

## Sacred cows and white elephants\*

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### International organizations

At 4.27 p.m. on Monday, 13 September 1982 the International Council of Scientific Unions voted to admit the International Union of Psychological Science into full scientific membership. The latter is the international coordinating body to which most of the national psychological associations, such as our own Society, belong. It was established in 1951, and our own Society was a founder member of it, along with 10 other national psychological societies. Its original title was the International Union of Scientific Psychology, its present name being assumed in 1966. The change in form may have a certain significance to which I shall refer later.

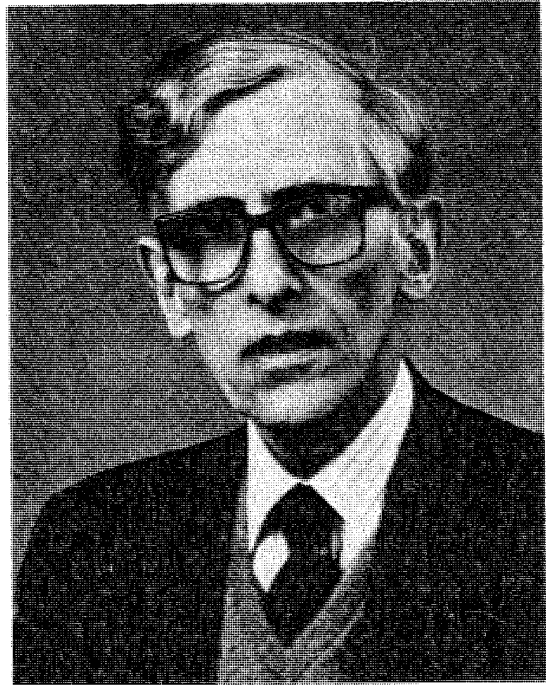
The International Council of Scientific Unions, which was established in 1931, is the international scientific organization which coordinates the various international scientific unions covering the whole range of scientific activity. The scientific areas represented by this Union are listed in Fig. 1. Psychology takes its place among the natural or physical sciences, or as in the case of mathematics and history and philosophy of science, subjects which are directly related to them. These are the exact sciences, in contrast to the rather more hit or miss social sciences. These sciences are more likely to regard human beings as organisms than as people.

Astronomy	Immunology
Biochemistry	Mathematics
Biological Sciences	Mechanics
Biophysics	Micobiology
Chemistry	Nutritional Sciences
Crystallography	Pharmacology
Geography	Physics
Geology	Physiology
Geophysics	Psychology
History and Philosophy of Science	Radio Science

Figure 1. International Council of Scientific Unions (ICSU)

There is also an International Social Science Council, founded in 1952, to which another set of international unions belong. These are listed in Fig. 2. These sciences are more likely to regard

\*The Presidential Address given at the Society's Annual Conference, at the University of York, April 1983.



Administrative Sciences	Peace Research
Anthropology and Ethnology	Political Science
Demography	Psychology
Economics	Public Opinion Research
Geography	Research Councils (Social Science)
Legal Sciences	Sociology
Mental Health	

Figure 2. International Social Science Council (ISSC)

human beings as people than as organisms. Two of the subjects appear on both lists, namely geography and psychology. Whereas psychology was one of the original members of the ISSC when it was founded in 1952, it was to be another 30 years before the ICSU was ready to include psychology among its members. Moreover, psychology's application for membership in 1982 did not go uncontested, although there was a good majority in support of its application. Professor Klix, the IUPsyS Chairman, in presenting the candidature of IUPsyS for ICSU membership, stated 'Psychology is in part a natural science and in part a social science'.

### Psychologists as scientists

This has led to considerable rejoicing among psychologists, for we see 13 September 1982 as marking a significant step forward for our discipline. Surely the

fact that psychology has been admitted to this important international scientific organization after some 30 years out in the cold, means that we have now been recognized as scientists employing respectable and recognized methods of research. Is this the end of the long haul from the days when William James described the psychology of the 1890s as 'only the hope of a science'? (James, 1892) That psychologists have long wished to be regarded as scientists there is little doubt. I can remember hearing Professor Flugel at University College, London, in the 1940s, describing just this. He explained how welcome was the introduction of Hipp's chronoscope to psychological laboratories. It looked so much like a real piece of scientific apparatus with its polished brass and glass covering case. Moreover, it made an encouraging 'ping' every so often when it was in operation. This little noise emanating from various parts of the laboratory made us all feel that we really were becoming scientific. Since then, of course, we have been able to develop a highly sophisticated range of hardware, most of it designed to measure something, and most of it looking very similar to the hardware of the physical sciences. In my own line, that of clinical psychology, I can well remember the boost we received when we were able to use biofeedback apparatus. It looked so unquestionably scientific.

