

CRITICISM AND CONTROVERSIES IN PARAPSYCHOLOGY
- AN OVERVIEW

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"When the main line of the history of parapsychological research is considered, as it tries to achieve a place among the recognized sciences, it immediately becomes apparent that the endless controversies and discussions associated with this enterprise which are still continuing to this day, are of a different nature than the usual conflicts in science. Apparently this debate touches upon more vital and essential values and issues than is normally found in polemics, for instance, about the acceptability or possible consequences of a technological finding or the introduction of a new chemical product. Especially the occasionally bitter tone of the discussions and the fact that they often become personal and violate common sense, is indicative of the inflammatory character of the issue" (Servadio, 1958, 1). This statement from the Italian psychoanalyst and parapsychologist Emilio Servadio highlights the peculiar position which these controversies about parapsychology and the critical attitudes towards its scientific nature occupy. There is no doubt that the problem of the existence of paranormal phenomena can be considered as one of the most controversial research topics in the history of science. It is even possible to view the history of parapsychology as the history of its controversies. Unlike in other scientific disciplines these controversies are not so much related to the

interpretation of certain phenomena but refer to the very existence of the phenomena themselves. As will be demonstrated this is the reason why even the most competent judges do not agree about the essentials of paranormal research and reach different conclusions.

First of all even the question of competence is in dispute. Who is entitled to be considered as a 'parapsychologist', and vice versa, who is allowed to act as a critic in this area? It is not difficult to see that a homogeneous group of parapsychologists characterized by certain qualifications does not exist. The necessity of a curriculum preferably on an academic level and of professionalisation is well recognized (Shapin and Coly, 1976; Johnson, 1977); but without an organisational basis, financial support, and the corresponding acceptance by the scientific community, realisation is only possible on a limited scale. In short, there are no authorities in parapsychology in the sense of representatives of an accepted body of opinions, who are supported by most scientists involved. At best one can say that there are 'experts' although in this context the meaning of this term remains uncertain. In an instructive discussion about "areas of agreement between the parapsychologist and the skeptic", R.A. McConnell, himself an active psi-researcher, argues "unless you are willing and able to spend years training yourself in psychology, physics, and in the sociology of science, you cannot make a competent decision about the quality and conclusiveness of the experimental evidence for parapsychological phenomena" (McConnell, 1976, 304). Judged on such criteria the United States can perhaps count on "two dozen reasonably qualified and active research workers in parapsychology (McConnell, 1976, 308).

The same applies to the qualifications of the critics. The often applied dichotomy - parapsychologists believe in ESP, critics or skeptics do not - is simplistic as well as wrong. From many examples in the literature it can be demonstrated that parapsychologists are the most severe and competent critics of themselves and their own research. Just consider the names of Besterman, Dingwall, Hodgson, W.F. Prince, the contributions published in Murchison (1927), Angoff and Shapin (1971) and the discussion by John Beloff (1972, 1975) about the skeptic's position - just to name a few. The role of the self-appointed sceptic or professional debunker may seem prestigious in the public opinion (note 1) but often lacks factually based or

logically acceptable argumentation (see for example Bender, 1964; Buchel, 1976; Bauer and von Lucadou, 1980).

Another controversy concerns the boundaries of the field. If the paradigms of the Rhinean School (Nilsson, 1975, 1976) which for forty years dominated research are accepted, then the only firmly settled parapsychological subject matter consists of extrasensory perception (ESP) and psychokinesis (PK). This explains why Rhine considered the "occult wave" (Bender, 1976, 7) which became prominent in the Western countries during the seventies and included acupuncture, Kirlian photography and astrology, as very dangerous for the image of parapsychology as an experimental science. Particularly because many parapsychologists appeared to take a positive attitude towards such pop-topics (to which can also be added ufology, Bermuda-triangle or pyramid-forces), Rhine warned that "Parapsychologists had better give some thought to the fact that their kind of psi is no longer nearly as securely under their own social control as in the past. The time has come when we who work with psi need to decide whether we really do know where we belong and just what our territory is. - - - Is there any other experimental science that rests on such a slight basis of uniformity and standardization?" (Rhine, 1972, 175).

