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THE FRAUDULENT EXPERIMENTER:
PROFESSOR HANSEL'S CASE AGAINST
PSYCHICAL RESEARCH

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ESP: A SCIENTIFIC EVALUATION. By C. E. M. Hansel, with an introduction by Edwin G. Boring. Charles Scribner's Sons, New York, 1966. Pp. xxi + 263. \$6.95.

ESP: A SCIENTIFIC EVALUATION: BY C. E. M. HANSEL: A REPLY BY S. G. SOAL, Privately published, no date, pp. 11 (there is also a version in which pp. 8, 10 and 11 have been corrected by hand).

PROFESSOR HANSEL'S publishers refer to his 'special' study of extra-sensory perception, presumably with the implication that studies in this field, hitherto, have been merely casual affairs. They also describe his book as a 'fully developed objective study', and as an 'authoritative and exhaustive work'. One might easily infer both from the publisher's blurb and from Professor Boring's enthusiastic introduction that this book, which is intended to demolish the case for the reality of psychical phenomena, is unique among the multitudinous publications in (so the publishers remark) 'an area marked by misinformation, misguided enthusiasm, and prejudice'. In reality it is only the latest of an extensive 'anti-psychical' literature, going back to the beginnings of the subject. Early examples are J. N. Maskelyne's *Modern Spiritualism* (1876) and *The Supernatural?* (1891) by the same author in collaboration with Dr L. A. Weatherly. In the present century, some examples of varying merit are Professor Jastrow's *Fact and Fable in Psychology* (1901), Ivor Tuckett's *The Evidence for the*

Supernatural (1911), Edward Clodd's *The Question* (1917), Stuart Cumberland's *That Other World* (1918), Harry Houdini's *A Magician Among the Spirits* (1924), J. F. Rinn's *Sixty Years of Psychical Research* (1950, published in this country as *Searchlight on Psychical Research*) and D. H. Rawcliffe's *The Psychology of the Occult* (1952). Hansel refers to some of these authors, and in particular leans rather heavily on Rinn, with, as we shall see later, unfortunate results.

The theme of these works has generally been that parapsychologists were deceived or incompetent or both. In 1956 Dr Dingwall, Mrs Goldney and Mr Trevor Hall broke new ground with their publication of a lengthy and extensively documented argument ('The Haunting of Borley Rectory', *Proceedings S.P.R.* vol. 51, Jan. 1956) intended to demonstrate that a well-known psychical investigator, the late Harry Price, had systematically cheated during his last important investigation. Suggestions of this kind were not unknown before, but had never been presented with such elaboration and authority. Professor Hansel has now taken matters to what must surely be their ultimate by making detailed suggestions of fraudulent practice on the part of parapsychologists still living and, unlike Price, capable of taking appropriate action if they so wish.

This development has been greeted by some with indignation. Dr Soal, for example, in his pamphlet, remarks:

'About Hansel there is a queer, almost pathological, determination to eradicate ESP like an evil weed which will affect open-minded people unfavourably.' Earlier, Dr Gaither Pratt, commenting in his book *Parapsychology: an Insider's View of ESP* on Hansel's analyses of some of the work at Duke University, wrote of 'trails through the sloughs of unsound and unsavory criticism'.

Polemics of this kind, while perhaps natural from those under attack, seem to me bound to create a bad impression among the uncommitted. It has to be remembered that when reporting the more substantial ESP experiments it has been common practice for the experimenters to discuss, in an apparently unbiassed spirit, the possibility that fraud had been perpetrated even by themselves. For example, in the crucially important report by Dr Soal and Mrs Goldney on their experiments with Basil Shackleton (*S.P.R. Proceedings*, vol. 47, December 1943) the authors remark:

'We gave much thought and discussion throughout to the question of rendering the conditions in which these experiments took place proof, so far as was humanly possible, against even the possibility of fraud, on the part of Percipient *and experimenters*

alike' (italics mine). Similar assertions have been made in connection with the crucial Duke University experiments.

Such claims clearly open the door to the critic who wishes, if he can, to establish fraud by the experimenters. Subsequent complaints as to the unsavoury character and the pathological motivation of such criticism can only throw doubt on the sincerity of the claims, at the time, that experimenter-fraud had been seriously considered.