Why was this scientific status so important to us? After all, we do not hear chemists and physicists expressing anxiety about their scientific standing. They do not have to insist that they are real scientists. The difficulty resides, I believe, in the nature of our data. A chemist can talk about atoms, molecules, pH values and valencies; a physicist about fields of force, neutrons, protons, and quanta. They are not likely to be questioned by the layman. A psychologist has no such protection. His data are directly accessible to everyone, and everyone has an opinion about them. Inasmuch as everyone is interested in predicting how other people will behave and guessing why they do the things they do, then everyone is a psychologist: the butcher, the baker, the candlestick-maker, grandma and the boy next door. Not surprisingly, those who wish to 'hold themselves out as psychologists' in the professional sense, need to claim some special attributes. These are that they have had a scientific training and therefore approach the subject empirically. Scientific psychology wishes now to be seen to depend less on armchair speculation and more on experimentation. Scientific psychology seeks particularly to be sharply distinguished from the more speculative activities of those who seek to tell us how to make friends and influence people. This is all entirely understandable. Psychology as an area of study and as a field of professional practice, is beset by people advancing opinions as if they were facts, based on no evidence whatsoever, and offering psychological services based on such opinions. It is not surprising, therefore, that we have been greatly concerned to establish ourselves as reputable

scientists, and have tried to offer services only when based on some sort of scientific foundation. More recently, of course, we have been considering legal registration as a means of enhancing our professional status.

### Definitions of psychology

In Wundt's day, psychology was concerned with mental phenomena and with data gained from introspection. For him it was 'the science of immediate experience'. A reasonable definition of psychology at that time would have been 'the branch of science which investigates mental phenomena or mental operations'. Psychological textbooks in the 1950s, 1960s and 1970s dropped the suggestion that psychology any longer studied human experience, and stated that psychologists could only properly study behaviour. Although George Miller in his textbook of 1962 defined psychology as 'the science of mental life', he qualified this definition by saying that this science was 'based on evidence obtained through the observation and analysis of one's own behaviour and the behaviour of others'. This at least had a reference to people. I think it would not be unfair to say that many textbooks published in the three decades referred to would have been happy to define psychology as 'the systematic investigation of the behaviour of organisms'. It was during this period that the International Union of Scientific Psychology became the International Union of Psychological Science. Scientific psychologists began to regard themselves as psychological scientists. Many went further and described themselves as behavioural scientists, dropping any further pretence of being interested in the psyche. Indeed, it has now become the fashion to teach medical students behavioural science rather than psychology.

On the morning of Thursday, 27 January 1983, in this instance the exact time has not been recorded, the University Grants Committee classified psychology as a biological science, when before it had been classified as a social science. This reclassification has the advantage of making laboratory grants available to psychology departments, and may well contribute to the association of psychology with the natural rather than the social sciences. This might suggest to some people that psychologists are becoming more concerned with organisms than with people. If this idea gained ground, what would *that* do to our public image?

I shall define psychology as a *scientific discipline which sets out to understand why people behave in the ways they do*. In framing this definition, I have used the term 'understand' for reasons which will appear later. You will also have noticed that I opt for the term 'people' rather than 'organisms'. A corollary to this definition would be that such understanding enables psychologists to provide information and expert services in a number of applied fields.

### Natural science and understanding people

I do not wish to suggest that we are not perfectly aware that our discipline, as Professor Klix has reminded us, is both a natural and a social science. Nevertheless, I think it might be worth considering some of the implications of regarding psychology as if it were one of the natural sciences, if at the same time we are in the business of studying people rather than organisms. To do this, I shall select three of the central requirements of experimentation or data collection, which Popper (1959) insists are necessary for scientific respectability. These are:

- (a) that data must be collected under controlled conditions by means of carefully designed experiments, preferably in laboratories;
- (b) that data must be objective and accessible to all observers;
- (c) that experiments and observations must be replicable.

