

ceilings have thirteen panels, and most of the forty staircases have thirteen steps. Sarah would hold banquets in the central seance room for herself and twelve spirit guests. In the last years of her life she left the house only once and received no visitors.

Others of the cases have a more serious interest, the most noteworthy being perhaps the recent Miami poltergeist, of which Miss Smith had some personal experience. This case centred on a young man who worked in a Miami warehouse. Though sceptical at first, Miss Smith and other observers could find no normal explanation of why bear rugs, ashtrays and heavy crates should fly off storage shelves on to the floor.

Miss Smith does not draw any conclusions as to the nature of these phenomena, presenting only the evidence for them; but, as she says in her preface, they happen to a great many people in a great number of places.

KATHRYN SHERIDAN JONES

PUBLICATIONS ALSO RECEIVED

PARAPSYCHOLOGY—THE WORLD BEYOND OUR FIVE SENSES.

By Dr C. H. Berendt. The Dani Library of Popular Science, Reuben Mass, Jerusalem, 1966.

This book, a general survey of the subject, is the first work on parapsychology to be published in the Hebrew language.

THE JOURNAL OF PARAPHYSICS No. 6. The Paraphysical Laboratory, Downton, Wilts, 1967. 5s.

This issue contains a 'Classified Directory of Spontaneous Phenomena 1965-7'.

PARALLEL PATHS TO THE UNSEEN WORLDS. By Felix J. Frazer. Builders of the Adytum Los Angeles, 1967. 381 pp.

A survey of psychical research, with emphasis upon the evidence for survival and for physical phenomena, by a former Federal law enforcement officer.

CORRESPONDENCE

The Fraudulent Experimenter

SIR,—I found Mr Medhurst's critique of Professor Hansel's *ESP, a Scientific Evaluation* puzzling.

Mr Medhurst vigorously defends Hansel's right to publish

hypotheses involving fraud by experimenters and others. He agrees with Hansel's basic methodological position: '... a fair principle, dispassionately stated'. He apparently shares Hansel's scepticism about the Jones boys, and accepts his criticism of the Pratt-Woodruff experiment. On the Pratt-Pearce experiment, though he has no comment on Hansel's two main counter-hypotheses nor on his severe criticism of the confused reporting of this series, he quotes another critic who agrees about the latter and apparently does not entirely reject the former. Mr Medhurst seems to accept the essentials of Hansel's strictures on the early Duke work and commends Hansel's investigations of Croiset. Even on the Soal-Goldney experiment he accepts the physical possibility of all Hansel's hypotheses, objecting to them only on psychological grounds, which he admits to being quite inconclusive. Finally, he is 'inclined not to disagree' with Hansel's central contention that ESP is unproved.

In other words, Mr Medhurst basically supports Professor Hansel on all important points. Yet, inexplicably, he chooses to give exactly the opposite impression by filling out his review with a succession of quite trivial objections to details of Hansel's exposition.

Thus, Mr Medhurst considers it unfair that Hansel should use the expression 'alleged' or 'claimed' about other people's statements which he has not investigated: apparently to say that X alleged or claimed something is to imply faking or misrepresentation by X.

Later, Hansel is criticized for referring to a book but not referring to it in the right place, for saying that a researcher who died 80 years ago committed suicide when in fact the cause of his death is uncertain, and for saying that an alleged communicator's mother refused to join the American S.P.R. when in fact she was an Associate Member (though the point of the story, namely the *reason* she is said to have given for her refusal, is not questioned by Mr Medhurst).

There are many more criticisms of this order. Some are fair but trivial, some are not even fair. Of the former, perhaps the least trivial concerns Hansel's treatment of Lodge: certainly Hansel does Lodge an injustice, but even this has only a very minor bearing on the reality of ESP, which is the subject of the book. The most unfair, to my mind, is where Mr Medhurst objects to Hansel's 'shabby' treatment of Mrs G. Albert's allegation that she saw Soal 'altering the figures'. Mr Medhurst feels that Hansel was unfair because he omitted to add that Mrs Albert also asserted that she had smoked one of Shackleton's cigarettes and found it

drugged. Leaving aside some difference of opinion as to whether she really *asserted* this or merely advanced it as a speculation, what is this supposed to prove? Apparently that Mrs Albert was a hysterically unreliable witness! But perhaps the cigarette *was* drugged, or perhaps it was an 'asthma cigarette', or perhaps she felt dizzy for some purely internal reason and jumped to an unjustified conclusion. Just how far does this discredit her as a witness? And if every argument we do *not* mention reveals our prejudice (which I do not accept) then I could argue that Mr Medhurst shows his prejudice and Professor Hansel his open-mindedness when neither of them mentions another fact: that Mrs Albert's allegation was suppressed for 17 years until Mr Fraser Nicol and myself brought pressure on the authors concerned to publish it.