If Schmeidler's questionnaire study (Schmeidler, 1971) can be considered as representative then it appears that the members of the Parapsychological Association at least concur that ESP is a proven phenomenon and that there is no reason to provide again and again new evidence (this may perhaps be too optimistic; see for example the recent enquiry among P.A. members by McConnel and Clark, 1980). But apart from that the opinions among leading parapsychologists are evenly divided as to which psi modality should be empirically studied. Rhine (1974b) for instance takes the position - criticized by Thouless (1973) - that a large number of parapsychological research topics, such as out-of-the-body experiences, the survival problem, retrocognition, psychometry and even telepathy are basically insolvable problems which cannot be studied empirically as it is impossible to eliminate clairvoyance as potential alternative hypothesis. It is safe to assume that this dilemma is not simply a semantic one. It reflects principally different theoretical models which have of course consequences for the empirical testability of the hypotheses derived therefrom. This picture of parapsychological diversity makes it rather easy for critics to compile from the literature a collection of widely

varying statements and opinions (see for a recent example Alcock's coup de grace for parapsychology and Palmer's articulate rejoinder; Alcock, 1981; Palmer, 1983) which can be used to paint a livid picture of the most absurd consequences from research in this area, for instance, from a juridical point of view.

An overview of the history of a hundred years of research in parapsychology allows us to detect rather typical forms of pro and contra argumentation which influence the structure of the controversies in a remarkable way (see for instance the overviews given by Nicol, 1956; Crumbaugh, 1966; Dommeyer, 1966; Ransom, 1971). The address in 1882 by the first president of the British Society for Psychical Research (S.P.R.), Henry Sidgwick, is typical of how the pioneers of this research took it for granted how they would meet the objections of the scientific world. Sidgwick speaks of 'sufficient evidence', that is, "evidence that will convince the scientific world" (Sidgwick, 1882a, 9). Sidgwick elaborates on this in his second presidential address: "...if they will not yield to half-a-dozen decisive experiments by investigators of trained intelligence and hitherto unquestioned probity, let us try to give them half-a-dozen more recorded by other witnesses; if a dozen will not do, let us try to give them a score; if a score will not do, let us make up the tale to fifty" (Sidgwick, 1882b, 67). Thus the opposition should be gradually silenced and recognition of parapsychology enforced (note 2) by applying this principle of cumulative evidence, i.e. by adding more and more proof for the existence of ESP.

Closely related is the principle of reputable testimony: it became more or less standard procedure especially for sessions with physical mediums to involve large numbers of personalities with established reputation as observers in order to use their testimonies regarding the genuineness of the phenomena in question to change the opinion of the scientific community. However, the controversy around the 'physical mediumship' which broke out between the two world wars primarily about the work of Schrenck-Notzing could not be resolved that way. The positions of both adherents and opponents remained basically irreconcilable (note 3).

When in the beginning of the thirties J.B. Rhine came forward with his experimental-statistical ESP research it seemed that this would create a change in the discussion. For the first time a

number of independent researchers accepted a common methodology and terminology and applied it to a specified problem. It was also the first time that the scientific community was challenged by an excess of experimental results achieved under laboratory conditions by conventional methods and with unselected subjects.