I feel no doubt, in view of the basically fantastic nature of the phenomena (as Hansel remarks, of a nature 'to shake the very foundations of science') that it is legitimate for the critic to explore the possibilities of incompetence or fraud by all concerned. Some distinguished parapsychologists have not shrunk from this course in the past. Dr Soal, for example, is quite prepared to envisage trickery by Professor Gilbert Murray during the latter's well-known experiments in thought reading (Soal¹ remarks: 'Yet, apparently, no one even accompanied Professor Murray outside the room to see that he "played fair"'). Again, Dr Soal has never hesitated to make public his suspicions of much of the work carried out at Duke University. J. B. Rhine² has described Dr Soal as 'over the years . . . one of the most harshly unfavourable (and, to my mind, unfair) among the critics of the researches with which I have been associated' (see also comments by Dr R. A. McConnell, *Journal S.P.R.*, 34, June-July 1948, p. 242). Writing, for example, of the Pratt-Pearce experiment Dr Soal remarked:³

It is . . . regrettable that Drs Rhine and Pratt, with the knowledge that they were getting epoch-making results under conditions beyond criticism, did not take every possible precaution to guard themselves against any suspicion of collusion. Under such circumstances my own procedure would have been as follows; I should have arranged for the head of another department of the University to be present in the Physics or Medical Buildings to witness and actually perform the shuffling of the pack and the recording of the actual card sequences. *I should have instructed him to take every precaution that I did not substitute a 'prepared' pack for the one which he had himself shuffled. . . .* (the italics are Dr Soal's).

It is worthy of note that in directing attention to the possibility of fraud by Rhine and Pratt Dr Soal goes rather further than Hansel, who is content in the main to suggest fraud only by the subject, Pearce.

Of course, to defend the propriety of critical examination of all

¹ Harry Price: *Fifty Years of Psychical Research* 1939, 171.

² S. G. Soal and H. T. Bowden: *The Mind Readers*, 1959, p. 284.

³ *Proceedings S.P.R.*, vol. 45, 1938, p. 95.

aspects of ESP experimentation, including possible experimenter fraud, is not to condone mere abuse. By and large, Hansel's book is free of this, though its appearance has touched off some deplorable lapses of taste. A reviewer, for example, in the American journal *Science* refers to the medium Eusapia Palladino as a 'clever biddy', though I should be surprised to find that he knows any more about this lady than he has picked up from his reading of Hansel's book. On the other side, an S.P.R. member took the unusual liberty, in the correspondence columns of *The Listener*, of applying the epithet 'pathetic' to Professor Hansel. I imagine that this same member, had he been writing in the same strain thirty years ago, might have referred to 'the pathetic Mr Soal', with as little justification as good manners.

Over the last six or seven years Professor Hansel has published a series of communications in *Nature*, the *New Scientist*, *Proceedings S.P.R.* and the *Journal of Parapsychology*, intended to discredit four of what one might call the fundamental experiments in ESP research, namely the Pratt-Pearce series, the Pratt-Woodruff experiment and Dr Soal's tests first with Basil Shackleton and then with Mrs Gloria Stewart. Three of the chapters of the present book consist of an elaboration (and on some points a partial retraction) of his earlier analyses. These are in fact the only sections of real interest. It is a pity that the scope of the book was not restricted to topics on which Hansel had previously written and, presumably, where his chief interest lies. But at some stage it was evidently decided to make it a 'fully developed' study, as Professor Boring rather quaintly puts it, of 'the whole history . . . of psychic phenomena', and the consequence has been that the book is padded out with superficial and often inaccurate material largely derived from previous 'anti-psychical' publications.

Professor Hansel's attitude is summed up in the following quotation:

In parapsychology research, the process being investigated is both hypothetical and a priori extremely unlikely. Any possible known cause of the result is far more likely to be responsible for it than the hypothetical process under consideration. . . . It is necessary to discuss openly possible trickery or cheating by participants to produce a spurious conclusion. If the result could have arisen through a trick, the experiment must be considered unsatisfactory proof of ESP, whether or not it is finally decided that such a trick was in fact used. As a further step it may be necessary to establish whether there is any evidence to show that trickery did in fact take place.

This seems to me to be a fair principle, dispassionately stated.

If Hansel had carried out an equally dispassionate analysis of the outstanding ESP experiments on the lines thus laid down he would have put parapsychologists in his debt. Instead, when he comes down to cases, there is a sad deterioration. An uncomfortable proportion of his arguments take the form of innuendo, loaded phrases or partial reporting. This is particularly so in his treatment of the Shackleton and Stewart experiments.

