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LETTERS

What is appalling is on one hand the waste of talent, training and experience; and on the other the frustration and embitterment of individuals who have devoted a good part of their lives to preparing for a rewarding vocation of, they are told, tremendous social significance. They do not usually begin their education dedicated to the ultimate study of hadrons, gravitons, excitons or rotons. They bring to it instead a love of physics and a desire to do something with it. Those of us who are responsible for shaping their education guide them to certain topics and skills and then give them a document that, we claim, certifies that they are competent in something worth being competent in. In the past they have always had the possibility of, in effect, continuing their graduate study indefinitely; so they never questioned the validity of their elders' guidance. Now Herbert Hersh writes (*PHYSICS TODAY*, May, page 11) that at the New York placement service he saw "... a lot of green PhD's coming into a difficult labor market that their graduate schools and professors had not prepared them for. There is an employment crisis." There is a crisis, and it is far deeper and more bitter than a matter of jobs. It is a crisis that has already begun to alienate the young and beginning physicists from the schools, from the "Establishment," from all the forces that have shaped a physics environment that no longer appears valid to them. This environment has to change, or they will stay angry, even if they do get jobs in other fields; anger is no monopoly of the poor. This anger, and this frustration, and this bitterness, can be as great a threat to the future of physics as, on a broader scale, they are to the future of higher education.

It is imperative that the physics community respond to the crisis that faces it; the most pressing area in which change is needed is in the schools, but they are not independent either of each other or of the "outside world" of physics, especially the federal government. It is therefore vital to have the active participation and leadership of AIP, its member societies and publications. If AIP and the rest of the physics Establishment do not concern themselves more seriously than they have with what is going on, the alienation to

which I referred above will become total, and the glorious space age will be remembered not as the flowering of physics, but as its funeral.

WILLIAM SILVERT
Case Western Reserve University

AN AUTHOR REPLIES: The information on job offers was collected as part of AIP's annual graduate-physics-student survey. If a student identifies himself as a 1967-68 graduate-degree candidate who is looking for employment, he answers the following request for information: "State the number of written job offers received in anticipation of your degree."

The questionnaires were mailed out during the second half of May with a follow-up mailing around 1 July, and we received the responses throughout June, July and August.

In May 1969 we sent a "letter of enquiry" to those who indicated zero job offers during the summer of 1968. This letter was designed to determine the present employment status of those new physicists. If these one-year-old addresses are still good, we hope to publish the results shortly.

SUSANNE D. ELLIS
American Institute of Physics

Continental drift?

I wish to point out what appears to be an overenthusiastic advocacy of continental drift by Donald L. Turcotte and E. Ronald Oxburgh (*PHYSICS TODAY*, April, page 30). This they expressed as "Today most earth scientists accept continental drift as a geological fact" and "In the last five years verification of ocean-floor spreading has changed the status of continental drift from speculative conjecture to a generally accepted fact." The physicist reading this and the rest of the article will be misled as to the present status of the concept of continental drift. It is still very much in the argumentative stage, although the authors ignore the important evidence against it, mostly faunal.¹ It may soon reach a state of generally accepted theory, but now discussions of this concept are most apt to turn into a good argument.

The authors' main evidence centers around three possibly interrelated phenomena: polar wandering, sea-floor spreading and continental drift. There is a tremendous body of evidence for sea-floor spreading, most of it quite

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In addition, the distribution of certain marine fossils in the Permian (approximately 250 million years ago) suggests that the position of temperature zones with respect to our current geographic poles was similar to that of today.³

It is thus clear that the authors have overstated their evidence as well as the acceptance of theory. I am not attempting to say that it is all nonsense. There *is* evidence for continental drift as well as against it. One of the best evidences is the fit of rocks on both sides of the South Atlantic (their figure 1) [below]. The point is, the jury is still out!