Mr Medhurst makes four criticisms of Hansel's treatment of the Soal-Goldney experiment which he considers to involve error, misrepresentation or suppression. The third of these has just been mentioned. The first two are complex and would require a lot of space to deal with thoroughly. Briefly, they relate to a passage whose essential purpose seems to be to describe the various conditions in different sittings so that the possible methods of cheating can be pin-pointed. Mr Medhurst does not comment on this aspect of the discussion. However, at the two points concerned Hansel makes, in passing, the implication that an observed change of scoring, following the change of conditions, would be expected on his own cheating hypothesis. This is where Mr Medhurst concentrates his attack. He shows that Hansel's account of these correlations is incomplete and that he has picked out the more suspicious instances without mentioning some instances which fail to support his suspicions. In the second case, Mr Medhurst's criticism is that Hansel accepted the Soal-Goldney report as published, instead of going back to the original record where he would have found that the report was erroneous and the situation was a little less suspicious than he supposed. These criticisms are justified, certainly, but they hardly amount to a serious indictment of Hansel's attack on the Soal-Goldney experiment. Any author who could write a book without opening himself to this kind of criticism would be a real paragon of objectivity.

Mr Medhurst's fourth criticism of Hansel's Soal-Goldney chapter seems to be entirely misconceived. Hansel found in 1959 a curious periodicity in the scoring within runs. Mr Medhurst writes that Hansel 'presented this as evidence of the use of a substitution code, operated by the agent . . . It is not clear why he reproduces this in his book, since he seems to have largely aban-

done the hypothesis of a substitution code'. The relevance of the periodicity effect to Hansel's hypotheses is that, if the percipient had to fake a few calls in each run, memorized in advance, he might be expected to spread these out at intervals over the 25—say one every 5, where the lines are ruled on the score sheets, and this would nicely account for the observed periodicity. Now this would apply to almost all of the hypotheses advanced by Hansel (or at least to trivially slight variants of them) and not to the agent's substitution method alone. Hansel's finding is therefore still relevant.

Mr Medhurst's criticism of Hansel's treatment of Schmeidler's work also seems a little unfair. Because Hansel fails to mention the book by Schmeidler and McConnell at this point (though he does so elsewhere), referring only to a 4-page paper, Mr Medhurst finds it 'hard to avoid the impression' that Hansel wishes to belittle the extent of Schmeidler's work. This seems extraordinarily far-fetched. Why in any case should Hansel wish to do this? And if he did, why would he mention at the beginning of this paragraph that Schmeidler's are 'the most extensive of these tests'? Of course this is again a very trivial issue. As to the substantive criticism of these tests, which Hansel makes and Mr Medhurst questions, my own review of the Schmeidler-McConnell book (*S.P.R. Journal*, June 1959) showed that the question of subject classification was *not* adequately covered in the book. Mr Medhurst seems to have overlooked this review.

Of course I would agree that Hansel's treatment of Schmeidler's work in a few lines is superficial. But on this accusation of superficiality one has to be realistic. Can anyone imagine what a *non*-superficial book about the whole of psychical research would be like? It would certainly never get inside No. 1, Adam and Eve Mews. Surely, if any broad assessment of the field is to be attempted we have to condone some selectivity—and this means superficiality in parts.

All in all, while Mr Medhurst has leaned over backwards to be fair to Hansel over all important aspects of his thesis, he has cancelled the effect of this on the reader by a barrage of minor criticisms which give the impression of a scathing indictment. I would agree that Hansel's book is weak in parts, and particularly superficial where he deals with mediumship and the history of psychical research. But on the central thesis of the evidence for ESP I believe that Hansel has made a powerful and effective attack. It seems to me that Mr Medhurst basically agrees with this and I regret that he did not say so more forthrightly.