The main facts of the Shackleton experiments are familiar to all serious students of psychical research. As is well known, both Shackleton and Mrs Stewart were discovered, as high-scoring percipients, in 1939 when Soal re-examined for pre-cognitive hits the score sheets, previously thought to show nothing but chance results, of 160 persons, tested some years before. In Hansel's narration of this remarkable event his unfortunate partiality to the loaded phrase immediately makes itself felt. Instead of an uncoloured account, one finds that Soal 'claimed' that Shackleton had displayed a high-scoring pattern of pre- and post-cognitive hits, and later Hansel talks of 'the high displacement scores that Soal *alleges* he found' (italics mine). The implication appears to be that Soal faked the score sheets (presumably in 1939) or in some other way misrepresented the situation. But is it fair and honest criticism to insinuate such a suspicion into the mind of the reader while making no attempt whatsoever to discuss the evidence? Photostats of the original score sheets still exist in the S.P.R. files and are open to inspection by bona fide students. They are unfortunately not of high quality and in places are written over. But the more important portions are legible, including in particular the signatures of witnesses. If Hansel has reasons for doubting the authenticity of these records surely he should be prepared to make them known.

In an earlier critical analysis of the Shackleton experiments, published in the S.P.R. *Proceedings* for May 1960, Hansel attempted to explain the significant results in terms of a trick operated by the percipient in collusion with the agents, the experimenters not necessarily being implicated. The suggested trick involved a substitution method, operated by the agent. In reply, in the same *Proceedings*, Soal was able to show that such a trick would be quite inadequate to account for the scoring level observed. Hansel acknowledges this in the present book and in fact, in the chapter on the Soal-Goldney work there are no new suggestions to be found. Instead, he returns to an article by Dr G. R. Price, published in the journal *Science* in 1955 and described by Hansel as 'a brilliant analysis'—perhaps because it is opposed to the ESP hypothesis! Price suggested a number of

ways in which the results could have been obtained by fraud, almost all of them implicating Soal. Hansel considers the successive sessions in the light of Price's suggestions, and decides that one or another of the postulated tricks could have been operated in all sessions. However, it is important to notice that, owing to changes in conditions, no one method of fraud could have been used throughout the series: it would have been necessary to make quite radical changes in technique from session to session, sometimes on the spur of the moment. Because of the variety of tricks that must be postulated, no one, or even two, persons could have fraudulently produced all the observed results. As Hansel remarks, Soal (who is an essential ingredient in any plausible fraud hypothesis) would have required 'the assistance of 3 other persons—Basil Shackleton, Rita Elliott and J. Aldred—to have faked the result of the Soal-Goldney experiment'.

Hansel evidently feels, with some justice, that a bare assertion of the possibility of fraud would be insufficient to carry conviction to whatever public he envisaged for his book. He consequently presents what purports to be confirmatory evidence that fraud did in fact take place. It is interesting that on each occasion when he does so he either falls into error or misrepresents or suppresses material facts—it is not always clear which.

The first of these passages in Hansel's book occurs on p. 118, in his discussion of sessions in which counters were drawn from a bowl to determine the target sequences. His preferred trick, in this situation, would have been difficult to implement if an observer had been watching the agent. Hansel points out that at six sessions when this technique was used the only observer present watched Shackleton and not the agent, and high scores were obtained throughout. At the seventh such session (session 17) Professor H. H. Price, as observer, watched Shackleton for three runs, during which high scores were obtained, then for the next four runs he watched the agent, and during these runs the scores were at chance level. Hansel comments: 'Thus, following 6 sittings at which high above-chance scores were consistently obtained when counters decided the targets, high scores were still obtained for the first 4 [Hansel means 3] runs at the seventh such sitting. Then, as soon as an observer watched Soal recording the targets, scores dropped to the chance level'. Put in this way, the situation looks sinister, as Hansel intends it should. However, there is a ready and plausible alternative explanation for the change in scoring pattern. While the first three runs of the seventeenth session were carried out at the normal operating rate, the next four were at the 'slow rate', i.e., at half the normal. Hansel does

indeed make a passing reference to this earlier, but he fails to explain that at the slow rate Shackleton *never* scored significantly high. I feel also that the reader might have attached less importance to this particular item of Hansel's evidence if he had added that after the four runs during which Price watched the agent and no significant score was obtained, Price went back to watching Shackleton for the rest of the session and still no high scores resulted. If a trick involving the agent had been operated at the beginning of the session it could equally well have been used during the concluding six runs when the conditions were the same again.