The reaction of the scientific community to the proposed methodology of card experiments was accordingly animated. Between 1934 and 1940 about 60 critical publications by 40 authors appeared, mostly in the psychological literature, dealing with nearly every aspect of the experimental conditions and statistical evaluation (Honorton, 1975a). Most of the criticisms raised can be classified in three groups (Pope and Pratt, 1942). The first group concerns the mathematical-statistical assumptions of the evaluation techniques which were applied; the second the validity of the experimental procedures and the third the logic of the interpretation of the results in terms of the ESP hypothesis. The overview published in 1940 (Pratt et al, 1940) of all the main experimental research from 1882 till 1940 - the 'bible' of experimental parapsychology - lists and discusses 35 alternative hypotheses. To these belong erroneous statistical methods, improper selection of data, insufficient shuffling of target decks, optional stopping, unconsciously motivated errors in recording and checking target and response sequences, insufficiently eliminated sensory cues (unconscious whispering, marked cards) and finally incompetence and gullibility of the experimenters. Of the 142 publications from the previous 60 years only six turned out to be sufficiently robust to withstand all these objections, thus according to the authors providing valid evidence for paranormal cognition. These six are all experiments carried out in the Duke laboratory since 1927.

By applying such objective procedure Rhine and his collaborators to a large extent succeeded in silencing the main opposition by academic psychologists. Not in the least because they adapted their research in accordance with valid criticisms. Hence although the reality of ESP was not generally accepted, in the beginning of the forties at least agreement existed about what a proper ESP experiment should look like (Honorton, 1975b). When in 1943 the research program of the Duke laboratory expanded to include research in psychokinesis (influencing the throwing of dice) the criticisms remained restricted to those offered by the British parapsychologists who were mainly motivated by their lack of

success in repeating these experiments. It was not before 1962 that the American psychologist Edward Girden published a fundamentally critical evaluation of 200 PK experiments and concluded that "evidence of PK as psychological phenomenon is totally lacking. And this deficiency will persist until the effect is produced in the presence of a specified psychological variable, and the effect does not appear in its absence" (Girden, 1962, 387).

Pratt (1964) objected that Girden exaggerated the defects of the experiments under consideration (for instance lack of strict experimental procedures, bad control of dice bias, improper evaluation of inhomogeneous data) and that he had ignored the experiments to which such objections were not applicable. Further information about the complexity of the problems relevant to the PK controversy and the differences between the opinions of the parapsychologists involved can be obtained by consulting the relevant literature (see Girden, Murphy, Beloff, Eisenbud, Flew, Rush, Schmeidler, Thouless, 1964).

After the successful completion of the "ESP controversy" in the sense that the opposition became silent at the end of the thirties Rhine took it for granted that only time was needed before parapsychology would be fully integrated in the psychological sciences (Nilsson, 1975, 1976). But this hope proved futile. In the next 15 years the 'establishment science' (Honorton, 1975b) took hardly notice of parapsychological research. The active confrontation failed to materialize. It was not before the middle of the fifties that the controversy erupted again. The immediate cause were two publications in the perhaps most influential interdisciplinary scientific journals 'Nature' and 'Science'. The Oxford logician G. Spencer Brown (1953) gave a new twist to the 'statistical controversy' in parapsychology by directing his criticism not against technical details of application of statistical procedures, but against the basic assumptions of probability theory itself. He disputed the common procedure in parapsychology to infer, from the 'improbability' of the result of the statistical evaluation the existence of ESP, despite the lack of repeatability and of demonstrable patterns in the phenomena. Such a naive way to infer a 'cause' from 'significance' was also criticized by the German mathematician Tornier. He argued that statistics is only a research tool and can never itself provide 'proof' (Tornier, 1959, 115) (note 4). Thornier's criticism was

discussed at length in German parapsychology; the various positions concerning this controversy can be found in Bender (1959), Mischo (1974), Krenzel and Liese (1978), and especially Timm (1979).