CHRISTOPHER SCOTT

SIR,—Mr Scott finds my review of Hansel's book puzzling because, he believes, I 'basically support' Hansel 'on all important points' and yet by some caprice chose 'to give exactly the opposite impression by filling out [my] review with a succession of quite trivial objections . . .'. But Mr Scott has read the review with the eye (if I may say so without offense) of a committed disbeliever, and he has seen what is not always there, as well as missing what is. Thus, he thinks I agree with 'Hansel's basic methodological position', though I said no more than that Hansel had expressed a fair principle to which, in my opinion, he had not adhered; and again he thinks I share Hansel's scepticism about the Jones boys, though I merely said that the possibility 'of the use of an ultrasonic whistle seems to diminish severely the evidential value of these experiments' (let me again urge those unfamiliar with this fascinating case to read the contemporary discussions, glossed over or ignored by Hansel, such as the paper by Soal, mentioned in my review, and Professor Mundle's review, in the *Journal* for June 1959, of the Soal-Bowden book). Mr Scott takes me to task for citing 'another critic' in regard to the Pratt-Pearce experiment rather than offering my own comments. Perhaps I failed to make it sufficiently clear that Dr Ian Stevenson (the other critic in question), besides having contacted the percipient in this case, had made an on-the-spot investigation of the layout of the crucial offices, windows, etc. Under these circumstances, I would have hoped that Mr Scott might consider a reference to Stevenson's report more useful than less well-informed comments of my own. If I did not know Mr Scott better I would have regarded as outright parody his purported summary of my remarks on the Soal-Goldney experiments, i.e. that 'he accepts the physical possibility of all Hansel's hypothesis, objecting to them only on psychological grounds, which he admits to being quite inconclusive'. It is no minor achievement to have compressed into such a small space three quite distinct misinterpretations of what I actually said!

Mr Scott's defence of Professor Hansel's book follows a pattern that has become rather familiar in this and other contexts. This pattern revolves around the accusation that the critic is merely hacking fanatically away at trees while missing the wood. In fact, it begins to appear that the critic who tries to assess the objective merits of the separate arguments in a book such as Hansel's has damned himself in advance. In a carefully leading passage in a review which abounds with loaded phraseology, Professor S. S. Stevens remarks: 'Hansel may have cleared the air from the point of view of the objective scholar, but he will have done little, I predict, to attenuate the fervor of the supernaturalists.

They will read Hansel with an acute singleness of purpose, namely, to trip him up on whatever points he may have muffed, and to fasten their own faith more firmly to the idol of their convictions', and so on (*Contemporary Psychology*, Vol. 12, Jan. 1967, pp. 1-3). So there it is; to return to the previous metaphor, the more trees we can show to be rotten the further we reveal ourselves as unobjective scholars: if the whole forest were to require condemning Hansel's case would doubtless be proved. In fact, as I tried to say in my review, a proportion of Professor Hansel's trees do seem to me sound, but not a sufficiently high proportion to make this the powerful and effective attack that Mr Scott believes it to be.

Turning to Mr Scott's detailed points: yes, I certainly think that Hansel's choice of words, in his account of Soal's discovery of the initial high scoring by Shackleton and Mrs Stewart, reveals an attempt, conscious or unconscious, to bias the reader *ab initio* against Soal. Mr Scott might like to consider why this terminology does not occur elsewhere in Hansel's book. Why, for example, does not Rinn 'allege' or 'claim' that he saw Palladino free her leg in a seance (Hansel, p. 215)? And why does not Mrs Albert 'allege' or 'claim' that she saw Soal altering figures on the score sheets (Hansel, p. 117)?

If it were true, as Mr Scott asserts, that I criticized Hansel merely 'for referring to a book but not referring to it in the right place' then I should certainly have been grasping for straws! I take it that the book in question is the Schmeidler-McConnell book, and the issue here is surely that Hansel's referencing of the Schmeidler experiments was so grossly inadequate as to be actually misleading, a serious fault in any book pretending to scientific status.