I now propose to outline three steps which I think we need to take when we seek to understand why people behave in the ways they do. I shall then examine the extent to which these steps are compatible with the three requirements of natural science to which I have drawn your attention.

*Clearly, we begin by studying human behaviour: what people do.* This inevitably takes us out of the laboratory and out of any situation in which we can hope to control the conditions under which our data are observed. I am aware, of course, that astronomy and biological sciences depend hugely on the collection of data by means of uncontrolled field observations. In many cases, these data may lead to controlled experiments and measurements which confirm predictions made in the light of earlier observations. The problem for psychology is the extent to which it will accept data based on anecdotal evidence, which cannot later be confirmed by experiment.

*We have next to invite our subjects to tell us what they are thinking and feeling when they behave as they do.* Imagine trying to understand a cricket match or a Quaker meeting without being able to inquire into the purposes and experiences of the participants. One can imagine the frustrated psychologist asking the cricketer or the Quaker, 'What on earth are you doing?' and getting the reply: 'You can see what we are doing!' The unfortunate psychologist would have to admit that he could see no such thing. While the psychologist could describe the behaviour, or lack of it in the case of the Quaker meeting, he would not know what was going on. Essential data could only be elicited by asking the participants of the cricket match or Quaker meeting to report their introspections. This is not at all out of the way, of course, since much of cognitive psychology must depend on recording the subject's introspections as he perceives, thinks and remembers. Even a personality inventory

like the EPQ depends heavily on the subject's being able to report on his introspections: for example, three of the questions are: 'Do you feel lonely?', 'Are you touchy about some things?' and 'Do people who drive carefully annoy you?'

But even this is not enough. It would be quite difficult to explain by verbal report exactly what it was like to play in a cricket match or to take part in a Quaker meeting. It would only be by participating in the match or the meeting that a psychologist could begin more fully to understand what was going on. *In other words we have to refer to our own thoughts and feelings if we are to make a meaningful assessment of what is observed.* It is a truism that however much one can observe another's bereavement and however much the bereaved person can describe his experiences, it is only when the observer himself experiences bereavement that a reasonably accurate understanding can be achieved. 'I did not really know what it was like until it happened to me'. I am told by animal psychologists that it is often helpful in theory building to suppose that an animal 'knows it will be rewarded' or that it is 'expecting something'. This sort of thing can only be based on the observer's own introspection: he will know what it is like to 'expect something', or 'to be rewarded'.

Many psychological events of the greatest importance only occur very rarely or perhaps only on a single occasion in a given individual's life. There is thus very little possibility of setting up a replicable experiment or of repeating the original observation. If we could repeat an experiment with a given individual we can expect learning effects. If we perform a similar experiment with another individual, then we have to take into account individual differences.

Turning back now to our selection of three of the requirements of natural science, we have the following problems to consider:

- (a) the collection of data under controlled conditions: but what about *anecdotal evidence*?
- (b) the objectivity of data: but what about *introspective data*?
- (c) replicability: but what about *rare or one-off events*?

I propose now to discuss each of these in turn.

### Anecdotal evidence

It is often considered that one of the most damning things that can be said about a psychological statement is that it is based on anecdotal evidence. This assumes that all anecdotal evidence — all evidence obtained outside controlled experimental conditions — is of the 'I know someone who has a cousin whose aunt says . . .' variety. The implication is that such evidence was unrecorded at the time, and thereafter related third or fourth hand and subject to all the well-known falsifications of memory. However, I shall take the term 'anecdotal' to refer to evidence about

events which have occurred spontaneously in uncontrolled conditions. Any event which has been contrived, especially when it has been brought about in controlled conditions, takes on the nature of an experiment. It is the evidence or data gained from spontaneous, often unexpected, events over which we have no control that I regard as anecdotal. However, records of the nature of such events and the conditions under which they occurred, can be made carefully and meticulously, and thereafter evaluated. They need not share any of the attributes of the casually observed and casually reported data usually associated with anecdotal evidence.

