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## letters

6. C. J. Sparks, Jr. *et al.*, *Phys. Rev. Lett.* **40**, 507 (1978).

C. LEROY ELLENBERGER

Senior Editor, *Kronos*

St. Louis, Missouri

4/84

THE AUTHOR COMMENTS: All of Ellenberger's examples of absolute dating are based on the fallacious idea that the rates of geological processes have been constant. In particular, his claim that bristlecone pines do not produce multiple rings is directly contradicted<sup>1</sup> by experimental evidence. On a more fundamental level, all types of geologic and radiometric dating methods are undergirded by the uniformitarian principle, which is actually only an assumption that physical laws have remained unchanged throughout time.<sup>2</sup> This evolutionary premise is contradicted by the occurrence of polonium halos in Precambrian granites, a phenomenon I have suggested<sup>2,3</sup> is evidence of a virtually instantaneous creation of these rocks. I have challenged my evolutionary colleagues to synthesize a piece of granite with polonium halos as a means of falsifying this interpretation, but this has not been done. If the uniformitarian principle were valid, there is no reason why this synthesis should not be possible. Failure to meet this challenge provides unambiguous evidence that the uniformitarian principle is false.<sup>2</sup> Without the uniformitarian principle, none of the "absolute" dating methods referred to by Ellenberger have any credible basis.

Nearly all of Ellenberger's remarks about my involvement with superheavy elements in 1976 are in error. He is wrong in claiming that giant halos were subsequently explained by alpha-proton scattering from water; at the 1978 International Symposium on Superheavy Elements I showed there was no experimental evidence to support this hypothesis. On this basis alone, Ellenberger's accusation that I have ignored a potential explanation of the polonium halos is groundless. Moreover, if he had taken time to carefully understand the proton scattering hypothesis, he would have realized that it is not applicable to the point-like centers of the polonium halos. Also, an accurate reading of my reports would have made him aware that those radiocenters do not contain fluid inclusion.<sup>3</sup>

Lastly, by citing only the first author in his sixth reference, Ellenberger left the impression that I was not involved in the follow-up experiments on giant halos. A news account<sup>6</sup> of the 1978 symposium shows I reported on those experiments and retracted the initial claim of evidence for superheavy elements, something others in the original

experiments did not do. I believe my involvement with superheavy elements is indicative of an attitude different from what Ellenberger pictured in his letter; namely, I am willing to admit mistakes and change my views when the evidence demands it. As of this writing, the evidence that God placed his fingerprints in the primordial rocks of this earth stands on a firmer basis than ever before.<sup>2</sup> My colleagues in physics might reflect on the possibility that the Creator of the Universe is attempting to gain their attention through a unique and nonreproducible radioactive phenomenon in earth's basement rocks.

## References

1. W. E. Lammerts, *Creation Res. Soc. Quart.* **20**, 108 (1983).
2. R. V. Gentry, "Radiohalos in a Radiochronological and Cosmological Perspective," *Proc. 63rd Ann. Mtg. Pac. Div., AAAS*, Vol. 1, Part 3 (1984).
3. R. V. Gentry, *Science* **184**, 62 (1974); R. V. Gentry *et al.*, *Nature* **252**, 564 (1974).
4. R. V. Gentry, *EOS* **61**, 514 (1980); *PHYSICS TODAY*, October 1982, p. 13; *PHYSICS TODAY*, April 1983, p. 13.
5. R. V. Gentry, in *Proc. Int. Symp. on SHE*, Lubbock, Pergamon, New York (1978).
6. *Science News* **113**, No. 15, 236 (1978).

ROBERT V. GENTRY  
Knoxville, Tennessee

8/84

## Peer reviews

On the basis of a small amount of evidence, I put forth the hypothesis that peer reviews are too often irresponsible, if not malicious and legally defamatory. In the manner that many recognize and often practice as the scientific method, I am now in the process of gathering evidence to confirm my hypothesis. For this purpose I am requesting anecdotal material from those who believe they have had their opportunities in physics restricted by irresponsible peer reviews.

Perhaps all such problems would disappear if there were adequate funding for physics research. Perhaps the worst problems would be diminished if legal actions were occasionally undertaken. Perhaps just occasional reminders of the requirement of responsibility for the success of the system would minimize the rest of the problems. Perhaps there really are not as many problems as I guess, in which case there should be very little response to my request.

All letters will be treated in confidence, none will be photocopied, and none will pass the 49th parallel a second time. Conclusions based upon the evidence gathered will be presented to the Committee on Opportunities in

ANTHONY ARROTT  
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9/84

## Author review policy

I was disappointed that your response to R. S. Raghavan, who complained (August, page 84) that changes were made in his manuscript after he reviewed it, was not forthcoming. You say it is not usual for authors to see the final galley at magazines, but you could very well decide yourselves that the stature of PHYSICS TODAY requires you to ask your authors and correspondents to review all changes made by the editors at any point. Why don't you do so? You could give an immediate deadline or query the authors on the telephone to eliminate delays—but almost any delay would be better than having material appear over an author's name that the author would have corrected.

JAY M. PASACHOFF  
Williams College  
Hopkins Observatory

9/84

## Physics of soaring

I enjoyed Lloyd Hunter's cover story "The Art and Physics of Soaring" in PHYSICS TODAY (April, page 34), but one point needs to be corrected. Hunter notes that thermals drift with the wind, but he then ignores this drift when calculating the speed to fly to achieve best cross-country speed in a wind. One can insert the missing extra term into Hunter's expression for the time for cruising between thermals,  $t_D$ , and work it through, but it is more illuminating to arrive at the result by taking an imaginary flight in a sailplane.

We'll assume that the entire flight, consisting of climbs in thermals and glides between thermals, takes place in a constant horizontal wind. If our frame of reference is comoving with the sailplane, the speed to fly cannot depend on the wind, but only on the sailplane's polar curve, the climb rate achieved in thermals and the inter-thermal subsidence. Back in the earth's frame, we see the cross-country speed is obtained simply by adding the wind to the cross-country speed one would obtain in no-wind conditions. Thus the pilot can ignore the wind when calculating the optimum cruising speed at which to fly the sailplane.

In Lloyd Hunter's example, with a

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