Mr Scott's remark about George Pellew's mother leaves me aghast. He appears to be saying that I should have paid serious attention to the *reason* (Mr Scott's italics) she is stated to have given for her refusal to join the American S.P.R., in spite of it being known that she belonged to that Society at the relevant time. I must confess that it did not occur to me that I needed to set out, in plain words, the inference that since she belonged to the Society she did not refuse to join, and since she did not refuse to join she did not give a reason for not joining. There seems to me no possibility of ambiguity in the source (a letter by George Pellew's brother, Professor C. E. Pellew, later Viscount Exmouth) from which Hansel took this story. The story hinges inextricably on an alleged invitation by Hodgson to Mr and Mrs Pellew to join the Society (Professor Pellew hints slyly at an improbable financial motive for the invitation), which invitation, it is quite clear, could

not have been issued. Those interested will find the letter quoted in full in Rinn's book (English edition, p. 126).

Militant advocacy of any cause, be it religion or anti-religion, nationalism or internationalism, belief in paranormal phenomena or rejection of such belief, is apt to bring about a certain blunting of susceptibilities. In the eyes of the protagonists, the enemy are scarcely human, and the ordinary decencies of social relations become not only superfluous but strategically undesirable. I think this is well illustrated in the matter of Mrs Albert's accusation against Soal. Mr Scott sees nothing shabby in Hansel's treatment of this episode, and I am not sure how to make him see how this might appear to the uncommitted outsider. Perhaps the issue may be made clearer if one divorces it from ESP. Suppose, for example, Mrs Albert had said that she had happened to be in the examination room when I was taking my B.Sc. finals and had seen me using a concealed crib (no worse a crime, though she may not have realised this, than that of which Soal was accused). Since both Hansel and Scott are fundamentally decent people I would expect either of them, if they felt they had to put this episode into print, to give the greatest prominence to any doubt or qualification that might tell in my favour. But since this is Soal, then hack at his reputation in any way so long as the demon ESP is thereby exorcised! Hansel, in his treatment of the Mrs Albert story, not only gave it with no caution or reservation as to its reliability but went on to comment on it as though the event she said she saw is established fact (immediately after his version of the story he adds: 'Whether Soal was in fact altering figures or merely tidying them up is immaterial', with the plain implication that he was certainly doing *something* to the score sheets). This, by normal standards, is shabby treatment by one professional man of another, and if Mr Scott does not now see this I can think of nothing further that will convince him. The issue raised by Mr Scott regarding the circumstances surrounding the publication of Mrs Albert's testimony is discussed by Mrs Goldney elsewhere in this issue of *Journal*.

Regarding the other matters raised by Mr Scott in connection with the Soal-Goldney experiments, I find them a little difficult to discuss in a reasonable space because his criticisms of my remarks seem so consistently misleading that a line-by-line refutation would be needed to make an adequate reply. For example, he says, as regards session 28 of the Soal-Goldney series, that I have shown that 'the situation was a little less suspicious than [Hansel] supposed', but this is not so: the suspicious circumstance that Hansel thought he saw does not exist.

Since two of my points (described by Mr Scott as 'complex'—I am not sure why) he at least concedes are 'justified', I shall leave these in order to consider a little further the one he feels is entirely misconceived. This relates to the so-called periodicity effect in some of the Shackleton runs (not, as Mr Scott might be taken as implying, in all the runs, but only in those at 'rapid rate'), which was the subject of a letter by Hansel in *Nature* in 1959. I do not know why Mr Scott questions my remark that Hansel 'presented this as evidence of the use of a substitution code, operated by the agent', since this is just what he did. Up until 1960, when Soal (as now conceded by Hansel) showed that the substitution code described by Hansel on p. 113 of his book could not account for the observed scoring rate, this was Hansel's preferred method of fraud. When he discovered the 'periodicity effect' he believed he had found actual proof of fraud, in this form. In *Nature* he said: 'I have stated elsewhere that the result of this experiment could have been produced by normal means. I now propose to show that this explanation in terms of everyday processes—besides being possible—is necessary in order to account for features which are present in the published results'. One cannot help feeling that for the moment Professor Hansel was carried away!

