LETTERS TO THE EDITOR

Editor: Commendations to you, Dr. Sturrock, and your Associate Editors for the tributes to Ian Stevenson, MD. I thoroughly enjoyed the articles and remembrances from those who wrote of their associations with Dr. Stevenson and his many contributions to so many disciplines.

I wish to provide a few comments to underline what others have written about Dr. Stevenson: a *scholar* and a *gentleman*.

During 1980–1981, he and I exchanged a half dozen letters, with reports and some audiotape recordings. I had referred a family to him, and he had referred a family to me, for the investigation of POLs (Possible Other Lifetimes). His intelligence, and experience, were on a higher level than mine; yet, he treated me as a peer.

I was busy as a faculty member and Director of Counseling and Testing, University of Wyoming. Also, I was attempting to establish, on campus, the Rocky Mountain UFO Conference (1980–2000). Yet, his schedule of activities, writing, and travel gave me a different perspective of stamina and commitment.

Although his writing indicated his skepticism about the use of hypnotic procedure to investigate ‘‘cases of reincarnation type’’, he encouraged me in my hypnosis sessions with a family and their possible earlier lifetimes together. Also, he tolerated my report of ‘‘psychological resonance’’ (channeling information about the possible family interactions). Although the channeled information may have added ‘‘flavor’’, it probably did not add ‘‘substance’’ to the previous information that he had gathered so skillfully.

I treasured the opportunity to exchange correspondence with Ian. I assume that he continues his spiritual path in service to Humanity & Creation.

May we all share more Love & Light.

R. Leo Sprinkle
Laramie, WY

Editor: While Bill Bergston’s survey of the membership is useful in canvassing the views of SSE members on ‘‘scientific anomalies’’, one aspect caught my attention, which I will use to make an important point, without any criticism of the survey and its worthwhile objectives. In the questionnaire, the scale of responses to ‘‘scientific anomalies’’ (phenomena such as UFOs, out of body experiences, etc.) was graded by the degree of skepticism the item engenders in the reader. I had submitted to *JSE* last year a paper on ''dowsing the vocabulary'', in which I reported on practical experiments with a method of dowsing that evinces subconscious responses to phenomena in divination mode. It gives quantitative values for our emotive and practical vocabulary, which span a wide range of vital energies when expressed on the Bovis Scale. I can already imagine a significant proportion of readers putting on their skeptical hat at the mention of ''dowsing'' and ''Bovis'', but without having tried the method themselves, I ask them to bear with me for a few more minutes. For example, when I hold in mind the emotion ''Skepticism'', I receive readings that are substantially below the levels registered for practical words, such as ''assess'', ''consider'', or ''evaluate'', and of course much lower than for emotional words, such as ''joy'', ''love'', ''family'', ''friendship'', etc. One conclusion is that at the time of Galileo and later, ''proto-scientists'' had to invent a vocabulary that falls within what I have called (Caddy 2007) ''the band of rationality'', excluding emotive words, in order that the logical consequences of initial assumptions and observations can be followed without the distraction of either ''doubt'' or ''inspiration''. From practical experience, I have noted that paranormal or ''anomalous phenomena'' are difficult to induce while negative emotions dominate the mind; perhaps because these create a ''morphic field'' such as those suggested by Rupert Sheldrake? My working hypothesis is that an observer should avoid skepticism when evaluating a ''scientific anomaly'', since the skeptical mind filters out phenomena that contradict its accepted axioms. It seems better to phrase questions alternatively, such as asking whether the evidence for the phenomenon in question is adequate, incomplete, or lacking. Even in the circumstances that the last response is chosen, this does not preclude an improvement in methodology or new evidence from easily reversing an initial opinion.

A Post script: My paper on dowsing the vocabulary was rejected for publication in the *JSE*; apparently on the grounds that dowsed responses are subjective. The referee/editor suggested that the paper would have been suitable for publication if I had been attached to an electroencephalogram, and if it had been written by the professional neurologist making observations from electronic instruments on his dowsing ''guinea pig''. I mention this to make the point that ''anomalies research'' is heuristic; often the only way to gain partial verification is to present a method for testing by a wider audience. At present however, the only way to read my paper is to contact me at jfcaddy@yahoo.co.uk, and I will send a copy.

