

Deception by Subjects in Psi Research¹

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ABSTRACT: Parapsychology has long been tainted by the fraudulent behavior of a few of those claiming psychic abilities. Recently there has been renewed interest in studying persons who claim psi abilities even though they have been caught cheating. The issue of subject deception must be considered when evaluating most parapsychological studies; however, in certain research programs, attempted trickery is virtually certain, whereas in others it is unthinkable. When evaluating a report, a reader must consider the likelihood that deception may have been attempted, along with the effect this might have on the legitimacy of conclusions. This paper discusses two major approaches for providing safeguards against cheating. Subject-based control is an approach that focuses attention and resources on the subject. Target-based control is primarily concerned with adequately securing the target; this approach is the more easily implemented and provides the higher degree of security. A section is devoted to the special security problems with telepathy experiments. Designing sufficient controls requires some knowledge of magic. A survey of past presidents of the Parapsychological Association was conducted, revealing that they had little familiarity with conjuring. A discussion of the role of magicians is included. Recommendations are made for dealing with the problems of subject trickery.

Psychic occurrences have endured a poor reputation because of fraud by a few of those claiming psychic powers (e.g., Keene, 1976). The affiliation of psi and fraud is found all over the world; both Rose (1952) and Reichbart (1978) have cited a number of anthropologists who have reported observing simulated psychic events. This association has tainted the scientific research as well. For instance, Irwin (1987) has described how the reputation of a prominent medium hindered the acceptance of parapsychology in Australia. Palmer (1988, p. 109) recently wrote: "Psychic fraud . . . has been the single most important factor in damaging the reputation of parapsychology and retarding its growth." Even more serious, a number of researchers have endorsed tricksters as having genuine psychic powers.

The early Society for Psychical Research (SPR) instituted a policy of refusing to work with psychics and mediums who were known to have engaged in deceptive activity. Recently, Inglis (1984) has vehemently and

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bitterly denounced that policy, and a number of people seem to agree with him (e.g., Braude, 1986; Gregory, 1982). Several leaders of the field seem to agree. Beloff (1985) has pleaded with the skeptics to examine the Palladino mediumship. He has stated that Glenn Falkenstein deserves investigation (Beloff, 1984a). (Falkenstein is a well-known mentalist; for example, see Booth, 1984.) Presently Beloff (1988) is promoting the Margery mediumship. Recently Azuma and Stevenson (1987) have advocated further study of the notorious psychic surgeons.

This greater willingness to study (alleged) psychic functioning of reported frauds was exemplified at the 1986 Parapsychological Association (PA) convention, where three papers presented work with persons previously reported to have engaged in fraudulent activity (Egely & Vertesy, 1986³; Stewart, Roll, & Baumann, 1986⁴; Warren & Don, 1986⁵). This is not an isolated instance. As seen in the Table, every annual convention of the PA since 1980 has included papers reporting positive results from subjects who later admitted to or were reported as having used trickery at some point in their careers.

In spite of the research with tricksters, there has been rather little parapsychological literature (in either the journals or other major reference sources) dealing with the topic of subject deception. The *Handbook of Parapsychology* (Wolman, 1977) has no chapter devoted to the issue, nor is there any in the volumes of the *Advances in Parapsychological Research* series (Krippner, 1977a, 1978, 1982, 1984, 1987). Indeed it would seem that most researchers think that it is quite easy to rule out trickery and that the database of parapsychology has little contamination by fraud. For instance, Rhine (1974) commented: "Subject deception . . . has long since ceased to be a major issue" (p. 101). A statement by Beloff (1980) also seems to express this view: "An experiment in which it is possible for the subject to cheat in any way at all is, quite simply, an invalid experiment and no editor or referee who knew his business would allow such an experiment to be published" (p. 119). (This comment was made specifically in reference to work with Bill Delmore by Kanthamani and Kelly, which is discussed more fully later.) Chauvin (1980/1985) even disparages discussion of the topic.

The attitude of some parapsychologists stands in stark contrast to that of the skeptics. For instance, Kurtz's (1985) *A Skeptic's Handbook of Parapsychology* has five chapters explicitly dealing with issues concerning

³ Zoltan Vassy (personal communication, March 1987) reported observing Pavlita engage in trickery when demonstrating his device.

⁴ At the convention, Stewart did admit that Tina Resch had been observed in trickery. This was not directly stated in the written report. To the authors' credit, they did address the issue in their abstract in *Research in Parapsychology 1986* (Stewart et al., 1986).

⁵ At the convention, Warren and Don did admit that their subject was Olof Jonsson and that Cox (1974) had reported observing Jonsson engage in trickery.

