# Letters

## Earth's Cores

In the interests of historical accuracy I would like to suggest a clarification of the following statement by Charles L. Drake and John C. Maxwell (3 July, p. 15) about the discovery of the earth's internal structure: "The fluid core was found through the seismological studies of Wiechert, Oldham, and Gutenberg, and the solid inner core by Lehmann in 1936."

Examination of the original publications shows that Wiechert, Oldham, and Gutenberg did not claim that the "core" they discovered is fluid. It was generally believed that the entire earth is solid until 1926, when Harold Jeffreys presented convincing evidence that the core is fluid (*1*). Contrary to the usual statement in modern textbooks, failure to observe transverse waves through the core was *not* sufficient to establish its fluidity. Conversely, Lehmann did not state that the inner core is solid in her 1936 paper; that was first suggested by Francis Birch in 1940, and was not established until much later (*2*).

Omission of first names in the sentence quoted above unintentionally conceals the fact (apparently known only to specialists) that one of the most interesting features of the earth's structure—its inner core—was discovered by a woman. Danish seismologist Inge Lehmann.

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**References and Notes**

1. H. Jeffreys, *Mon. Not. R. Astron. Soc. Geophys. Suppl.* **1**, 371 (1926).
2. For further details on the history of this subject, see S. G. Brush, *Am. J. Phys.* **48**, 705 (1980).

## Publication Credit

Derek de S. Price (Letters, 29 May, p. 986) proposes a mechanism for dividing credit on research papers in order to discourage putting many authors on a single paper. He suggests that each author be given equal "credit" (in whatever terms) so that in a paper with ten authors, each would receive 1/10 credit for the work. The scheme does not, however, take into account the relative contributions of the different authors. Our experience is that, at least in biomedical research settings, the first author has done the major portion of the work, as well as the writing of the manuscript, with subsequent authors contributing decreasing amounts.

Some time ago, for our own amusement, we attempted to quantify credit for a research paper. We felt that any formula should meet the following criteria:

1) The larger the number of authors, the less credit per author.

2) The first position should get the most credit, and in general the $i$th author should receive more points than the $(i + 1)$th author.

3) If the first author needed so much help, the first position should get less credit as the number of authors grows, and in general the $i$th author out of $N$ should receive more points than the $i$th author out of $N + 1$.

4) The first author should receive significantly more points than the second. This may be controversial, but we feel it is appropriate in biomedical settings.

We propose the formula

$$\text{Points} = [(1/i)/(1 + (1/2) + \cdots + (1/N))] \times 100$$

for the $i$th author out of $N$. (The points are standardized to 100 per paper.) Table 1 shows the resultant point distribution for up to $N = 6$ authors. This formula satisfies all properties above. Note that the ratio of points allotted to the $i$th and $j$th authors is always $j:i$, regardless of $N$. Thus concerning criterion 4, the first author always receives twice as many points as the second.

Table 1. Point distribution for up to $N = 6$ authors.

| Authors ($N$) | Position in list | | | | | |
|---|---|---|---|---|---|---|
| | 1 | 2 | 3 | 4 | 5 | 6 |
| 1 | 100 | | | | | |
| 2 | 67 | 33 | | | | |
| 3 | 55 | 27 | 18 | | | |
| 4 | 48 | 24 | 16 | 12 | | |
| 5 | 44 | 22 | 14 | 11 | 9 | |
| 6 | 41 | 20 | 13 | 10 | 8 | 7 |

In contrast, the more straightforward formula

$$\text{Points} = [(N + 1 - i)/(1 + 2 + \cdots + N)] \times 100$$

does not consistently satisfy either criterion 3 or 4 above. For example, if there are five authors, the first author receives only 33 points, compared with 27 for the second. Moreover, the fourth author out of five receives more points than if he or she had been fourth out of four.

We realize there is often a "last author" effect, whereby the laboratory director or principal investigator is put last on the list of authors, and that, psychologically, that person is given more credit for the paper than the previously listed authors. The paper is then referred to as "coming out of Ptolemy's group" (even though Ptolemy may never even have laid eyes on the paper), and the name of the first author is lost to posterity. This effect becomes more pronounced as the list of authors grows. We have deliberately not included such an effect as we do not wish to encourage this pernicious habit.

There are several immediate consequences of our scheme. The person most responsible for the work will have an incentive to keep the number of authors as small as possible. There will be much scuttling around as people reevaluate curricula vitae—their own and others—in light of the fact that it takes *14* papers being sixth out of six authors (25, being eighth out of eight) to equal one SAE (Sole Authorship Equivalent).

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Price, in his letter concerning multiple authorship of scientific papers (29 May, p. 986), appears to be proposing to legitimize the assessment of a scientist on the basis of the number of times his name appears in print by applying a factor which is at best marginal in its significance. Attributing to a given author fractional credit for a paper by dividing it by the number of co-authors assumes that the value of all papers is the same, irrespective of length, content, or number of collaborators. This contention is

clearly unjustifiable. In any event, it is incredible to me that anyone who is seriously attempting to evaluate the performance of a scientist should use any method based only, or even primarily, on number of publications irrespective of their content. Clearly it is equally invalid to assume that the value of a publication is proportional to its length. There are a number of instances in which a Nobel Prize has been awarded on the basis of a short note, often jointly authored. Obviously in such cases the committee has looked into the value of the publication with great care.