The most radical and perhaps most influential criticism so far directed against parapsychology up to that date was offered in an extensive discussion by the chemist George Price in his article 'Science and the supernatural', published as leading article in *Science* (Price, 1955). He started by admitting that the opposition of parapsychology has been practically silenced by an impressive number of careful experiments and intelligent argumentation. However, in view of the fact that the existence of ESP must be considered in conflict with current theories in science, Price was forced to conclude that all significant results in parapsychology which cannot be explained by faulty experimental procedures, statistical errors or unconscious use of sensory cues, had to be due to "deliberate fraud or mildly abnormal mental conditions" (Price, 1955, 360). Deliberate fraud by the investigator as an alternative for psi- under such a premise Price discussed a number of scenarios how fraud could have lead to the very significant results obtained by the British mathematician Soal (Soal and Bateman, 1954) even though he did not provide factual evidence. Apart from the reactions of the scientists who were personally attacked (Rhine, 1956; Soal, 1956) especially Meehl and Scriven (1956) drew attention to two untenable presuppositions in Price's argumentation: Firstly that ESP is in conflict with modern science and secondly that modern science in its present shape should be correct and complete. In any case, seven years later Price withdrew his suspicion of Rhine and Soal as frauds as 'highly unfair' (Price, 1972, 356) (note 5). Nevertheless both arguments, the a priori improbability of ESP and the possibility of fraud on the part of the experimenter, were taken up and extended in the book by the British psychologist C.E.M. Hansel, published in 1966: 'ESP, a scientific evaluation'. The non-parapsychological world seemed to consider this book as the final word to be wasted on the subject (see Slater, 1968). According to Hansel the process investigated in parapsychology is: "both hypothetical and a priori extremely unlikely" (Hansel, 1966, 17). Any possible known cause of the results, including conspiracy by the participants of the experiment to cheating, is far more likely to be responsible for it than the hypothetical process (ESP) under consideration.

In the analysis of four experiments, of which three belong to the 'classical' conclusive ESP experiments: the Pearce-Pratt series and the Pratt-Woodruff experiment both from the early Duke period and Soal's experiments with Mrs Stewart and Basil Shackleton as well as the Soal-Bowden experiments with three Welsh schoolchildren, Hansel demonstrates with remarkable ingenuity how fraud could have been committed. According to Hansel this is sufficient to question any positive claim for convincing evidence of ESP (Hansel, 1966, 241). It is hardly possible to counter such charges of fraud, at least not as long as independent confirmation for the findings are lacking. But in the case of parapsychology the argument of fraud is not more plausible than in the case of other scientists (see for instance McConnell, 1975). The scandal around Rhine's successor, W.J. Levy, which erupted in 1974 and which resulted in world-wide news comments, demonstrates particularly the essential point that Levy's fraud was detected by his colleagues and that Rhine himself made it public (Rhine, 1974c) (note 6).

Hansel's critical approach to parapsychology was heavily criticised by pointing out the apparent bias of his arguments and on account of many factual errors and inaccuracies which makes it doubtful whether this work can be called a 'scientific' evaluation of psi (note 7). Nevertheless Hansel's penetrating criticism highlights a number of fundamental problems. The opinions regarding the importance of these problems differ in the parapsychological community.

In the first place one can consider in this connection the problem of repeatability (see especially Crumbaugh, 1966; and - more recently - the thorough discussion by Hovelmann, 1983). At a minimum it can be said that everybody agrees that parapsychology knows repeatable experiments but not repeatable results. According to Beloff (1972) an experiment with repeatable results can be considered as the description of an experimental procedure which, when applied by competent researchers, "must work at least 50 percent of time and, even more important, must not depend on the availability of a particular individual as subject" (Beloff, 1972, 198). But the opinions under parapsychologists about this matter varies. For instance Beloff in agreement with Crumbaugh (1966, 526) and Dommeyer (1966) concludes that parapsychological results will only be generally accepted by the scientific community when at least one repeatable effect can be demonstrated. Proposals to

modify the concept of repeatability, along the lines suggested by LeShan (1966) or Murphy (1971), make it dependent on the specific character of the research object. For parapsychology this would imply that an 'intrasubjective' repeatability exists in the sense, that over several years a subject achieves positive results with different investigators. An example is the 'focussing effect' of Pavel Stepanek (Keil, 1977). The sort of 'internal repeatability' found in experiments of the Maimonides group with telepathic dream induction (Ullman et al, 1973) or in the phenomenology of paranormal metal bending (Hasted, 1977) could also be rated as such. At any rate, the demand for repeatability remains a fundamental methodological problem in parapsychology. But this holds not only for parapsychology but also for the behavioral sciences. For instance, in psychology the results are similarly characterised by widespread inconsistencies, by non-repeatability and non-predictability (Maschewsky 1977, 212). Regarding repeatability Honorton (1975b) feels that compared to certain fields in psychology parapsychology is even in a better state.

