

If Stepanek can still perform successfully when cues of this nature and other possible sensory cues cannot be utilized by anyone present during the tests, there should be no difficulty in convincing critics like myself that he utilizes E.S.P. Parapsychologists do not, however, appear to have availed themselves of this opportunity to prove their case.

Professor Stevenson is concerned with the freedom with which accusations of fraud are thrown around when parapsychology is discussed. But fraud is a frequent ingredient of parapsychology, as its history shows, and its possibility can never be ignored. Professor Stevenson says that it is not quite impossible that Pearce could have cheated. Presumably, by this, he means that it is possible that Pearce did cheat. That also is my conclusion.

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DEAR SIR,

Critics of my review have taken me up on a number of points of detail. I am grateful for corrections where I have misstated facts, even though they are, I think, of minor and peripheral significance. It is important that readers should get as fair a view of the field of dispute as possible. To the references quoted in the correspondence there are a number of others to be added. Professor Stevenson's review of *The Spiritualists* should be supplemented by Mr. Hall's reply (1) and by his own rejoinder (2). A full account of the period in the history of the S.P.R. during which the Smith-Blackburn hoaxes occurred has been provided in a work of very careful research by Mr. Hall (3); and the criticisms of J. F. Nicol, referred to by Dr. Beloff, have been answered at length and in detail by Mr. Hall (4). Blackburn had a number of disreputable adventures as a young man; but he was no villain, and he settled down into being a solid and respectable citizen: "It was in mature life and not during the follies of youth that Blackburn revealed that he and Smith had tricked the S.P.R." (Hall, *loc. cit.*)

Medhurst and Goldney, cited by Dr. Beloff and Dr. West, did their best for William Crookes, but in effect could do no more than reach a verdict of not proven. That is not the last word. Their arguments, together with all the other criticisms of *The Spiritualists*, were subjected to a judicial appraisal by Dr. Eric Dingwall (5). Dr. Dingwall is a universally recognized authority; he himself at the offices of the S.P.R. twice interviewed Anderson, the key witness on the question of Crookes's motivations, who has been most under attack. Dr. Dingwall considers that on the

material points Anderson was a truthful witness and his memory is not likely to have been at fault. However that may be, the case for regarding Crookes as the dupe rather than the ally of Florence Cook is, in Dr. Dingwall's judgment, so thin as to be unacceptable. Dr. Dingwall found himself in complete agreement with Mr. Hall's conclusions.

I must return to my own views about these vexed questions. The evidential value of the Pearce-Pratt experiments is annihilated by two circumstances, either of which by itself is final and sufficient: (1) Pearce was not supervised; and (2) the experiments were carried out from August, 1933, to March, 1934, but not adequately reported until 20 years later, in 1954.

It is, to me, very surprising that, on those facts which are not in dispute, parapsychologists should think that Crookes can be defended. Can anyone—do any of them?—now believe that Florence Cook ever in her life produced genuine full-form materializations? Yet William Crookes maintained that she did, and his detailed statements make it plain that he was in a position to know. If the materializations were not genuine, then Crookes told lies about them. If one of the world's great scientists, of such unimpeachable integrity that he is elected to the Presidency of the Royal Society, can go on record with lies about his parapsychological investigations, then the *bona fides* of absolutely nobody (with offence intended to none) can be allowed any weight in the evidential balance-scale. This sounds desperately unfair on the parapsychologist, but if he undertakes to prove a miracle this is the burden that is laid upon him.

As it seems to me, none of Professor Hansel's critics appreciate the strength and solidity of his position. In effect, parapsychologists are claiming that miraculous, and as far as we can see lawless, phenomena are part of the structure of the universe in which we live. Millions of miracles, telepathic, precognitive, etc., are happening every second. Objects are moved without force being applied, and information is conveyed from point to point at a negative velocity. If this is so, then our complexly interlocked sciences of physics, chemistry and biology are rotten to their foundation and the logic of science is a mockery.