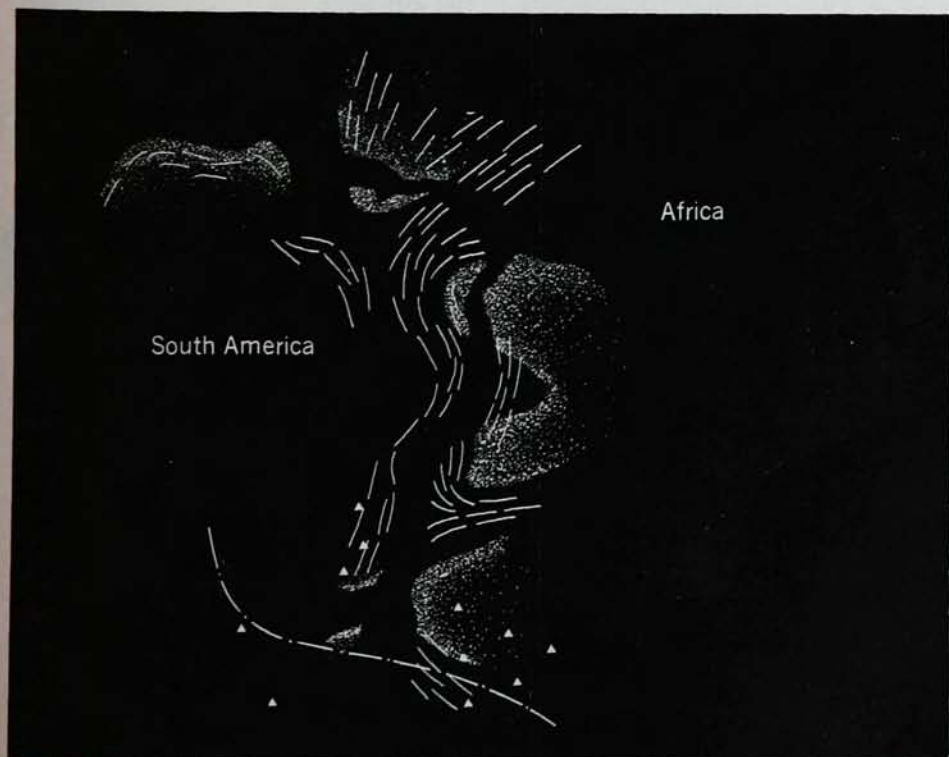
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WILLIAM R. COOK
Clevite Corp

THE AUTHORS REPLY: Cook takes exception to our statement: "Today most earth scientists accept continental drift as a geological fact." Neither we nor, we imagine, Cook have conducted a formal opinion poll on this question, but a fair indication of the way active earth scientists feel is gained from the abstracts of the hundreds of papers presented at the annual meetings of the American Geophysical Union and the Geological Society of America over the last two or three years. A rough count of relevant papers suggests something like 99 papers accepting, or in favor of, drift for every 1 against.