The attitude towards the repeatability issue and its epistemological foundations has far reaching consequences for a number of related 'subproblems' which can only be shortly mentioned here. For instance the problem of selective reporting of only positive results could lead to a distorted picture of the actual achievements of research (for this problem see the discussion between Rhine, 1975 and Beloff et al, 1976). Another related problem is the empirical verification of hypotheses. By combining in an uncritical way different 'effects', like psi-missing decline-effects or influence of experimenter bias, it becomes in principle possible to interpret each outcome of a parapsychological experiment in support of the psi hypothesis. The danger of such a strategy which ensures the immunity of the psi interpretation against nearly all criticism is reinforced by the generally applied terminology in parapsychology. For instance it is asserted that a certain phenomenon can be 'explained by ESP' (see Mundle, 1971, 20). Such an expression neglects that the ESP concept has no explanatory power but should be considered merely a verbal convention to label a certain as yet unexplained group of phenomena (more about this in Staub, 1978). The frequently discussed observation that psi often fails to appear when skeptical observers (for instance magicians) or researchers are involved can be interpreted, from a psychological point of view, as an indication for the dependency of psi phenomena on complex

psychological conditions, a delicate affective field (Bender, 1976) between the participants in a psi experiment. In other words, there may be quite a number of unknown conditions which requires new strategies for dealing with.

A further problem for the controversial status of parapsychology, indirectly related to the issue of repeatability but of exceeding importance, concerns the remarkable erosion of evidence. This 'evaporative effect' as Scriven called it (quoted by Eisenbud, 1963, 251) means that some initially so convincing results of parapsychological research seem to lose their strength with later reevaluations. When time passes even the researcher will eventually become affected by the destructive influence of doubt. John Beloff (1972) as president of the Parapsychological Association gave a lively illustration of this 'genesis of doubt' (Rogo, 1977) with examples of prominent parapsychologists (note 8). The 'will to believe' of parapsychologists as assumed by the skeptics seems more like a 'will to disbelieve' their own experiments and observations. This "principle of retroactive dissonance" (B. Inglis) can be nicely demonstrated by for instance the famous S.P.R. investigations of Eusapio Palladino in 1908 (see Rogo, 1977). The 'erosion of evidence' is one of the most stable traits in the history of parapsychology. Nearly every 'classical case', every 'conclusive experiment' has been subjected to this 'test of time', the process of re-evaluation based on new evidence and re-interpretation viewed from a different perspective. This reconstruction of evidential material often accompanied with much controversy can only be touched upon here; it fills virtually thousands of pages as the history of psychical research (see for example the Proceedings of the SPR) makes it abundantly clear (see especially Inglis, 1977). The disputes about the genuineness of William Crookes' experiments with D.D. Home and the 'materialisation' medium Florence Cook last already more than a hundred years (Medhurst and Goldney, 1964; Medhurst, 1972). Trevor H. Hall (1962) for instance tries to prove in his much debated book 'The Spiritualists' that Crookes was Florence's lover and helped her to cheat during sessions (see the discussion between Stevenson, 1963, 1964 and Hall, 1964a). Especially Hall's investigations who like a detective tries to detect 'weak spots' in the old S.P.R. experiments (see Campbell and Hall, 1968) give constantly fresh impetus to the historically oriented controversies, as in the case of Hall's book on one of the founders of the S.P.R., Edmund Gurney. According to Hall (1964b)