I have already noted that astronomy has necessarily to gather its data from the observation of events over which astronomers have no control whatever. The biological sciences, in particular, must necessarily base their initial data on the gathering of evidence which would have to be called anecdotal in the sense that I am using the term. Psychology is no exception. Bromley (1977) was, I think, the first person to point out that much psychological data has to be evidential and must therefore be evaluated in a quasi-judicial manner. In a valuable discussion of the psychological case study, he introduces us to the quasi-judicial method. 'A case study', he says, 'is essentially a reconstruction and interpretation, based on the best evidence available.' He goes on to lay down six rules for the preparation of a case study. These concern the need to ensure the accuracy of the evidence, and the importance of any particular fact being established by rational argument and not by rhetoric. There should be a statement of the aims and objects of the study and an assessment to the extent to which these have been achieved. Any inquiry into episodes of deep emotional significance to the person should be conducted by someone suitably trained to do so. The person should be seen in his ecological context, both biological and cultural. And finally, the case study should be written in good plain English with due regard to high standards of evidence and argument. These sorts of consideration are important because they treat anecdotal evidence seriously, and seek to make it capable of contributing not only to inquiries made by applied psychologists in the field, but to the collection of research data for theory building.

A splendid example of the use of anecdotal evidence — that is, the use of spontaneous, uncontrived and unlooked for data — was given us by Ellis (1983) in the paper he gave at the London Conference as last year's Spearman Medallist. He told us about Luria's theory of writing. Luria proposed that when we want to write a word we first retrieve its pronunciation from some sort of internal lexicon, and that we then apply our knowledge of sound-spelling correspondences to generate the correct letter string. However, Shallice produced a brain-damaged patient who could spell many familiar words correctly, despite not being able to generate plausible spellings for pronounceable neologisms

such as BLEP. He could both hear and repeat the neologisms, but he could not spell them. It would appear, therefore, that he had lost his capacity for sound-spelling conversion, while retaining his capacity to spell familiar words. Clearly then, Luria's scheme would not do. Ellis therefore proposed an alternative scheme: the patient's correct spellings of familiar words must have come from some sort of graphemic lexicon used in the production of writing and probably reading too. Here we have a single piece of anecdotal evidence properly observed and recorded which is enough to lead to the modification of theory. I shall be returning to the problem of anecdotal evidence when I consider the matter of rare or one-off events.

#### Introspective data and phenomenology

It is now time to consider the problem of introspective data. I should like to spend a few minutes examining the popular assertion that observable behaviour is a fit subject for scientific study while observable mental events are not. In doing so I shall follow the argument presented by Sullivan in her article in the *Bulletin* in 1974. She outlines the popularly held position as follows:

Psychology to be a science, should, like other sciences, deal with data provided by the senses. Mental events are not in this category and therefore are not a fit subject for scientific study. Data provided by the senses are given the attribute 'observable'; mental events are regarded as unobservable. Behaviour is the category of observable events which is regarded as falling in the field of psychology.

Sullivan then goes on to examine whether this difference is important from the scientific point of view.

Although the one public event can be observed by many and the one private event can be experienced by only one, it is not single events that science is interested in, but types of events which recur. In fact, in scientific experimentation, a lot of similar public events are observed by a lot of different observers, and a lot of similar private events would, if accepted as suitable subject-matter for psychology, also be experienced by a lot of different experiencers.

Verbal communication establishes the similarity in both cases. That the same public event has been observed by the several observers is only known by the agreement of their reports. The private event can only be observed by the person experiencing it, but if his report agrees with that of others in similar circumstances, then agreement of the reports should be able to be taken as a sign that the same or similar event has been experienced by each experiencer, just as in the case of a single public event observed by several observers.

A great deal of research in cognitive psychology depends on just this assumption.

Phenomenological or subjective data were once, of course, the stuff of experimental psychology. When I was a student at University College, London, much of the experimental laboratory work was of this nature. I

have looked up my old practical books of the mid-1940s, and I see that they contain an experiment on experience without awareness, another on the analysis of the act of choice, and a third on the assessment of mental imagery. During the 1950s, 1960s and 1970s, as we have already noted, psychology was defined, typically, as the scientific study of the behaviour of organisms. Hebb in his textbook of psychology, published in 1958, specifically states in the preface that psychology is fundamentally a biological and not a social science. Eysenck in his chapter in *Models of Man* (1980) links phenomenology with the idiographic literary approach of psychoanalysis and existentialism and 'other essentially non-scientific movements'.