If university appointment committees, promotion committees, grant agency panels, or any similar collection of (we hope) intelligent people are not looking more than arithmetically into an author's publication list for the information they require, we should be rocking the boat as hard as we possibly can through our scientific societies (which are supposed to have some credibility politically) and our university councils. There is no valid way in which the numbers game, as applied to publications, can be used to determine anything important in the career of a scientist.

In this era of the computer, there is a growing desire to commit everything possible to numbers. Such a development may in the long run prove inevitable; but to do this, some way of quantifying quality and value must be devised if the quantification is to give anything other than a grossly misleading picture. I fear Price's suggestion may lead some people to believe that a minor modification to an invalid process may render it acceptable.

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## Hmong Deaths

We would like to comment on the notion of congenital weakness of the autonomic nervous system subsequent to inbreeding as an explanation for unexplained deaths of Hmong (Laotian) refugees (News and Comment, 29 May, p. 1008). Ethnographic reports on fieldwork with the Hmong in Thailand during the 1930's (*1*), and in Laos through the 1960's (*2, 3*) provide no evidence of inbreeding. In fact, in all reports, the rules of marriage preclude marriage within clans and suggest that consanguinity up to the third generation is viewed as an obstacle to marriage. In a report on the Meau (Hmong) of northern Thailand, it is noted (*4*) that the incest taboo is observed very strictly. Further, our own work with Hmong refugees in the state of Washington fails to support the hypothesis of inbreeding.

The lack of evidence for inbreeding does not, of course, preclude the possibility that familial factors may be implicated in the sudden deaths of the Hmong. It is interesting to note that, while obvious familial factors are involved in the development of cardiovascular disease in the United States, inbreeding has not been invoked as an explanation.

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**References**

1. H. A. Bernatzik, *Akha and Miao* (Human Relations Area Files, New Haven, Conn., 1970).
2. J. LeMoine, *Un Village Hmong Vert du Haut Laos* (Editions du Centre National de la Recherche Scientifique, Paris, 1972).
3. G. L. Barney, in *Southeast Asian Tribes, Minorities, and Nations*, P. Kunstadter, Ed. (Princeton University Press, Princeton, N.J., 1967), pp. 271–294.
4. H. A. Bernatzik, in *ibid.*, pp. 128, 289, 331.

With regard to the matter of the Hmong deaths, another related issue may be the interpretation which many Hmong ascribe to their dreams. The appearance of one's deceased grandmother in a dream is an indication that grandmother's spirit did indeed come and visit the dreamer during the night and is not merely a psychological event. In addition, there are certain dreams or nightmares which indicate bad luck or the likelihood of death in the family (for example, seeing a water buffalo cross one's path, or visits from deceased relatives). At a weekly psychiatric clinic which I conduct for Hmong people here in Minnesota, the complaint of nightmares and night terrors is especially frequent (it is also frequent among the other Indochinese refugees).

The traditional Hmong interpretation of dream events would not in itself account for the predominance of young male deaths, since women and older people also have such nightmares and night terrors. However, preliminary tabulation of a mental health survey conducted among the Hmong in Minnesota (by Tou Fu Vang and myself) indicates a higher self-report of depression and other symptoms among the men. This is the reverse of self-reports from other ethnic groups in which women tend to report more symptoms than men. As Margaret Mead once noted about the peoples of Oceania, rapid culture change appears to affect men more adversely than it affects women.

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## Argentinean Scientist Flees

I would like to call attention to the plight of a distinguished fellow scientist from Argentina. Nicolas Bazan is a well-known biochemist who works in the area of brain fatty acid metabolism and nutrition and biochemistry of the retina. He has been professor at the National Southern University in Argentina and the director of its Biochemical Research Institute. His contributions were sufficiently recognized that the *Journal of Neurochemistry* invited Bazan to become a member of its advisory board last year. In January 1981, Bazan began serving on the board.

In March 1981, Bazan attended the annual meeting of the American Society for Neurochemistry in Richmond, Virginia. When he returned home, he found that in his absence he had been summarily dismissed from all academic posts. The Argentinean government apparently refused to give the reason for his dismissal. Bazan initially fought to get himself reinstated. However, after receiving threats, he fled the country in late June.

Those of us who know Bazan well are very disturbed by these developments. He is an internationally known scientist. To the best knowledge of any of us, Bazan has not been involved in the internal politics of his country. Wrecking a career of a distinguished scientist by an abrupt dismissal without due cause—or at least without stated cause—for what appear to be purely political reasons is a cause for concern to all scientists, although it is particularly distressing to those of us who know Bazan well.

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*Erratum*: In the report "Plasmid DNA in *Treponema pallidum* (Nichols): Potential for antibiotic resistance by syphilis bacteria" (31 July, p. 553), the authors inadvertently omitted the word "widely" from a sentence and changed the meaning. The sentence (p. 554, column 3, line 47) should read: " . . . and bacteriophage DNA is not known to *widely* persist extrachromosomally inside bacterial cells."