Gurney withheld indications for fraud and later committed suicide (for a detailed critical appraisal see Nicol, 1966, and Hall's rejoinder, 1968). So the controversies continue (for a recent example see Brandon, 1983). The special studies of famous cases, like Tietze's (1973) study of Margery, Rogo's (1975) study on Palladino or Anita Gregory's (1977) study of Rudi Schneider, demonstrates the typical pattern of the scientific controversies in parapsychology. To this pattern belongs the emotional polarisation of the antagonists, the competence claims, the committees to evaluate the 'conclusive evidence', the offering of awards, etc..

The 'erosion of evidence' affects also those experiments which for a long time were considered as the most solid data of parapsychology. The tragic irony of the famous Soal-Shackleton series of 1941-1943 on precognitive telepathy (Soal and Bateman, 1954) with its experimental design aimed at eliminating possible experimenter fraud but to which Soal himself gave rise to suspicion (the only afterwards admitted loss of the original protocols; use of the random tables differs from as reported; the allegation by Grete Albert that she saw Soal changing figures; see for this controversy Scott et al, 1974; Scott and Haskell, 1975; Markwick, 1978) (note 9). In the same vein for already decennia the Pratt-Woodruff experiment constitutes a platform for accusations of fraud by critics (Pratt, 1976). These examples teach us at least one lesson. The conclusive experiment convincing every sceptic of the the existence of psi does not exist. It is an illusion to make the break-through to scientific recognition depending on the 'perfect' experiment. In his review of Hansel's book Stevenson wrote the remarkable sentence: "If we give up the idea of a fraud-proof experiment we ought also to give up the idea that our experiments are in any way conclusive or can be regarded as proof" (Stevenson, 1967, 263f). He rather argues in favor of some agreed-upon standards developed in cooperation between researchers in parapsychology and critics inside and outside the field for the evaluation of a specific experiment (Stevenson and Roll, 1966). The apparent impossibility of the 'decisive experiment' is confirmed by Nicol's observation that even "psychical researchers of undoubted authority do not agree among themselves as to whether some of the leading experiments are conclusive evidence for paranormality" (Nicol, 1956, 29).

Considering such a situation Coover's 'fagot-theory' (Coover,

1927, 233) offers some perspective. Although each piece of evidence, each branch so-to-say, can be criticised and in principal be refuted, together they constitute a strong bundle of evidence. On the other hand it is equally possible to defend a 'chain' model (Beloff, 1976, 93). The chain of evidence for ESP is as strong as its weakest link. It seems therefore unavoidable that parapsychologists often apply subjective criteria in weighing the evidence. For Rhine (1974a, 113), for instance, the unexpected post hoc discoveries of 'fingerprints of psi' in the card guessing and dice experiments, the decline effects and U-curves, constitutes convincing evidence. Surely much misunderstanding would be avoided if subjective evidence could be kept strictly separated from compelling scientific evidence, although it is questionable especially in the case of parapsychology whether that is possible. Is not personal motivation, the experimenter effect and a positive attitude towards psi an essential condition for eliciting psi?

One group of critics considers the answer to this question as the very solution of the mystery of psi. According to this argumentation first offered by Moll (1929), further developed by Gubisch (1961) and taken up by Prokop and Wimmer (1976) the gullible parapsychologists live in a joyful anticipation of the occult and cover their superstitions with a pseudo-scientific cloak. Thus the whole field of parapsychology only exists because of the perhaps psychologically abnormal motivation of parapsychologists. Especially the German critic Wilhelm Gubisch reduces the whole problem of ESP to the 'psychological structure of the believers in the occult' (Gubisch, 1961, 98). As a pseudo-clairvoyant in his 'experimental demonstration of ESP' he collected from the general public valuable material about the gullibility and the will to believe. But this very example can also be used to demonstrate how Gubisch's motivation as a debunker, analysed according to the principles of research in social perception, distorted the way of handling his data (see for examples Neuhausler, 1964). But despite the intensity of his negative attitude at least Gubisch demonstrates his awareness of the possible consequences of the paranormal (Bender, 1964). With others the problem is reduced to one of a purely psychological nature (see for instance Wimmer, 1973). Already W.F. Prince (1930) observed that even when scientifically educated persons enter the field parapsychology and pass the 'enchanted boundary' they suddenly appear to become one-sided in the information they