Surely, there are just two ways ahead for the parapsychologist. He can either combine thesis and antithesis in a new synthesis, and show us a scheme of things entire in which science and para-science are harmoniously united. Or he can face us with a brute fact and force it down our throats though it choke us. If telekinesis is a fact, then nothing stands in the way of the perpetual motion machine. Let him show us a

working model, operating on thought power, and we will give him best.

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PHYSIOLOGICAL MEASUREMENTS OF ANXIETY

DEAR SIR,

There has been a welcome trend within recent years for physiological indicants of anxiety to be assessed in close association with the clinical practice of psychiatry. Dr. D. H. W. Kelly and Dr. C. J. S. Walter are to be congratulated for developing the technique of forearm plethysmography to where it contributes to the routine management of an anxious patient. Their recent paper (*Journal*, May, 1968, p. 611) reports findings on the levels of blood flow and other measurements in relationship to a variety of clinical diagnoses. The technique can be readily understood and applied easily by those relatively unsophisticated in physiology or statistics; it is to be hoped that its use will soon be universal so that it can assist the evaluation of different approaches to the treatment of anxiety. However, before this takes place, I venture to suggest some alteration in the method of statistical analysis of the data.

Examination of their findings reveals that it is inappropriate to examine means and standard deviations without prior transformation to achieve a normal distribution. The distributions of "basal" blood flow given in Fig. 2 are asymmetrical and pertain more to log normal. Also there is a rectilinear relationship between the means found in each diagnostic category and their standard deviations (Table III) ($r = 0.74$; $p = 0.01$); such a heterogeneity of variance can be largely corrected by logarithmic transformation. This necessity to transform biological observations to logarithms before

statistical analysis has long been acknowledged (Gaddum, 1945); indeed with measurements of skin resistance, it has been shown that if this is not done the conclusion is affected (Haggard, 1949).

Kelly and Walter report measurements of blood flow obtained under "basal" and "stress" conditions as if they were independent. This fails to draw attention to the common observation in psychophysiology that the magnitude of the response is dependent on the initial level; necessity to pay attention to this has been advocated in many papers by Wilder on the "Law of Initial Value", and he has even suggested that its application should be extended beyond biology to sociology (Wilder, 1967). There is a sound theoretical reason for applying it to measurements of blood flow, where the amount of increase that can take place in response to any stimulus is determined by the number of arterioles that are not already dilated, in addition to the cardiac output. Presumably dilatation of the majority of arterioles in the anxious patient under "basal" conditions precludes much further increase in blood flow under "stress", whatever the magnitude of the stimulus and his psychic response to it. The same argument in favour of examining reactivity has been applied to the measurement of change in skin conductance, where the index is the proportion of active sweat glands. In view of the evidence in favour of analysing the measurements as logarithms, it should be noted that examination of change will then be in a dimensionless unit as the difference between two logarithms is a ratio. This has another advantage as it cancels out a common denominator whose measurement may be little more than a guess (i.e. forearm muscle volume). Kelly and Walter do look at percentage increase but they do so without allowing for the diversity in initial level.

Kelly and Walter report that the "basal" forearm blood flow of normals rises with increasing age ($r = 0.29$; $p = 0.05$). This type of association is commonly found in human physiology; both intrinsic heart rate and vital capacity fall, and, of course, blood pressure rises. It is thus disappointing that Kelly and Walter do not provide information on the exact relationship; presumably this could be calculated by regression equation as has been done for intrinsic heart rate (Jose, 1966) and vital capacity (e.g. Campbell, 1963). This can only improve the discrimination between the diagnostic categories, especially as 17 years separate the mean ages between two of them; Kelly has chastised others for not paying attention to this (*Journal*, October, 1965, p. 1012). What a pity that analysis of variance was not used to establish the significance of the observed differences. Not only is it more elegant, but the age