Mr Scott suggests that even if this form of code is abandoned, the 'periodicity' finding is still relevant because in almost all the hypotheses advanced by Hansel the percipient might tend to locate his faked calls at regular intervals on the score sheets. This would not be so, for example, when Soal is said to have handed to the percipient sheets marked up ready for faking, but perhaps it might be argued that Soal would also tend to use preferred positions which happened to coincide with those of the percipient.

However, there is an odd feature of this 'periodicity' that has not, so far as I know, been discussed previously and appears relevant to a point that Mr Scott makes. The basic reason for the periodicity, on the fraud hypothesis, is the existence of lines ruled on the sheets after every fifth guess position which are said to have been used as aids to memory in positioning the faked calls. As Mr Scott says: these faked calls might be expected to be spread out 'at intervals over the 25 [calls in each run]—say one every 5, where the lines are ruled on the score sheets, and this would nicely account for the observed periodicity'. Curiously enough, it wouldn't, or at least not in this simple way. It so happens that when the numbers of rapid-rate hits are plotted against the position in the run, well-defined *minima* are found in positions 5, 10, 15 and 20, i.e., just

above each division line, and this largely accounts for the statistically significant periodicity. The maxima, not so nearly equal in magnitude or so well defined, occur at positions 2, 6, 13, 17 and 22, very oddly located, one would have thought, in relation to the ruled lines if these were used as an aid to memory when the fraudulent percipient or experimenter entered the spurious hits.

As regards Dr Schmeidler's work, I hardly think it is for me to suggest a motive (as Mr Scott invites me to do) for the striking inadequacy of Hansel's documentation of these extensive experiments. But I am glad to see Mr Scott's confirmation that Hansel's animadversions on subject classification *were* intended to apply to the Schmeidler tests, since Hansel does not say this and I had only inferred it. I had not overlooked Mr Scott's review, but in the same issue of the *Journal* there are detailed replies on the question of subject classification both from Dr Schmeidler and Dr McConnell, and in absence of any subsequent discussion from Mr Scott I had assumed, perhaps wrongly, that the replies were satisfactory. But this seems no excuse for Hansel's failure to amplify or substantiate the suggestion, which the reader is led to infer, that Dr Schmeidler's results might be spurious due to reclassification after the scores are known.

Perhaps in conclusion I might rephrase my objection to Hansel's book, which, to judge from Mr Scott's defence of this book, I may not have made sufficiently clear. *ESP: A Scientific Evaluation* is represented as 'a dispassionate and comprehensive study' of 'the whole history' of psychical research and an 'exercise in honest scepticism', though one of Professor Hansel's admirers does express doubt as to whether 'his care and diligence in the sifting of evidence [will] put an end to the counter claims of the faithful' since even though 'casuistry for the cause of ESP may suffer a temporary blockage, . . . the will to believe can surely find a loophole to squeeze itself through' (I have pieced together this panegyric from the publishers' blurb, Professor Boring's Introduction and Professor Stevens' review, cited above). Instead, and very disappointingly, I found it to be a book which had interest in those sections which summarized and sometimes modified Hansel's already published criticisms of five well-known ESP experiments, but for the rest gave a spurious impression of comprehensive coverage of the psychical field by uncritical quotation from dubious sources and misleadingly superficial accounts of cases and experiments some of which are of great interest and complexity. In spite of Professor Boring and the publishers, I had not expected a truly comprehensive treatise which, as Mr Scott rightly says, might hardly be housed in 1, Adam & Eve Mews,

but I did expect that those cases chosen by Hansel for comment would be treated in an unbiassed way and that Hansel would show evidence of more than a token acquaintance with the literature; it might also have been reasonable to expect Hansel to have selected cases fairly representative of the field he has chosen to evaluate. Mr Scott feels that the bulk of my criticisms are trivial. I, on the contrary, feel that Professor Hansel has achieved an appearance of having demolished the case for the paranormal only by the too frequent use of selection and distortion.