collect and to ignore arguments. In short, they react so irrational in their opposition as would be unthinkable inside their own field. Apparently firmly rooted defenses against the acceptance of the paranormal lie behind the rational discussions. Servadio (1958) when interpreting this defense proposes a psychodynamically based 'disbelieve reaction' to parapsychological phenomena. In Eisenbud's (1963; 1966) speculations the defense against psi is part of nature itself, and even parapsychologists are prevented from gaining experimental control over these powers by an 'unconscious sabotage' directed against their own efforts. LeShan (1966) applies Festinger's model of cognitive dissonance. The psychological motivation to reject paranormal phenomena originates in their very observation, which is in conflict with the familiar social-cultural context and thus creates a threatening conflict. An explanation is attempted here by means of depth and social psychological concepts, namely that neither the amount nor the scientific quality of evidence for parapsychological phenomena contributes to its social acceptance. This becomes even more clear when considered from the point of view of the history and sociology of science. Here the controversial status of parapsychological research becomes the prime example of the general problem in the development of science, i.e. that the acceptance of new phenomena and theories is hardly influenced by the objective state of evidence (Ferrera, 1977). Especially the sociological study of the parapsychological community can serve to demonstrate the close association between the social organization of an innovative group and the reaction of the established sciences (Allison, 1973).

Among parapsychologists McConnell (1966) has been the first to interpret the controversial situation of parapsychology in terms of Thomas Kuhn's (1962) influential model for the development of sciences. In this model parapsychological data become 'anomalies' which are in conflict with the currently dominating 'paradigms' of the natural and social sciences and consequently provoke opposition. The picture of modern parapsychology - uncoordinated and random observations, conflicting experimental results, the lack of well-defined concepts, of generally accepted working hypotheses and related theories, the desintegration into competing schools, the emotionality of the controversies - are also the features of an immature preparadigmatic phase of science awaiting an 'Einstein of parapsychology' (Pratt, 1974) to guide the field into the realm of accepted sciences. To what extent this

interpretation in terms of Kuhn's model is perhaps too optimistic or even misleading, is in itself the subject of current discussion (Shapin and Coly, 1977). But undoubtedly such 'metaperspectives' are of great value to determine the position of a 'protoscience'. Parapsychology as a study object for a 'relativistic sociology of science' (Rao, 1977) demonstrates the extent to which the scientific acceptability depends on the concensus of a group relying upon changing historical criteria. This further relieves parapsychology from concentrating on the existential question of the 'yes' or 'no' of her phenomena, of the concept of psi, again and again newly discovered and accepted by one group of people and then rejected and buried by others. In short, in this perspective the conflict around parapsychology becomes the touchstone for hidden and anthropological assumptions in our scientific worldview and research methodology. This too constitutes a challenge of parapsychology.

POSTSCRIPT

This slightly revised overview was written in 1977 and first published in German in the 15 volume "Kindler's Psychologie des 20. Jahrhunderts". It was primarily intended for psychologists and other educated people, who can be assumed to be rather ignorant about parapsychology and who probably have never heard of the 'European Journal of Parapsychology' and other professional journals in our field. Because of lack of space the topic had to be discussed in relatively few pages and therefore important aspects of the subject of the paper sometimes could only be touched upon.