Hansel's second piece of evidence for fraud, mentioned on p. 120, concerns session 28. Hansel considers that during this session 'conditions were far more stringent than at any other', in such a way as to make a trick difficult if auditory clues were not available. However, he asserts that 'at this sitting, the only one at which cheating was impossible except by using the substitution method, the conditions were changed so that the agent heard Shackleton's calls'. This again has a sinister ring, but if it were not true that 'conditions were changed' at this session then Hansel's point would be blunted. In fact, it is not true. Hansel's evidence is based in part on an unfortunate confusion in the original Soal-Goldney report which, however, Hansel could easily have sorted out. It should be explained that the authors intended the report to include a comprehensive 'Chronicle' giving a detailed account of the experimental conditions at each session. Owing to space restrictions this Chronicle had to be omitted, though accounts of three representative sessions were reproduced from it in the report. The complete Chronicle was available in duplicated form from the S.P.R. offices, and in due course Hansel applied for and was supplied with a copy.

Hansel quotes the passage about a change in conditions from the version of part of the Chronicle reproduced in the printed report. He does comment, in a rather puzzled way, that the Chronicle for session 28 in the complete duplicated version is different, in that it omits this passage. If he had pursued his researches he would have found that the change of conditions occurred not during session 28 but four months earlier during session 19, and it was not a change such that the agent could hear Shackleton's calls while not having done so before, but rather a change in the method of recording guesses. The confusion (though, as I remarked earlier, it is easily resolved) has arisen because the authors of the report, in trying to make self-contained the account of session 28, which in the full version of the Chronicle refers back for some details to session 19, have rather carelessly

included the passage about a change in conditions which was actually made during the earlier session.

Hansel's third item of evidence concerns the sessions involving Gretel Albert, one of the three successful agents. I cannot help regarding his handling of this episode as rather shabby. With no discussion or comment he says quite baldly that Mrs Albert 'stated after one sitting that when glancing through the hole in the screen, she had seen Soal, while acting as EA [i.e. the experimenter associated with the agent], altering figures on the score sheets' Hansel made a similar statement on television, to an audience on the whole even less likely to check the full facts than readers of his book. Put in this unqualified way the statement, as I am sure Hansel appreciates very well, is not only extremely damaging to Soal but grossly unfair. If he had revealed to his readers and his television audience that this lady also asserted that she had smoked one of Basil Shackleton's cigarettes and found it to be drugged, though in fact many other people smoked Shackleton's cigarettes at this time and suffered no ill effects, it is certain that a different weight would have been put on the value of her testimony.

Hansel's final item of evidence for fraud in the Shackleton experiment is one that he published some years ago in *Nature* (vol. 184, p. 1515, 7th Nov., 1959). This concerns the +2 pre-cognitive hits during the 'rapid-rate' trials. Hansel divided each run of guesses into successive blocks of five (the final block will only contain three) and considered the distribution of totalled hits in corresponding positions in all such blocks. He was able to show that with moderate significance the distribution was non-uniform, and he presented this as evidence of the use of a substitution code, operated by the agent (it should be noted that there is no such non-uniformity in the results of the far more numerous 'normal-rate' trials). It is not clear why he reproduces this in his book, since he seems to have largely abandoned the hypothesis of a substitution code.

Soal, in his pamphlet, has re-evaluated the probability and finds a p value of 0.02, instead of Hansel's 0.01. His explanation of this discrepancy seems not to be correct: the difference appears to arise because Hansel, in evaluating his chi-square, has included the contribution from the misses as well as the hits, whereas Soal has only considered that from the hits. I believe Hansel's method to be statistically correct, though the difference, in the present context, is not of great importance. The same criticism would apply to the chi-square values given in Appendix D of the Soal-Goldney report.