John Caddy
Latina, Italy

Editor: Comment on Dieter Gernert, ‘‘How to Reject Any Manuscript,’’ *JSE*, Vol. 22, No. 2, 2008, p. 233–243.

There is very little that is really new in this interesting and provocative paper. It is well known that on rare occasions even Nobel class work has been rejected by one or more sets of referees. A fellow in Spain named Juan Miguel Campanario has written about this subject. He often refers to the *Citation Classic Commentaries* that were published in *Current Contents,* which demonstrated that on occasion these highly cited papers were rejected even by journals as respected as *Nature*. Wolfgang Glanzel and I published a paper in *The Scientist* about the myth of delayed recognition: Glanzel W. and Garfield E., ‘‘The Myth of Delayed Recognition–Citation analysis demonstrates that premature discovery, while rare, does occur: Nearly all significant research is normally cited soon after publication’’, *The Scientist* 18(11): 8–8 June 7, 2004. Original article in *The Scientist* <http://www.the-scientist.com/article/display/14757/>

Quite frankly when you consider the tens of millions of papers and books that have been published, it is surprising to me that it is so rare that such paradigm breaking papers are delayed or rejected. One wonders what Gernert would consider an acceptable level of rejection considering the huge volume of publication. Indeed many people would argue that rejection rates should be even higher. I am glad he agrees that peer review does serve a useful purpose if properly administered. I’ve had a lot of positive experiences with the system and a few bad ones. The worst two cases involved papers that were actually requested of me by the editors of the *New England Journal of Medicine* (*NEJM*) and *Science*, respectively.

In the case of *NEJM*, the then editor, who is justifiably a highly respected editor and scientist (Arnold Relman), after making me go through several revisions of my manuscript, refused to publish it because it would be ‘‘unseemly’’ for a paper published in *NEJM* to show how much higher *NEJM* ranked as compared with the other journals in the study. After delaying my paper for almost two years, he turned it down but within a few months it was accepted by Edward Huth, the editor of the *Annals of Internal Medicine*.

The second paper was requested by Daniel E. Koshland when he was editor of *Science*. It took me almost two years to write what I thought would be my magnum opus for *Science*, since I had published two core papers there in 1955 and 1964,1 which are both highly cited. By the time I sent in the ‘‘Synoptic history of the Science Citation Index’’ manuscript, Dan had retired from *Science*. His successor Floyd Bloom, a highly respected neuropharmacologist, refused to publish the manuscript after delaying it for six months or more. The extensive revisions he requested would have delayed the paper another year. Shortly thereafter, I was asked to speak in Copenhagen and my ‘‘talk’’ was published in an established European journal of library science. The full text is available under the title ‘‘From Citation Indexes to Informetrics: Is the tail wagging the dog?’’ *Libri*, 48(2), p. 67–80, June 1998. Based on oral presentation–Center for Informetric Studies, Royal School of Librarianship, Copenhagen, December 15, 1997.

<http://www.garfield.library.upenn.edu/papers/libriv48(2)p6780y1998.pdf/> The original title was ‘‘A Synoptic History of the Science Citation Index’’. That it has been cited only 26 times in ten years tells you something about the importance of where you publish. Had it appeared in *Science* or some other leading journal I have no doubt that it would have been more widely read and cited.

EUGENE GARFIELD

References

Garfield, E. ‘‘Citation Indexes for Science: A New Dimension in Documentation through Association of Ideas.’’ *Science*, 122(3159), p. 108–11, July 1955; and Garfield, E. ‘‘*Science Citation Index*–A New Dimension in Indexing.’’ *Science*, 144(3619) p. 649–54, 1964. See also: Garfield, E. ‘‘Citation Indexing for studying science,’’ *Nature*, 227 (5259) p. 669–671, 1970.