R. G. MEDHURST

The Shackleton Experiments: Mrs Gretl Albert's allegation against Dr Soal

SIR,—Mr Scott, in his criticisms of Mr Medhurst's review of Professor Hansel's book *ESP: A Scientific Evaluation* (S.P.R. *Journal*, March 1968), states that 'Mrs Albert's allegation was suppressed for 17 years until Mr Fraser Nicol and myself brought pressure on the authors concerned to publish it'. This statement might well convey a misleading impression and since many readers will not have available the account given by Dr Soal and myself in the *Journal* for September 1960 (40, pp. 378-81), the editor has kindly allowed it to be reprinted here in full:

Readers of the Basil Shackleton report ('Experiments in Precognitive Telepathy', *Proc.*, 47, 1943) will remember that we tried out several people as agents and that two people besides the main agent (R.E.) were successful. One of these was Mr J.Al., the other was Mrs G.A., who attended two sittings, at the second of which the very interesting 'Double Agent' experiment was carried out (op. cit. p. 60). As there have been surmises as to why she was dropped after the second sitting when she had proved so successful; as these enquiries have lately recurred, and as it has been brought to our attention that a sentence in the report is misleading,¹ we have decided to publish the following statement. This account is based on very full notes made at the time by K.M.G.

Statement regarding two sittings attended by Mrs G.A. on 16 May, 1941 and 23 May, 1941

Whilst S.G.S. was looking out for people to try out as agents with

¹ This sentence reads: 'A number of persons witnessed the successful scoring and all testified to the fraud-proof character of the methods employed'. (op. cit. p. 36). This remark is strictly true of all the observers, twenty-one in number, who assisted in the experiments. G.A. was not an observer (or witness) but acted as an agent, in the same way as R.E. and J.Al. None of these three agents are listed among the witnesses on p. 145 of the report. Nevertheless, the remark as it stands may be considered misleading. For the very favourable testimony of the witnesses see the report p. 80 ff.

B.S., he asked K.M.G. if she could suggest anybody. She suggested Mrs G.A. She and her husband were, and still are, personal friends of K.M.G.'s. Mrs G.A. attended for the first time at sitting No. 15, at which K.M.G. was also present. After this sitting Mrs G.A. asked K.M.G. whether Shackleton took drugs and related that she had seen three cigarettes lying loose on his table together and had taken one, remarking to him 'I have taken one of your cigarettes'. He looked surprised, she said. She started smoking it 'and felt shortly as if she would faint; her mind had a feeling of being somewhat drugged, and she had to put the cigarette out and throw it away', and thereafter developed a headache. This was her description and we can only comment that we had no reason to believe B.S. used drugged cigarettes or took drugs in any form. During the course of over two years' experiments with B.S. many persons must have smoked his cigarettes, including K.M.G. But at no time was any similar comment made by anybody else.

Unfortunately K.M.G. was unable to be present at the next sitting, No. 16, at which G.A. again acted as agent. At this sitting Dr B. P. Wiesner, D.Sc., Ph.D., (an Austrian by birth, resident in England) attended as observer. On K.M.G.'s return to London, G.A. told K.M.G. that at this last sitting she had seen S.G.S. 'altering the figures' several times on a score-sheet, observing this through the small aperture of the screen placed between the agent and the experimenter who showed the agent the cards (op. cit. p. 38).

K.M.G. was very taken aback at such a statement and proceeded to make the fullest investigation privately, without at this stage informing S.G.S. She asked G.A. what she had said or done when she noticed this. G.A. said she had not liked to do anything, as K.M.G. was not there. She said she did murmur 'What are you doing?', but since nobody took any notice, she said no more, neither did she at the time, or later, say anything at all to Dr Wiesner.¹ (It would have been perfectly possible for G.A. to speak to him in German, her native language and his, had she wished to say something which S.G.S. would not understand). K.M.G. asked G.A. if she could indicate on what sheets the figures had been altered; she thought it was on the first sheet.

K.M.G. then asked S.G.S. to bring to the next sitting all the score-sheets of sitting 16 for her to see, since she would be interested to see the scores achieved in her absence. S.G.S. duly brought them along, still unaware of the allegations made. K.M.G. examined them minutely for any sign of alterations. She could see none and, since the figures were written in ink, alterations might have shown up clearly. She then

¹ Dr Wiesner has recently confirmed that G.A. said nothing at all to him at the time and that he had not heard of the allegation before being told of it in June, 1960. He placed no reliance at all on the statement in its implication as he had approached the experiments in a very critical and sceptical state of mind and had formed a very favourable impression of the care and precautions taken. He had watched the proceedings most carefully himself, as instructed to do, and had seen nothing amiss.

gave the score-sheets to a friend, Miss Olive Stewart, personal secretary to Mr H. W. S. Wright, a London surgeon. Miss Stewart was a most careful and reliable person with whom K.M.G. had worked on medical records. K.M.G. told her of the assertion that the figures on the score-sheets had been altered, though she did not mention at that time S.G.S.'s name. Miss Stewart also subjected all the score-sheets of sitting 16 to very careful scrutiny, was quite unable to detect any signs of alterations, and gave K.M.G. a signed statement to this effect.

