

theory of the atom; the basis of the fundamental hypothesis $\int pdq = nh$ is purely mathematical, and cannot be stated apart from mathematical conceptions. So also, I think, does the Maxwellian theory of the electromagnetic field, but to discuss this matter would lead us too far. All that I am concerned to assert is that there is no sense of the word "real," relevant to experimental physics, in which the principle of relativity has a different kind of influence on our views of what is real from any other theory. In particular, it has no influence whatever on the belief that matter is real in any scientific sense. It may have some bearing on that doctrine in the metaphysical sense; but since, after considerable philosophic reading, I am still unable to discover what metaphysicians mean by "real," I clearly cannot discuss that question. But since, again, I can understand science without understanding metaphysics, I am naturally convinced that the two are completely independent.

NORMAN R. CAMPBELL.

DR. NORMAN CAMPBELL has not understood me. Probably thinking that I am an idealist philosopher, he has supposed that I must be arguing that there is no scientific reality in the accepted meaning—that is, no scientific criterion of reality—and that the naturalist's mongoose, for example, has just as much or just as little reality as the drunkard's. What I was pointing out was the fact that the principle of relativity is the rejection of materialism. Materialism is a causal theory of scientific reality. It is the argument that when we pronounce anything in our sense-experience to be real we imply an independent cause of it. According to the principle of relativity, the inference is entirely unnecessary and to insist on it unscientific. Instead of this causal theory relativity offers a simple correspondence theory. The Minkowski-Einstein universe consists of events co-ordinated by observers in their different systems of reference. What is essential to constitute the "real event" of any observer is that there should be point-to-point correspondence between his co-ordination of it and the different co-ordinations of other observers. The co-ordination of an event by any observer—that is, his perspective of the event—is not an effect which is the appearance to him of a "causal" reality, but an actual case in point of the reality itself. The "event" in the four-dimensional continuum, and its track the "world-line," in re-forming the notion of scientific reality has relegated scientific materialism to its right place in the limbo of scholasticism. Whatever his disagreement, at least Dr. Campbell need not be alarmed for the basis of scientific research.

November 9.

H. WILDON CARR.

Hybridity and the Evolution of Species.

I AM sorry to say that the postscript of "The Writer of the Article" to my letter on p. 274 in NATURE of October 27 is not according to facts. It was he who used *Trillium*, *Dirca*, and *Scolioopus* as evidence against "bad pollen" being an indication of hybridity; this evidence appeared to me to be insufficient, and I stated the reason why. In his postscript "The Writer of the Article" makes no attempt to refute my arguments against his view, but says: "In such cases as *Trillium*, *Dirca*, and *Scolioopus* it is not sufficient for him [meaning me] to suggest that they must be hybrids merely because they have bad pollen," though I have never suggested this, or referred to *Trillium*, *Dirca*, and *Scolioopus* in any of my previous writings.

NO. 2717, VOL. 108]

Perhaps I may be allowed to make use of this occasion to state my point of view shortly with regard to the question of bad pollen. I do not think that bad pollen is proof of a hybrid origin, but consider it as "suspect"; neither do I share Jeffrey's view that absence of bad pollen is a sign of a non-hybrid origin; as a fact, I know that it is not. Some of my segregates of the cross *Antirrhinum glutinosum* × *majus* have bad pollen, while others have not. I further think that "The Writer of the Article" is mistaken in his view that the theory of mutation requires the occurrence of a certain proportion of defective germ-cells. The facts are these:—When de Vries found bad pollen in *Oenothera Lamarckiana* he accounted for its presence on the assumption that this defect was caused by mutations; we now know that *O. Lamarckiana* is a hybrid, so that it is much more probable that hybridity is the cause of the presence of bad pollen; recent cytological work seems even to prove this.

May I beg zoologists to answer a question I should like to put to them, namely: Is there any evidence that the presence of oligopyrene and apyrene sperms in some insects and molluscs is due to hybridity?

I might finish my remarks here were it not that "The Writer of the Article" reproaches me with "begging the question" at issue. Nothing is farther removed from my intentions, so that I desire to deal shortly with all the points mentioned by him. He argues that it militates against the general applicability of the origin of species by hybridisation that not all British roses are hybrids. I fail to see the force of this argument, as it is well known that homozygotes can arise from a cross without showing any sign whatever of their hybrid origin; consequently, the fact of specific purity can never be used as an argument against a hybrid origin.

Nor does the fact that pollen sterility and fertility behave as a pair of characters in the sweet pea and the velvet bean tell against the origin of that bad pollen by hybridisation, as "The Writer of the Article" seems to think, unless he can bring forward arguments in favour of his contention which are unknown to me. Until then I must acknowledge I fail to see how behaviour of a character already existing can reveal its mode of origin; the idea that specific characters do not segregate while varietal characters do is, of course, obsolete.

"The Writer of the Article" finishes his remarks by pointing out that it will be necessary to bring some more convincing argument in support of hybridisation as a constructive evolutionary factor before it is likely to receive much serious consideration from biologists. If he means some more convincing argument than the suggested hybrid nature of *Trillium*, *Dirca*, and *Scolioopus*—a suggestion which is not mine—I cordially agree with him. I wonder whether the following will assist him in taking a kinder view of my theory than he evidently does?

It is a generally acknowledged fact that new breeds of animals and plants can arise by crossing, while no other mode of origin of them has ever been proved, although various other modes have been suggested. We all know that Darwin explained the origin of new forms in Nature largely on the mode of origin of domesticated races, so that it is of considerable importance to know the real nature of the "variations" among plants and animals under domestication which play so important a rôle in Darwin's writings.

Some time ago I happened to come across a letter of Darwin himself in his "Life and Letters," which seems to throw important light on this momentous question. The letter is printed in full on p. 342 of the third volume of the "Life and Letters"; it was