Hansel seems hardly at all inclined to consider the *likelihood* of fraud under the conditions of these experiments, though at one point, possibly in an unguarded moment, he does remark that 'it is difficult to believe that the percipient and agent would go to the effort of memorizing long lists of symbols and their positions on the score sheets for week after week'. It is possible that if he had known the percipient and the agents he might have used stronger words. I find the greatest difficulty in believing in Hansel's plot, operated by Soal, Basil Shackleton, Rita Elliott and Jack Aldred in collusion. In fact, Hansel seems to me to have made a positive contribution towards renewing confidence in this experiment in his clear demonstration that no *one* person could have engineered a fraud. None of Soal's 'accomplices' stood to gain from this work (Shackleton in particular got no special recognition at the time; he is not even named in the Soal-Goldney report). They were virtual strangers to each other and none of them has even hinted at a guilty secret during the subsequent quarter of a century. As mentioned earlier, Hansel has been unable to show that any one method of fraud could have been operated throughout the series: he is forced to postulate the use of a number of quite complex techniques, most of them requiring coaching and practice. It is far from clear when the necessary meetings and rehearsals took place, bearing in mind that Soal was lecturing in Cambridge throughout the week, and making special journeys to London for the experiments under the difficult war-time conditions. Shackleton has never ceased to declare his belief in his powers. He was clearly disappointed when, during further experiments carried out by Mrs Goldney and others a few years ago, he failed to achieve significantly high scores—surely an odd attitude in one who, on Hansel's hypothesis, knew that all his original results were achieved by fraud. Finally, if I may interject a remark based on personal experience, Soal's manner during these experiments was totally unlike that of a trickster. During the run of Aldred sessions that I attended, in which I was in sole charge of the score-sheets, I have a clear memory of Soal's behaviour when we had the first successful rapid-rate +2 scoring with this agent. Normally a withdrawn and unexpressive person, he was on this occasion alight with excitement and pleasure, like a man in the presence of a wonder rather than like a successful rogue.

Of course, if Hansel were to ask whether such impressions constitute 'proof' of ESP it would be absurd to give a positive answer. But my own feeling is that Hansel, throughout his book, is not asking quite the right question. His constant preoccupation is with whether ESP is 'proved', and his reply is always in the

negative. I would be inclined not to disagree. But a more practically useful question would be 'Is there a case for continuing to investigate ESP?' and on the evidence of the Shackleton experiments alone the answer seems clear.

I shall pass over Hansel's chapter on the Jones boys, since the possibility, not realized at the time, of the use of an ultrasonic whistle seems to diminish severely the evidential value of these experiments.¹ However, in a 'scientific evaluation' (whatever precisely this may be) I should have expected some discussion of the arguments in Soal's paper 'The Jones Boys: The Case Against Cheating' (*S.P.R. Journal*, vol. 40, June 1960, p. 291), which Hansel fails to mention.

Hansel's discussion of the Pearce-Pratt experiment, carried out in 1933-4, revolves around the hypothesis that Pearce, a divinity student, gained surreptitious knowledge of the target cards by peeping through the window of Pratt's office, or through a hole in the ceiling when another room was used.² Dr Ian Stevenson has considered the former suggestion in detail in his review of Hansel's book (*Jour. Am. S.P.R.*, vol. 61, July 1967, p. 254) and decides that the operation was not physically impossible, though he regards it as unlikely to have taken place. He admits the validity of Hansel's criticism of the grossly unsatisfactory method of reporting this experiment, claimed by Pratt and his associates, in their book 'Extrasensory Perception after Sixty Years', to be one of the six best in the period before 1940. It seems to me, however, that in his anxiety to score all possible points Hansel over-reaches himself. He comments rather scathingly on the absence of a statement by Pearce himself, though if Pearce had cheated, as Hansel supposes, it is hard to see what would be the value to Hansel of any denial he might now make. Stevenson, following Hansel's suggestion, has obtained such a denial. Hansel will now doubtless tell us whether this changes his view.

Both Stevenson, in his review, and Charles Honorton, in a review in *The Journal of Parapsychology*, brush aside Hansel's criticism of the Pratt-Woodruff experiment (Stevenson, in a comment on this and other Hansel criticisms, mentions 'implausible, almost impossible speculations about how the subjects or the investigators could have cheated'). This, in my opinion, is the one experiment for which Hansel has not only demonstrated a

¹ See, in particular, 'The Jones Boys and the Ultrasonic Whistle', by Christopher Scott and K. M. Goldney, *S.P.R. Journal*, vol. 40, March 1960, p. 249.

² Compare the method of fraud suggested by Dr. Soal and mentioned earlier.

practicable and consistent method of fraud, but has also produced internal evidence that needs serious consideration. I shall not enlarge on this here, since there is a likelihood that the matter will be considered at length elsewhere.