Now Aristotle asserted that history is what Alcibiades did and suffered (Berlin, 1982). Psychology, in my view, is what people do and suffer, and their suffering can only be studied phenomenologically. This point of view, having been lurking in the wings for almost the whole of my professional life, is, I am glad to say, now emerging from the shadows. Gale, in the paper he gave as guest speaker at the London Conference (1983), insisted that psychophysicists should develop a general theory of human behaviour, acknowledging the complexity of the subject matter, studying psychological processes, recognizing the possible relevance to behaviour of the person's phenomenal experiences, and conducting experiments which sample real life rather than that contrived in the laboratory.

One of the most fruitful new psychological theories is the theory of psychological reversals by Apter and Smith (Apter, 1982). The theory sets out to integrate the mechanistic and the phenomenological traditions of psychology. In other words, it studies both man as an organism and man as a person. In doing so Smith and Apter recognize that any study of people must be concerned with both human experience and human behaviour. Reversal theory, then, is based on the view that behaviour cannot be completely understood without reference to the mental correlates of behaviour, including the subjective meaning which the behaviour has for the person who performs it. The theory is concerned particularly with the way the individual interprets his own motivations. Smith and Apter treat as one of the central problems of human behaviour that of trying to understand why so much of it is apparently unrelated to biological needs, or to survival. They point out that such behaviour includes not only the biological gratuitous or superfluous behaviour such as that associated with art, religion, sport, humour and entertainment, but also those forms of behaviour which seem to militate against the survival of the individual, such as sadistic or masochistic behaviour, vandalism and hooliganism, risk-taking, gambling, alcoholism, drug-taking, celibacy, martyrdom and suicide. The theory postulates the existence of a number of pairs of metamotivational states, or frames of mind, which determine certain general phenomenological characteristics of moti-

vation at any given time. One such pair is negativism and compliance. Reversal theory then describes how reversals can take place between one or other of a pair of such metamotivational states. This is not the place for me to provide a proper account of this theory, but I hope I have given you enough to indicate that it is firmly phenomenological in character. Indeed, Apter (1982) calls his book setting out the theory, 'an essay in structural phenomenology'. I believe reversal theory to be of considerable importance because it constitutes an attempt to integrate the study of human behaviour with the study of human experience.

#### Rare or one-off events

I propose at this point to introduce a short piece of autobiography to explain my interest in phenomenology, as this will also explain my interest in rare and anomalous events which I shall go on to discuss. I have already told you about my undergraduate training in introspective methods and in the phenomenological study of experience. Fairly soon after qualifying, I found myself as a trainee research and clinical psychologist at the Crichton Royal in Dumfries. Here I was exposed to the influence of one of a group of continental psychiatrists who had become refugees in Britain at the beginning of the war. They were interested in the psychology of testimony and particularly in the phenomenology of Karl Jaspers (1962). The lectures we received at that time from Mayer-Gross (1954) were very much to my taste, and gave me a permanent interest in a wide variety of anomalous experiential phenomena: unusual imagery and perception, hallucinations, hypnagogic phenomena and dreaming, relaxation and hypnotic states, dissociation and multiple personalities, altered states of consciousness, anomalies of memory and forgetting, reality and autistic thinking, fantasy, formal thought disorder and delusions, and meditation and transcendental states. These are aspects of abnormal psychology which have received scant attention in British textbooks. Abnormal behaviour and its physiological correlates have received plenty of attention, but psychologists have almost completely avoided any phenomenological approach. McKellar (1957, 1968, 1979) has been a notable exception. The only textbook known to me which deals specifically with abnormal psychology from the phenomenological standpoint is Reed's book on *The Psychology of Anomalous Experience*, published in 1972 and now out of print. It is only comparatively recently that an interest in altered states of consciousness has surfaced in our Society, notably reflected in a symposium on this topic at our annual conference in 1975. In a survey of papers and symposia given at our annual conferences since 1947, I find that less than two per cent of the papers and less than three per cent of the symposia were on topics that could remotely be called phenomenological and which still less could be included under the rubric of anomalous experience.