To say that a hypothesis is generally



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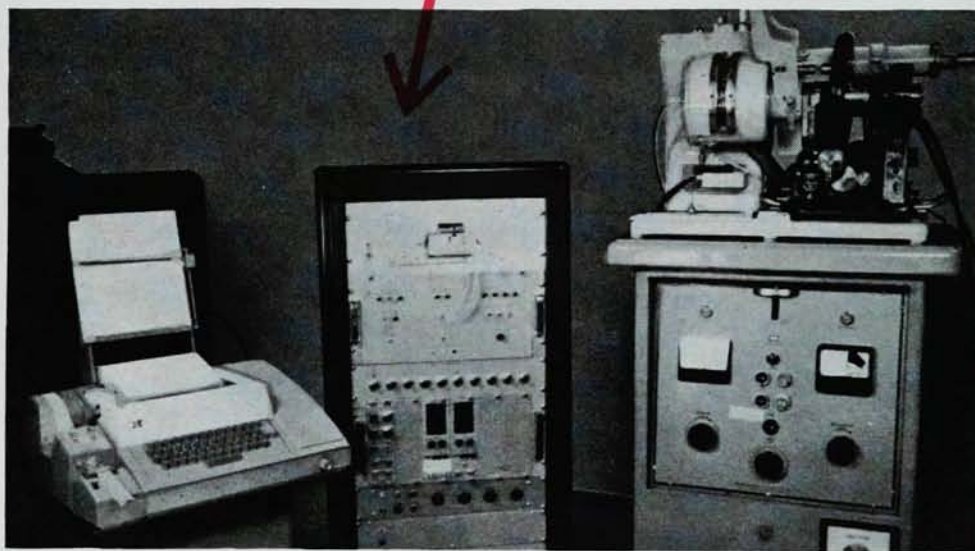
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accepted, however, is very different from saying that it is so complete that no problems remain. But, as we tried to show, the volume of supporting evidence from many different sources is so great that it now appears to the majority of scientists more reasonable to attribute any apparent conflict of other observations to our inadequate understanding of the phenomena to which they relate, rather than to question the drift hypothesis as a whole. We feel, however, that most of the points raised by Cook have been adequately dealt with in the published literature.

To consider the specific points raised: Faunal evidence may, with great caution, be used to test the feasibility of any particular reconstruction of the continents. Some of the reconstructions proposed in the past have been incorrect, and palaeontological evidence has helped demonstrate this in particular cases. We do not see how faunal evidence can be used to construct a general case against drift.

As stated in our article, we feel that the evidence in favor of the destruction of crust in oceanic trenches is very strong¹; our solution to the case Cook mentions is shown in our figure 7. That severely deformed sedimentary trench fillings are not found is surprising only until it is realized that sediment that has been severely deformed no longer contains planar reflecting horizons and becomes seismically opaque. The strongly deformed sediment probably forms the opaque material on the trench wall against the island arc.

We agree that divergence of polar-wandering paths for different continents is an important indication of drift, and we discuss the relationship between the geographic and magnetic poles. Cook is wrong to suppose that the reef-distribution argument depends on assumptions of the constancy of width of climatic belts. It depends only on the assumption that climatic belts have retained throughout geological time a generally symmetrical distribution about the geographic pole. The coral-reef test, however, was an important test in another way; if repositioning the reefs at the latitudes indicated by palaeomagnetic evidence had, for example, located them all at the north pole we should have strong reason to doubt the method. In fact,

the distributions of all kinds of latitude-dependent deposits make good sense when restored to their palaeomagnetic latitudes.²

As for Cook's point in his penultimate paragraph, the drift hypothesis does not preclude the possibility of certain continental areas having similar climates today to those that they had in Permian times. To retain Cook's metaphor, we believe that the jury came back several years ago.

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DONALD L. TURCOTTE
Cornell University
E. RONALD OXBURGH
University of Oxford

What happened to my journal?

In his article "What Happened to My Paper?" (PHYSICS TODAY, May, page 23) Samuel A. Goudsmit gives us valuable insight into the difficult job of preparing papers for publication in *The Physical Review*. But what happens to the printed issues? The latest I have seen is dated three months ago. Why the long delay (which can almost double the time from author to reader)? What happens to my journal, Dr Goudsmit?

ROBERT C. JOHNSON
University of Toronto

A REPLY FOR AIP: Goudsmit has asked me to respond. For *The Physical Review*, as for some 17 other journals, the American Institute of Physics takes over responsibility for copy editing and production once the articles have been accepted by the scientific editor's office. The handling of more than 60 000 pages per year, which amounts to around 25% of the world's journal literature in physics, is a major undertaking, and we are sometimes beset with difficulties that lower our performance to the level characteristic of many other of the scientific societies.

The Editorial Section at AIP, which took up a few lines in Goudsmit's article, numbers approximately 60 people, about one third of them working on *The Physical Review*. We have been hit hard by staff losses, and recruitment of competent people to be trained as copy editors has been

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