Hansel's chapter on psychokinesis would hardly be helpful to a newcomer looking for a guide to, and an assessment of a controversial topic. For example, he quotes from *Psychic News* a criticism by C. C. L. Gregory (whom he mistakenly believes to have been a Professor of Astronomy) of Haakon Forwald's attempt to calculate the magnitude of the 'psychic force', but he makes no attempt to discuss Forwald's very striking placement experiments. He quotes Fraser Nicol's criticism of the lack of witnesses of much of Forwald's work, but makes no reference to the witnessed runs at Duke University (J. G. Pratt and H. Forwald: 'Confirmation of the PK Placement Effect', *J. Parapsychology*, vol. 22, March 1958, p. 1: see also the important paper 'Psychokinetic Placement: I. A Re-examination of the Forwald-Durham Experiment', by R. A. McConnell & H. Forwald, *J. Parapsychology*, vol. 31, March 1967, p. 51). Neither does he mention the remarkable dice-throwing experiments conducted by G. W. Fisk in collaboration with A. M. J. Mitchell and D. J. West (*S.P.R. Journal*, vol. 37, March-April 1953, p. 45, and vol. 39, September 1958, p. 277). He complains of what he claims to be the total lack of evidence of directly observable psychokinetic effects while failing to mention, for example, Professor Winther's remarkable experiments with the medium Anna Rasmussen. Perhaps Hansel regards this work as unworthy of notice, or perhaps he is unaware of it. One has no way of knowing which alternative is correct.

What might be called the supplementary chapters, that is, those additional to the chapters concerned with the major ESP experiments, contain many assertions and pronouncements, often thrown out almost in a casual way, which would deserve comment if space permitted. For example, on page 30 one finds: '... Smith acted as Gurney's secretary, assisting him until his suicide in 1888...'. The uninitiated reader would certainly fail to realise that the description of Gurney's death as an act of suicide is merely a conjecture by Mr Trevor Hall, not universally accepted.

Hansel's strictures on the early experiments at Duke University tend to echo those of Dr Soal. His comment, for example, that 'psychologists have not easily forgotten Rhine's telepathic horse' (p. 182) was anticipated by Soal when he remarked on the 'unfortunate effect... upon British psychologists and men of science' of stories such as that of 'the telepathic horse which

pranced in the pages of *Extra-Sensory Perception*' (*S.P.R. Journal*, vol. 34, February 1948, p. 184). Hansel's brief discussion of the Turner-Ownbey experiment, conducted from Duke University in 1933, seems to be based on a misreading of the account in Rhine's *Extra-Sensory Perception* (Boston Society for Psychic Research, March 1934). This was a long-distance telepathy experiment, during which Miss Ownbey, one of Rhine's graduate assistants, exposed 25 target cards per day at Duke, while Miss Turner, an undergraduate, recorded her guesses 250 miles away. Miss Turner was intended to post her guesses directly to Rhine, but apparently by error she sent the records for the first three days to Miss Ownbey, who then passed them to Rhine. Hansel asserts that 'if Miss Ownbey had wished to deceive Rhine, she would merely have written out her record of the target series after seeing Miss Turner's guesses' (p. 55). If one accepts Rhine's account this would appear not to have been possible since Miss Ownbey had turned her target card record over to Rhine, evidently after each run, and Rhine remarks that 'I already had [this record] on my desk' when Miss Turner's letters were brought to him (*Extra-Sensory Perception*, U.S. edition, p. 105). However, as Rhine himself admits, fraud by the two young ladies in collusion would have been perfectly possible. One must agree with Hansel in finding it disconcerting that, while the staggering scores of 19, 16 and 16 were recorded during these first three runs (when the error in procedure occurred), in subsequent runs under the same experimental conditions the score dropped uniformly to a non-significant level. However, if the girls had been in collusion (which, if Rhine's account is accurate, would seem to be requisite for fraud) it is not obvious why they should not have continued to produce these high scores. Be that as it may, it is certainly, and most unfortunately, true that Rhine's report of this momentous occasion is woefully inadequate.

Hansel's investigation of a report concerning Gerard Croiset, the Dutch clairvoyant, is to be commended. This report appeared in an article by Jack Harrison Pollack, in the magazine *This Week*, in February 1961, and concerned the alleged tracing of a murderer by Croiset, in collaboration with the local police. A number of highly impressive evidential points were given in Pollack's account, but in correspondence with the local burgomaster Hansel discovered that on all matters of substance the story had no real foundation in fact, though Pollack had claimed to have checked the case in the Dutch police files. Dr Ian Stevenson, in his review of Hansel's book, takes Hansel to task for 'directing his attack at a report . . . in a Sunday newspaper supplement by a journalist',