Nevertheless, anomalous experience and its associated behaviour may be important in developing psychological theory. The results of the Michelson-Morley experiment on the speed of light were unwelcome and anomalous to Newtonian physics, but were central to Einstein's special theory of relativity. Anomalous psychological observations, properly witnessed and reliably reported, may be of equal importance for psychology. As Kuhn (1962) has observed, it is the anomalous events which lead to paradigm shifts in scientific thinking.

Important observations, I am suggesting, may be both unexpected, unlooked for, rare or even unique. The problem is how to evaluate such records of these observations as we may be able to make. Their rarity or uniqueness — their 'one-off' character — makes replicability unlikely or impossible, and their spontaneity, unexpectedness and unpredictability makes adequate recording at the time extremely difficult. A supernova explosion can only be recorded if a telescope happens to be focused at the right time in the right part of the sky, although the event might be inferred from other phenomena observed subsequently. Yet rare psychological observations may have considerable importance for our knowledge of human experience and behaviour, and what we consider to be within the range of human abilities. I suppose the one unique event which was as 'one-off' as any event could be, was the reported resurrection of Christ: a psychological supernova, if ever there was one. Theologians regard the literal reality of this event as being central to the Christian faith. There is no question, they say, of its being regarded as a myth. It really did happen (Stannard, 1982). The miracles might be explained away, but not the resurrection. Nowadays, however, Christians are not so willing to believe that this event really happened simply because the Church says it did. Neither are they willing to believe this event really happened simply because the Bible says it did. However, since all we have is the evidence in the gospel stories, we have to look for internal evidence within the text itself. Does it ring true? Does it read like an account written by people who actually witnessed the event? It is claimed that the recorded descriptions of the Easter events in the New Testament show all the hallmarks of a genuine account of eyewitnesses. However, it is difficult to see how a one-off event *could* be established as having really happened unless there were quite extraordinary detailed and permanent records made at the time with a large number of independent and unbiased witnesses.

I now wish to consider unusual and anomalous events which, although rare, have been reported sufficiently often to merit attention. I shall choose as my example the levitations which are claimed to have happened in the presence of the medium D. D. Home (Jenkins, 1983). He was born in 1833 and died in 1886. This means that he was too early to be investigated by the Society for Psychical Research which was

founded in 1882. However, I have chosen him because, despite a long career and innumerable seances, Home was never seriously accused of fraud, no one was found who would admit to having been an accomplice in trickery, and no one was able to demonstrate how he achieved his effects. The movement of heavy tables would have needed massive preparation and apparatus. An additional bonus in selecting Home as my example is that an article appeared in our own *British Journal of Psychology* in 1953 by E. J. Dingwall, who discussed problems raised by reports of some of Home's table levitations. Dr Dingwall is an anthropologist who, for a period, was research officer for the Society for Psychical Research. He is presently something of a sceptic as far as paranormal psychology is concerned, but he does not extend his scepticism to D. D. Home. His article in the BJP describes an eyewitness account of table levitation. The witness is one Lord Lindsay, later to become the Earl of Crawford, who was present at the seance with five other people and Home himself. Lighting conditions were by oil lamps, but reported to be good. The table was large and heavy, big enough for seven people to sit round in comfort. All witnesses agreed that the events occurred as Lord Lindsay recorded them. This is how Dingwall presents Lord Lindsay's account.

At the third sitting, at which the writer himself was present, he declined to sit with the circle at the table as he thought that if he were outside he might not be influenced by any suggestions, since he was aware of the explanation which maintained that the phenomena were subjective.

After the rest of the circle had taken their places, with Home sitting between Mrs Baker and Miss Crossman, the usual manifestations occurred. Taps began almost at once on the under side of the table, and then the table began to vibrate, and then the chairs; and then the floor and then the whole room trembled and shook, while the china rattled on the table at the further end of the room!

On looking under the table Lord Lindsay saw nothing except the feet of the persons present (and presumably the central leg which supported it). But immediately afterwards the table 'rose suddenly straight up to the height of four feet — remained suspended in the air for about half a minute, swaying about in different directions — I again looked under the table, while it was moving about, but there was nothing visible and then came down again quite gently . . .'

