

advantageous marriage, Coues was practically a millionaire. Dr. Harrison, following my reminder that the Madras Christian College (the then owners) had placed restrictions on Hodgson's use of the letters, claims that the S.P.R. should not have made use of them, since they were not available to Mme. Blavatsky's defenders. This is unreasonable on two counts, firstly because incriminating portions of the letters had already been made public (9), and Hodgson could clearly not ignore them; secondly, Madame's defenders did have the opportunity of examining the letters, but only a few availed themselves of this (10).

I did not claim in my letter (11) that the peculiarities shared by Mme. Blavatsky's writings and the K. H. letters 'proved' her authorship of both. I merely pointed out that in what was represented as a critical review, the topic was not even mentioned.

Nowhere did I suggest that the early members of our Society were infallible: I was arguing against Brian Inglis' suggestion that they had accepted Hodgson's verdict without examining the evidence for themselves, since this would be at variance with what we know of their characters.

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5. As ref. (4) pp. 346-57.
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7. Williams, G. M. *Priestess of the Occult: Madame Blavatsky*, New York, Alfred A. Knopf, 1946, pp. 301-3.
8. As ref (6), p. 219.
9. Patterson, D. (Editor) *The Collapse of Koot Hoomi I & II*. Christian College Magazine, (Madras), 1884 September & October.
10. *The Collapse of Koot Hoomi*. Madras, Christian Literature Society, 1904, pp. 56-7.
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To the Editor,

Some eleven years ago the late Dr. Dingwall wrote to me as follows: 'An account of the Cambridge sittings [with Palladino] with full transcripts of the MS records together with an introduction would be *very interesting* (my ital.), but I don't know whether anybody would be willing to do it.' (1).

I had in fact already suggested it. Dingwall himself had briefly dealt with the subject, saying, 'What actually happened at Cambridge we shall never know. The full and detailed reports have not been published, but remain buried in the archives of the Society for Psychical Research.' (2).

A complete transcript etc. has been for several years in the Library of our Society where it could have been consulted by Mr. Coleman who, for reasons best known to himself, has chosen to ignore it. It is the kind of work that before the

escalation of printing costs might conceivably have been published as a Proceedings, but at a more realistic level one had to be content to compress the conclusions of years of hard labour in the form of a brief article in the *Journal*. It duly appeared in February 1983 (Vol. 52, No. 793), and I now merely refer those interested to my 'Palladino at Cambridge'. It does not altogether redound to the greater glory of the Founders, least of all of Hodgson.

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1. Letter of 5th April 1976.
2. Dingwall, E. J., *Very Peculiar People*. Rider n.d.

To the Editor,

I regret the need to put to rest the latest example of misinterpretations of what are now known as SORRAT phenomena. (These are quite extensive, varied and lengthy, and have commanded my own research attention for over a decade.) Dr. John Palmer, in his review has criticized Dr. Brian Inglis for not having cited in his *The Hidden Power* 'the evidence for fraud uncovered by Hansen and Broughton.' (p. 153).

Dr. Broughton himself responded to my criticism of a review of *The Paranormal* by D. S. Rogo with an opinion that there had been planned fraud, and with doubts that I was ever the victim of a mail thief (as I contended, on good evidence, was the case).

The actual details of Hansen and Broughton's experiences were, logically in my opinion, omitted from the book because they are not clear indications of fraud. The latter is simply a conceivable alternate explanation under the diverse circumstances that prevailed.

My chief complaint is therefore in their singling out this disquieting deviation despite their being aware of my numerous other unprecedented SORRAT documentations.

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To the Editor,

Monte Carlo methods are becoming more frequently used in parapsychology. John Palmer (Edge, Morris, Palmer, Rush, 1986) indicated that such methods 'may well be the wave of the future in evaluating psi data' (page 151). One of the first examples was presented by Michael Thalbourne in an exceptionally readable article 'A More Powerful Method of Evaluating Data From Free-Response Experiments' (*JSPR*, 50, 1979, pp. 84-107). Unfortunately, that paper contains a number of serious errors and misconceptions.

1. The description of the Randomization Test given on page 92 is incorrect; the method described counts combinations that cannot occur with the actual experimental procedure. If the described method were used it would result in a wrong probability value being estimated. [Also, the number of combinations of