and asks 'why should parapsychologists be blamed for the errors of newspaper writers?' However (and I am surprised that Hansel fails to mention this) Pollack has subsequently produced a book on Croiset (*The Amazing Story of Croiset the Clairvoyant*, British edition published by W. H. Allen, 1965), the manuscript of which is said to have been 'indefatigably double-checked' by Professor Tenhaeff, Croiset's chief academic sponser, so Mr Pollack's handling of evidence has presumably become a legitimate subject for comment. The story whose magazine version was attacked by Hansel has been very considerably toned down in the book, the apparently spurious 'evidential' material being omitted, though no mention is made either of the earlier, more impressive version or of Hansel's criticism of it, which had been submitted to the magazine. One would like to see the many other cases given in Pollack's book subjected to similar independent checks.

Hansel's treatment of the interesting and important 'sheep and goats' type of experiment is so perfunctory that it might better have been omitted. Dr Gertrude Schmeidler's long series of tests is dismissed in one eight-line paragraph, with a reference to one only, and not the most important, of her papers. I find it astonishing that he fails, here, to refer to the book *Extrasensory Perception and Personality Patterns*, by Dr Schmeidler and Dr R. A. McConnell (Yale University Press, 1958), which contains an excellent exposition of Dr Schmeidler's work up to that date. Hansel is aware of this book, since he refers to it elsewhere in a different context. It is hard to avoid the impression that Hansel wishes to minimise Dr Schmeidler's work by making it seem that it rests only on a single four-page paper. Hansel mentions the paper¹ on a related form of experiment by J. Fraser Nicol and Dr Betty Humphrey (whom he demotes to the status of 'Miss' Humphrey) and remarks that the 'scores . . . tended in the same direction but were not statistically significant'. This depends of course on one's criteria of significance and would be regarded by many as untrue (readers of this Journal will find some of the relevant figures from the Nicol-Humphrey paper in Dr Soal's review, May 1954, p. 307). In a section on 'Precautions Necessary in Group Experiments' Hansel suggests that matters should be arranged so that 'there can . . . be no possibility that the original

¹ 'The Exploration of ESP and Human Personality', *J. Am. S.P.R.*, vol. 47, p. 133, October 1953. Mr Nicol and Dr Humphrey later carried out a further experiment of the same kind, with a totally non-significant result. Hansel's assertion that this has never been published is incorrect: it is described in a paper 'The Repeatability Problem in ESP-Personality Research', *J. Am. S.P.R.*, vol. 49, p. 125, October 1955.

classification will be changed after the scores of the ESP test becomes known'. Perhaps the reader is meant to infer that this method of cheating was used by Dr Schmeidler and others who have claimed success with this technique. If not, there seems little purpose in Hansel's mention of it. But if this is what he has in mind I would have thought it obligatory, in the course of 'a scientific evaluation', for him not only to say so but to establish the possibility by reference to the experimental conditions. In the case of Dr Schmeidler's experiments, the checks reported by Dr McConnell (see Appendix B of the book mentioned above) seem to make such a suggestion difficult to sustain.

The superficiality of much of Hansel's book is nowhere clearer than in his chapters on 'Spiritualism' and 'Mental Mediums'. These are subjects which have attracted a great deal of attention, much of it of a careful and critical nature, by men and women of not insignificant mental calibre. It seems to me almost an insult to his readers that Hansel, having apparently read a few books, some of dubious reliability, should feel competent to dash off what Professor Boring calls 'the whole history of . . . psychic phenomena'. Would-be psychical historians really should realise that the matter is not so easy.

I shall take from these chapters just three examples of Hansel's method. In discussing the communications ostensibly from Raymond Lodge, Sir Oliver Lodge's son killed in the first world war, Hansel asserts that Raymond's 'identification was "proved" at the first sittings' by a description of him given through Mrs Leonard, and he proceeds to pour scorn on such 'proof'. In reality, Lodge never made the silly error of accepting the description as 'proof': he merely remarked that 'a youth was described in terms which distinctly suggested Raymond' (*Raymond: or Life and Death*, 1916, p. 98). At the end of this particular chapter Lodge wrote 'I now propose to make some further extracts—of a more evidential character—tending to establish the survival of my son's own personality and memory', which clearly indicates the lack of weight attached by Lodge to the description. Also in connection with Raymond, Hansel tells us that '. . . it must be quite exceptional for a British officer to avoid regimental and mess photographs. But Sir Oliver was very impressed when "Raymond" mentioned such a portrait, which only subsequently turned up at the Lodge's home'. This is a parody of the true situation. Lodge was not simple-minded, though Hansel would like us to believe so. What interested and impressed Lodge was the coincidence of *detail* in the photograph and in the description given through the medium. Hansel may consider this coincidence

unimpressive, but if so he conceals his opinion and substitutes a mere jibe.

