is unnecessary.

Again, Dr. Hicks's definition of a concept presupposes the existence of universals, and apparently denies the existence of different degrees of universality. Here I definitely disagree. A single observation of Neptune is merely a bright spot surrounded by blackness, with appropriate modification in the case of a photographic observation. It is only when many observations are available that we can form the idea of a single body moving in a definite way; generalisation has already been applied in thinking of Neptune at all. The individuals are merely a certain finite number of bright spots.

I am quite clear that I do not experience another person's sensations. He can tell me about them, and I may believe what he says; but a long process of inference has been involved before I can attach a meaning to this statement and put them on a similar footing to my own. It is well known to astronomers that the observations of different

observers need certain corrections before they become comparable among themselves.

I agree with Dr. Campbell's second paragraph; I should also agree with his first if I was sure that he does not regard my view as a philosophy. I regard it as a description of the method of acquirement of knowledge. For example, what I call a concept is substantially what Pearson called a construct; but I prefer not to use the latter term because it suggests acceptance of the phenomenalist philosophy, while I think that there is nothing in my work that could not be equally well accepted by a critical realist.

Though I do not necessarily agree with Prof. Levy entirely, I consider him clearly right in objecting to Prof. Dingle's adoption of universal agreement as a fundamental criterion while trying to maintain a critical attitude about the reality of the external world, since other people are part of that world. My own opinion is that both are inferred from much more fundamental data.

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¹ NATURE, 135, 1035, June 22, 1935.
² "Scientific Inference", 1931.

DR. NORMAN CAMPBELL is so far right that in the philosophical interpretation of science conceptions about reality and existence lie at the very core of the matter. To me, these are primaries and indefinables. They can be explained only in terms of human practice; that is to say, again only in terms of the active side of reality and existence. The 'logical network' view cannot embody in its scheme this activist side of reality, for it is basically static and contemplative in character and purpose. A social philosophy of science, on the other hand, automatically makes man's capacity for changing the world an integral part of its story. The purpose of the philosophy is both conscious and dynamic.

It is for this reason that Mr. C. O. Bartrum¹ misses the point when he argues that the difference between the two views is merely one of words. The extent to which words may indeed arouse confusion is apparent when Mr. Bartrum quotes me as saying "that the man of science should be responsible for the social consequences of his work" and when he pictures the terrifying results that would follow in every laboratory if this were accepted. What I actually wrote was that my view keeps "the scientist alive to the social consequences". There is an ocean of difference between the two statements.

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NATURE, 135, 1036, June 22, 1935.

Origin of Man Again?

THE controversy on "Special Creation and Evolution"¹ continues to recur periodically in spite of the fact that it has long been known to scholars and learned Kabbalists that the Biblical narrative of *Genesis* is a representation of Chaldean allegories. To-day in many branches of science, writers can expound their knowledge for the uninitiated only by means of similes, metaphors and analogies; and yet it is still the practice to translate and interpret