One thing is for certain, even in 1984: The psi controversy is still with us. However, it appears that in the last years a start has been made towards a more rational and fair dialogue between proponents and critics of the paranormal (among the latter various representatives of CSISCOP). K.R. Rao, for instance, organized as part of the PA Convention in 1981 a symposium entitled 'Parapsychology and its critics: Implications for philosophy and sociology of science', in which a number of recently published critical views on parapsychology (Girden, Diaconis, Moss and Butler, Gibson, Kurtz) were discussed. Similarly at the Centenary-Jubilee Conference (SPR 1882-1982 and PA 1957-1982) a symposium was held, entitled "The case for skepticism", in which

among others C. Scott, S. Blackmore, P.H. Hoebens and R. Hyman took part. The 'Zetetic Scholar', edited by sociologist M. Truzzi has developed in the last few years into one of the best sources of information about criticism of research in parapsychology, with contributions from insiders and outsiders. Prominent are the 'Major Dialogues' between parapsychologists and skeptics (see especially Hyman, Beloff, Westrum, Hovelmann). In addition, a number of books taking a critical view of parapsychology have recently been published, among them the second edition of Hansel's book but also books by Alcock, Marks and Kammann, and Abell and Singer, which evoked several extensive evaluations from the parapsychological community.

NOTES

1. Symptomatic for this is the behavior of some members of the Committee for the Scientific Investigation of Claims of the Paranormal (CSICOP) founded in 1976 with which the best-known critics of parapsychology like Gardner, Hansel, Hyman and Randi are associated. Before issuing their own journal 'The Sceptical Inquirer' the 'Humanist' published by the philosopher Paul Kurtz was the mouthpiece of the CSICOP. The pretention of a rational evaluation of paranormal phenomena and the applied methodology has evoked some sharp criticisms by parapsychologists, see for instance Rockwell et al (1978) and Kurz et al (1978).

2. From the point of view of the sociology of sciences it certainly would be rewarding to compare the various presidential addresses of the S.P.R. in order to study the development in history of what is considered as 'established parapsychological knowledge', how that knowledge was acquired and the progress parapsychology has made regarding scientific recognition.

3. The controversy is most clearly presented in the 'Drei-Manner Buch' (three-men book) of Gulat-Wellenburg, v. Klinckowstroem, Rosenbusch (1925), the 'Sieben-Manner Buch' (seven-men book) published by Schrenck-Notzing (1926), and the subsequent discussions in the 'Zeitschrift fur Parapsychologie' and the 'Zeitschrift fur Kritischen Okkultismus'. An evaluation of the opposite views is presented by the Swiss psychiatrist Bleuler (1930).

4. Tornier expanded his criticism into 'Rhine - Fall of the parapsychologists', which was met with approval from critics like Prokop and Wimmer (1976, 122). The decisive mathematical rebuttal was, not considering Buchel (1975, 170), provided by Krengel and Liese (1978) and especially by Trimm (1979).

5. In an addition to the reprint of his article by French (1975, 373) Price states: "that I have myself become guilty of accepting and trying to follow (in a rather radical way) that strange system of beliefs that I accused Rhine and Soal of trying to promote, and consequently I now believe in much worse things than ESP".

6. The fundamental importance of the fraud and deceit argumentation is discussed by Muller (1980).

7. See for instance the critical evaluations by Honorton (1967), Stevenson (1967) and Medhurst (1968). Especially instructive are the positions of Eysenck, West, Beloff, Stevenson, the review by Slater (1968) and the discussion between Hansel and Slater (British Journal of Psychiatry, 114, 1968, 1471-1480; and *ibid* 115, 1968, 743-745).

8. Compare also the resigned attitude of W. James in his 'Final Impressions of a Psychical Researcher', of 1909, reprinted in Murphy and Ballou, 1969, 309-325, especially page 310.

9. Meanwhile the astute analysis by Betty Marwick (1978) leaves little doubt that Soal manipulated the target sequences of the Shackleton experiment. The motive for Soal's behavior remains unclear. However, undoubtedly experimental parapsychology lost an important piece of evidence and the adherents of the 'Psi=fraud' thesis scored another point.

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