Hansel's treatment of Eusapia Palladino is as selective as one would by now expect. He makes a great deal of the hostile criticisms published during her American tour; but surely in a 'scientific evaluation' he should reveal the existence of the detailed and documented defence by Hereward Carrington in his *The American Seances with Eusapia Palladino* (Garrett Publications, 1954), even if after discussion he were to reject this defence. Hansel cites the adverse report of the Columbia University committee, but he predictably fails to mention the remarkable subsequent letter by Professor R. W. Wood (Carrington, op. cit., p. 243). This reads, in part,

... Although I signed the statement of the investigating committee that no conclusive evidence was obtained of the supernormal powers of Palladino, I cannot accept the so-called 'exposure' of her methods of raising the table published in *Collier's*. I have myself been under the table, in a brightly lighted room, during its levitation to a height of fully two feet, and passed my hand between the legs of the table and the skirts of Madame Palladino. At one sitting (not an official one) I held the right hand of the medium during some very remarkable levitations. The room was brightly lighted, and both legs of the table were easily seen to be clear of the medium's feet and knees. . . . In addition I passed my hand under the feet of the table nearest the medium. If it is a trick it is a much cleverer one than that described by Professor Jastrow.'

In a review in the *S.P.R. Journal* (Nov.-Dec., 1954) Professor F. J. M. Stratton remarked: 'Such a statement from so keenly critical and able an observer as Professor R. W. Wood must give all critics pause.' But it evidently does not give Professor Hansel pause.

Hansel's treatment of Mrs Piper's mediumship is almost ludicrous. He passes over the enormous volume of evidence and critical discussion in the *S.P.R. Proceedings* and elsewhere, and has the misfortune to draw on the notoriously inaccurate book by J. F. Rinn for his critical material. In two paragraphs he makes three factual statements, all of them wrong or misleading. He asserts, when discussing the 'G.P.' phase of her mediumship, that George Pellew's mother refused an invitation from Richard Hodgson to join the American Society for Psychical Research (then a branch of the English Society) because she thought the 'G.P.' communications 'utter drivel and inanity', whereas a glance at the membership list published in each issue of the *S.P.R. Proceedings* would have shown him that Mrs Pellew was an

Associate Member from 1892 to 1904, throughout and well beyond the period of these communications. Hansel's remark that 'the famous tin box was in fact empty' is evidently made in disparagement of the 'communicator', the medium or the investigators, but seems to me quite meaningless. No special claims were ever made about the tin box episode, described fully and unambiguously in Hodgson's report (*S.P.R. Proceedings*, vol. 13, 1898, pp. 302-3), which I can only conclude Hansel has not read. It is curious that Hansel compounds error by failing even to transcribe accurately the material he takes from Rinn. He attributes to Pellew's brother an account of a visit to Mrs Piper which is clearly stated in Rinn's book to have been given by a Mr John Fiske (Fiske's sittings, incidentally, are described at length and apparently quite fairly by Hodgson in one of his Piper reports—*S.P.R. Proceedings*, vol. 13, p. 428 etc.—though Fiske's name is concealed under a pseudonym). I am indebted to Dr A. O. Gauld for this information about the Piper sittings.

In conclusion, I should like to return to Hansel's treatment of Soal, and to put a point that is easily overlooked. Hansel's thesis involves the hypothesis that Soal could have been a trickster and a rogue. But suppose, as I believe, that no trickery occurred and that Soal made a contribution of major importance to scientific knowledge. Is it not tragic that in his old age Soal should find himself one of the major targets of a rather brash, sometimes superficial, often mis-informed but widely publicised and applauded book?

TWO SYNCHRONOUS EXPERIENCES CONNECTED WITH A DEATH

by G. W. LAMBERT

IN the following narratives, actual names and addresses are withheld; initials are altered; dates are altered as to month (not day of month); and years are actual. The case, as a whole, is of peculiar interest, as it combines features which are usually found independently of one another, and each of the two experiences was described by the percipient to someone else before the close coincidence of them in time was discovered.

The writer of this report heard of the case through a mutual friend and invited the statements, which were furnished in answer