Journal of Scientific Exploration, Vol. 3, No. 2, pp. 217–219, 1989
Pergamon Press plc. Printed in the USA.

0892–3310/89 $3.00+.00

BOOK REVIEW

The Relativity Question, by Ian McCausland. Toronto: published by the author, Department of Electrical Engineering, University of Toronto, 1988, v + 111 pp. (paper).

In 1972, Herbert Dingle—a philosopher and acknowledged expert in relativity—published Science at the Crossroads (London: Martin Brian & O'Keeffe), a classic in the literature of scientific controversy. The book describes Dingle's attempt to have substantively addressed his claim that the formulation of the special theory of relativity is paradoxical.

According to the special theory, two clocks in uniform motion relative to one another keep (or perceive) time at different rates. But since there exists no preferred frame of reference against which to measure the speeds with which the clocks move, there is also no basis for deciding which of the clocks runs faster and which runs slower. Is that not paradoxical?

One obvious possible answer to Dingle's claim of paradox would be that neither of the two clocks actually runs faster than the other, it is just that each sees the other as running slower (or faster?). Some of the acknowledged experts in fact proffered that answer; but most did not, and some cited experimental evidence to show that the disparity in time-passing is physically real. Others again, despite having taught relativity to students, admitted to Dingle that they had not the insight into the theory to answer the "subtle point" he had raised. Most of those few who accepted Dingle's challenge—and also the editors of Nature—resorted to patent obfuscation: for instance John Ziman, who answered the question as to which clock runs faster thus: "the fastest working clock between any two events is one that travels between them by free fall." Never mind, apparently, that the special theory of relativity deals only with relative uniform motion, whereby gravitation and free fall and acceleration cannot be taken into consideration. (Einstein himself had committed this mistake when, in his original paper, he gave as illustration that a clock at the equator would run slower than one at the pole.)

Dingle died in 1978 without having received what seemed to him a satisfactory substantive answer to his question. Now Ian McCausland has taken up the story where Dingle's book left it, by commenting on the reviews that Dingle's book received and by relating his own attempts to have Dingle's Question receive a substantive answer. Both Dingle's book and McCausland's afford the opportunity to learn about little-recognized aspects of scientific activity; and the books should be required reading most particularly for those who attempt scientific exploration of anomalous phenomena.

Those who whiff "crank" at the hint of anti-Einstein or anti-relativity should be reassured: this is no attack on the *general* theory of relativity, in which time-dilation accompanies acceleration; nor is any claim being made as to what actually happens when two clocks are in uniform relative motion (if only because it is not taken for granted that there actually exists no physically preferred frame of reference). The Dingle Question is simply intended to show self-inconsistencies in the *formulation* of the *special* theory: the claim is simply that it is paradoxical to assert that two identical clocks, in a situation where there exists no basis for distinguishing the one from the other, can keep time at different rates, that time for those two clocks can actually pass at different rates.

In response, one has to say either, "Dingle is obviously right"; or—and much more commonly—one says, "Relativity is so complicated, recondite, subtle a matter that I can't possibly think about it; if the experts tell me that Dingle is wrong, and if their experiments seem to support them—or at least don't contradict them—then I have to believe them." And so "The Relativity Question" exemplifies the dilemma we all find ourselves in: matters of science and technology, matters indeed of life and death, are decided by the experts in language that seeks to exclude the laity; and we hesitate to push the experts to explain because we doubt our own ability to distinguish sense from nonsense when specialized technicalities are under discussion. Surely, however, one *can* discern it when a Ziman (see quote above) evades a question? Surely one *can* draw a conclusion when a President of the Royal Society (cited, with documentation, by Dingle) confesses lack of understanding of a subject that he teaches?

In this reviewer's opinion, this controversy illustrates several points that are germane to students of the anomalous:

1. When something works in science, it is used *and thereby implicitly accepted,* even if there is no physical basis for the mathematical formulation or if the physical basis ascribed to the mathematics is doubtful or even illogical. And further, the experts will deny that the physical interpretation is illogical or that any problem exists with it (unless and until some alternative view is in the offing).

This state of affairs runs counter, of course, to the popular view of science; but there are many examples of the refusal to see something wrong with an inadequate theory so long as no better alternative has been proposed: apart from the Dingle Question, note the acceptance over decades of the concept of the potential-independent electrochemical transfer-coefficient (Henry H. Bauer, 1968, *Journal of Electroanalytical Chemistry, 16,* 419–432); or, the acceptance by biochemists of the chemically meaningless concept of "energy-rich" bonds (Daniel E. Atkinson, 1988, *Science, 242,* 946–947); or, the ignoring of statistical fallacies in epidemiologic studies (Alvan R. Feinstein, 1988, *Science, 242,* 1257–1263). (I would be grateful for references to other instances of this, which one might call the ostrich phenomenon.)

2. Mainstream journals cannot cope well with unorthodoxy or controversy.

Even when they wish to be open to minority views—if only so as not to miss out on publishing a breakthrough—ultimately they must bow to the orthodoxy imposed by the processes of peer review. As a result, one periodically sees unorthodoxy published (though not without a struggle) only to find it quickly abandoned again or even attacked by the very periodical that first published it (whereupon the proponents of the unorthodoxy feel stabbed in the back, though they would also have been furious if they had not achieved publication in the first place). Nature's handling of the Dingle affair, well documented by McCausland and Dingle, is a case in point; earlier there had been erroneous denigration of sonar apparatus used in Loch Ness, and inept handling of remote-viewing studies; recently, the Benveniste affair. (Again, I would be grateful for references to other similar instances.)

3. Perhaps stimulated by frustration over these two types of response, protagonists of unorthodox views easily slip into making excessive claims. Thus Dingle sought to have authoritative bodies take formal part in his quest for an answer—when authority simply can have no proper *intellectual* role in science; and he sought to buttress that attempt by claiming that society finds itself in some great (albeit undefined) danger because of the acceptance by atomic physicists of the erroneous theory.

Finally, this controversy could help to teach humility by reminding us how very difficult indeed it is to advance intellectually, and that we all are prone to lapses that seem inexplicable to other reasonable people. Thus John Ziman's evasion of the Dingle Question seems incongruous since he has written arguably the best, authentic, accounts of science as an intellectual *and* social activity, in *Public Knowledge, The Force of Knowledge,* and *Reliable Knowledge* (all Cambridge University Press, respectively 1968, 1976 and 1978). If Ziman has such blind spots, then surely I—we all—have some too, and moreover of the very worst sort: ones we are incapable of knowing about.

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