When told about it, S.G.S. was extremely indignant that such remarks should have been made, particularly since no steps were taken at the time to ask him what he was doing; when nothing had been said to Dr Wiesner, the witness, which he might have looked into at the time; and when subsequent examination of the score-sheets showed no sign of anything suspicious. He insisted that K.M.G. should make perfectly clear to G.A. the seriousness of such remarks and that he would have no hesitation in resorting to legal action if he heard of such statements being repeated. He also refused to have G.A. at any further experiments.

When the report came to be written, we discussed whether or not to describe this incident. K.M.G. was in favour of including a description of the matter in one of the appendices to the report, but S.G.S. decided against any mention of it. He was conscious of the responsibility which rests on anyone who has to decide whether to hand over to a hostile critic or to withhold from him a bit of information which he reckons that that critic is likely to use unfairly if it is made available to him.

Some critics leave no stone unturned to imagine combinations of fraud; even the observers, brought in to check the agent, are postulated as being 'in the trick'; and honest observers are presumed to be half-wits who never have either the intelligence or the good fortune to detect cheating in those they have been instructed to watch. What misfortune attends the honest observer, in the manipulation of the story by such a critic! Never do his eyes turn in the right direction at the right moment, no, not in years of experiments; and how different from the dishonest observer and the dishonest experimenter whose quick wits achieve their wicked ends every time! It was with this type of critic in mind, of which he had even then had some experience, that S.G.S., bearing in mind the lack of any supporting evidence for Mrs G.A.'s statement, was unwilling to turn attention away from the remarkable pattern of apparently precognitive scoring in the B.S. experiments and to draw a red herring across so interesting a track.

Comments

It is obvious that G.A. had every right, in fact a duty, to keep her eyes wide open and satisfy herself that all was in order. She thought she saw S.G.S. altering some figures, and would have been acting correctly immediately to query his actions and receive an explanation—both her query and S.G.S.'s explanation being thus brought to the attention of the observer, Dr Wiesner. But though K.M.G. sympathizes with G.A.'s reluctance to 'kick up a fuss' amongst strangers with scientific

attainments, it is obvious that this reluctance produced a very serious state of affairs.

It would be unfair to all concerned not to remark that G.A. whilst reaffirming now her impressions at that time, fully realizes, and herself has stated, that she might have been mistaken in what she thought she saw. We must point out that her impression might have had a very natural explanation. For in the course of writing down a large number of figures during experiments, one is apt to lose neatness to a minor extent, and almost everyone has the habit of occasionally going over and re-shaping ill-formed letters or figures.

Finally, taking the experiments as a whole, it would seem extremely unlikely that an experimenter would risk altering the records during the course of the experiment with an observer standing by, or that he would get away with such a procedure without being observed by any one of the other witnesses, twenty-one in all, invited to watch and check the Basil Shackleton experiments.

(signed) S. G. SOAL (6th July, 1960)
K. M. GOLDNEY (8th July, 1960)

Mrs Albert's allegation was first made whilst I was having dinner with Mr and Mrs Albert at their home on 29th May, 1941—six days after the experiment in question. In a long discussion with both Mrs G. A. and her husband, I got the impression that she had not realised the importance of these experiments (she knew little or nothing about psychical research), nor what serious consequences her allegation might bring about. My notes of this conversation, dated 30th May, 1941, state 'She said the figures on Dr Soal's sheet, and alterations, were in ink; I said that in that case the alterations would be apparent'. The sequel is described above.

My files contain scores of letters between myself and Dr Soal from 1934 onwards, together with contemporary private notes concerning Mrs Albert's statement, etc. I should be happy to allow senior members of the Society to examine these at my home; I think that reading the many, many letters that passed between myself and Dr Soal gives an interesting insight into our discussions and actions whilst the experiments with Shackleton were in progress.