This account is very similar to other people's accounts of Home's exhibitions of psychokinesis. Dingwall points out that this account was written down by an eyewitness immediately after the event, that the lighting was good, and that the witness was reputable. He then examines possible explanations. It might have been that all the witnesses were hypnotically hallucinated. But if so, it was quite unlike any known hypnotic process. There was no period of induction, and several people were affected. It might have been the result of collusion between Home and all the

witnesses, that is: it never happened and they were all telling lies. But if this is so, and if Home used this method throughout his career, it is odd that not a single witness has come forward to testify to such collusion. It might have been elaborate conjuring on the part of Home. But this would have entailed very detailed and careful preparation with pulleys, ropes and so forth. Of course with adequate preparation, professional magicians could do all that Home did. The question is, however: 'Is this how Home did it?' The great difficulty in assessing evidence of this sort is that it is extremely rare, and no comparable performer has appeared since Home.

I want now to consider possibilities of inquiring into abnormal experiences which, although rare, are reported sufficiently often by large numbers of people. I refer to those which Maslow called *peak experiences* (1962). In an earlier day they would probably have been called *religious experiences* although nowadays they are often experienced in a secular setting. William James himself, of course, published a classic study of these experiences in his *Varieties of Religious Experience* based on his Gifford lectures (1902). He thought it quite proper to examine a large number of verbal reports of these experiences and to discern common features which distinguished them from other kinds of experience. A number of people have made similar collections of recorded experiences and a pattern is beginning to emerge (Starbuck, 1899; Laski, 1961; Hetherington, 1975; Beardsworth, 1977; Hay, 1982). They are very rare, occurring perhaps once or twice in a person's lifetime. They seem to occur at moments of great mental concentration. They involve some change in the time-sense. People often report a timelessness or out of time quality to the experience: although momentary in clock time, it seems to last for eternity. There is usually an experience of great meaning and significance, although what that meaning is in verbal terms is always difficult to express. William James himself had noted the ineffability of such experiences. Perhaps most characteristic of all, there is an experience of being part of a larger whole: of the disappearance of personal boundaries. These experiences are usually, but not always, accorded extreme importance by those who undergo them. In many cases they greatly alter subsequent behaviour. Maslow has suggested that they are sometimes markers of the achievement of self-actualization. At all events, they surely merit close study by psychologists.

### Conclusion

It seems to me, therefore, that psychology is not adequately described as a biological science. The methods of the natural sciences can at best only yield partial knowledge about why people behave in the ways they do. There has also to be a phenomeno-

logical approach if we are to make any sense of human behaviour. It might be helpful to employ what has come to be known as the hermeneutic method. This term is derived from the name Hermes, the messenger god who is associated with transmuting what is beyond human understanding into a shape amenable to human comprehension. Hermeneutics then, sets out to bring the unintelligible to understanding. R. S. Steele (1982) has a chapter in a book on Freud and Jung, which is devoted to 'hermeneutics and interpretive psychology'. What follows is taken from this chapter.

The hermeneutic task is to aid communication in order to facilitate understanding. Communication occurs between subjects, not objects. Human lives need not be seen as objects; all of them are resources of communication carrying meaning for anyone interested in them . . . if . . . people are viewed as carriers of meaning then they are subjects capable of answering our questions.

The methodological goals of the interpretive sciences are radically different from those of the natural sciences. The latter seek to explain by manipulation and control. Hermeneutics tries to understand through participation and openness . . . Natural science with its objective method, doctrine of replication, and controlled situations which require causal explanation determines what we will find: objects in causal relationships which are stable over repeated observations . . . The method of the interpretive sciences is consonant with the hermeneutic view of understanding experience. The method is simple: it is dialogue. We come to understanding, we establish meaning . . . by the age-old dialectic method of question and answer. Meaning is not something locked up in the solitary person: it is established through language in relationship to people.

My thesis is then, that since psychology is part a natural science, part a social science and part an interpretive science, we need to develop our *own* methods of inquiry, adequate for the study of human beings as organisms, human beings as members of social organizations and human beings as people with whom we have to enter into dialogue. Perhaps we need William James (1892) to remind us that 'the natural-science assumptions with which we started are provisional and revisable things'.

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