Our joint statement reprinted above shows that I myself was anxious to include a description of all that occurred concerning Mrs Albert in our report on Shackleton; but Dr Soal felt that he ought not to go out of his way to create suspicions and doubts by drawing attention to an allegation for which there was no real evidence whatever. Such action might cause detractors of the subject to draw attention away from the important results being achieved. I thought then, and I think now, that Dr Soal made a

mistake of judgment in deciding this. But I was a very junior researcher in this field at that time and felt that it would not be right, in fact would be intolerable, were I to take independent action against the considered decision of the Society's most able and experienced investigator.

Nevertheless, a sense of my responsibility was always a matter of concern to me, and so little was I desirous of 'suppressing for 17 years' all that had occurred, that I proceeded to write a very detailed and much fuller statement, with copies of the letters at that time between myself and Dr Soal; copies of my contemporary private notes; and comments of a personal and private nature on various aspects of the matter which, I felt, pointed to Dr Soal's integrity. This long, confidential, and contemporary document was deposited at the SPR to be available should later circumstances in my view demand it. Further, this document was shown to various friends whom I trusted on both sides of the Atlantic (including Mr Scott), so that certain researchers would have fuller information than could conveniently be printed and would be made aware of arguments which, I repeat, convinced me (who knew more of the persons concerned than did anybody else) of Dr Soal's integrity in the matter. It would be only fair to those who saw this document to say that most of them urged me to make the matter public independently. I am sorry if I was wrong in deciding not to do so until Dr Soal agreed the time had come for a joint statement.

I agree with the final paragraph of Mr Medhurst's review on p. 232 of the *March Journal*. Psychological research is a most difficult, and often a pretty heartrending occupation.

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SIR,—May I explain my reasons for not allowing any account of Mrs G. Albert's unfounded allegations to be published till the year 1960. In the first place careful inspection of the scoring sheets a few days after the incident failed to show any sign that figures had been altered or erased. I had at the time a post in the University of London which I knew would normally continue till 1958 at the earliest. As everyone knows accusations of fraud can be almost as damaging as convictions. Had such a report been published there would be a very real risk that some newspaper writer would have taken up the matter and probably one who was biassed and unfair at that. Lecturers and students would read

the articles with lamentable consequences, (or unpleasantness at the very least) for myself. I had nothing to reproach myself about, and there was no evidence whatever that I had cheated. It now turns out that Mrs Albert did not even understand what the experiments were about! I felt at the time, and I still feel, that I had every justification in not publishing any report while I was still associated with the University. Immediately that association ended I did not object and a full report appeared in 1960. Far from being 'mistaken' in what I did I felt it was the only action I could take to protect my own reputation against truly alarming contingencies.

S. G. SOAL

The Music of 'An Adventure'

SIR,—In support of the Revd. J. P. Hill's view, in a letter published in the December *Journal*, that 'less than justice' has been done to Miss Jourdain in the matter of the Versailles music, I should like to put forward some further considerations which I think favour the attribution of retrocognitive character to the experience.

I take it as clear that when Miss Jourdain wrote 'The pitch of the band was lower than usual' she could not have meant 'usual for this particular piece', as Ernest Newman and Rollo Myers have supposed, but that she meant something like 'usual for a piece of that type, as heard on instruments today'. For proof that a piece that one has never heard before may give the indubitable impression of being at a 'lower pitch than usual', reference may be made to the Suite in E minor by J. S. Bach, printed in vol. VII of the Steingraber Edition, pp. 54-61. The editor prefaces this with the remark (translated from the German): 'The deep tone-level at which this piece moves has given rise to the conjecture that it was originally intended for the lute'. If, for example, one played the Bourrée to any knowledgeable musician who had never heard it before, the inevitable reaction would be, 'Why is it written so low in pitch?' Ernest Newman's objection that 'no musician could listen to a piece of music he had never heard before . . . and say that "the pitch was low"' is therefore ill-considered, to say the least.

If we ask what might have caused the impression of 'lower pitch than usual' given to Miss Jourdain by the music she heard, there appear to be two possible kinds of answer. First, the selection of instruments in the band might have been somewhat more low-pitched on the average than what is usual today. Wanda Landowska,