Table
CLAIMED POSITIVE RESULTS FROM REPORTED TRICKSTERS IN PAPERS PRESENTED AT
PARAPSYCHOLOGICAL ASSOCIATION CONVENTIONS 1980-1988

Year	Reported Trickster	Paper or Other Presentation†	Source of Allegation
1980	Masuaki Kiyota	Kasahara et al. (1981)	Stevenson et al. (1985)
1981	Steve Shaw	Phillips & Shafer (1982)	Randi (1983b)
	Mike Edwards	Phillips & Shafer (1982)	Randi (1983b)
	SORRAT	Cox (informal workshop)*	Hansen & Broughton (1983)
1982	Steve Shaw	Thalbourne & Shafer (1983)	Randi (1983b)
	Mike Edwards	Shafer et al. (1983)	Randi (1983b)
	Eusapia Palladino	Cassirer (1983)	Carrington (1909)
	J. H.	Hasted et al. (1983)	Playfair & Grosse (1988)
	Rony M.	Berendt (1983)	Berendt (personal communication, October 12, 1988)
	SORRAT	Cox (informal workshop)*	Hansen & Broughton (1983)
	Thomás Coutinho	Montagno (informal workshop)*	Roll (Pulos, 1987, p. 107)
1983	SORRAT	Cox (1984b)	Hansen & Broughton (1983)
1984	Tina Resch	Roll (1984)*	Stewart et al. (1987)
1985	Tina Resch	Baumann et al. (1986)	Stewart et al. (1987)
1986	Tina Resch	Stewart et al. (1986)	Stewart et al. (1987)
	Robert Pavlita	Egely & Vertesy (1986)*	Vassy (see footnote 3)
	Olof Jonsson	Warren & Don (1987)	Cox (1974)
1987	Robert Pavlita	Egely & Vertesy (1988)	Vassy (see footnote 3)
	Olof Jonsson	Don et al. (1988a)	Cox (1974)
1988	Olof Jonsson	Don et al. (1988b)	Cox (1974)
	Susie Cottrell	McDonough et al. (1988)	Randi (1979)

Notes: † Unless otherwise noted, see *Research in Parapsychology (RIP)* for that year (year per first column of this table)

* Not in *RIP* but presented at convention

subject fraud. Further, nearly all major critics of psi research in the U.S. are well known within the conjuring fraternity (Hansen, 1985a, 1987a).

However, there is a growing realization that more attention needs to be paid to the possibility of subject cheating. At the 1983 and 1985 meetings

of the PA, roundtable discussions were conducted on the role of magicians in psi research, and the PA issued a statement calling for further collaboration with magicians ("PA Statement on Magicians," 1984). More critical evaluations are being made with regard to possibilities of subject deception (especially Akers, 1984, but also Alvarado, 1987; Hansen, 1985b; Hastings, 1977; Palmer, 1985, Chap. 6; and Stevenson, Pasricha, & Samararatne, 1988). Robert Morris (1985) in his Parapsychological Association presidential message said that "fraud detection and prevention is a rich, complex area of endeavor in itself; yet it is also very appropriately a part of the domain of parapsychology" (p. 3). Further, Morris (1982, 1986b) has started to outline major concerns in the simulation of psychic events. Another positive step has been the required class in magic for parapsychology students at JFK University. Loyd Auerbach (1983) has presented a workshop and prepared a reading list on magic for the American Society for Psychical Research.

Deceit is found in many human enterprises. Fascinating examples have been discussed by MacDougall (1940) in his book *Hoaxes*. Further, deception is not limited only to humans, but other species have displayed it as well (Mitchell & Thompson, 1986). It should be realized that parapsychologists are by no means the only scientists who must deal with subjects trying to deceive them. Psychologists often confront this problem. A number of psychological tests have been built in "lie scales" to detect faking, and there is considerable debate as to the effectiveness of such scales (e.g., McAnulty, Rappaport, & McAnulty, 1985). A quick glance at the index of *Psychological Abstracts* under the headings of "Faking" and "Malingering" will give an idea as to the extent of the problem. Hyman (1989) has reviewed research on the psychology of deception. Pollsters and others doing survey work must concern themselves with the validity of responses of those surveyed (e.g., Bachman & O'Malley, 1981; Bishop, Oldendick, Tuchfarber, & Bennett, 1980; Traugott & Katosh, 1979). Medical scientists confront similar problems in Munchausen's syndrome (O'Shea, McGennis, Cahill, & Falvey, 1984).

This paper is written primarily for research workers currently engaged in parapsychology. A newcomer may get the mistaken impression that the field is rife with fraud. This is not the case; in fact, trickery is an issue in a quite limited portion of the research. Nevertheless, this particular portion is usually highly visible, and much of the public (including outside scientists) associates parapsychology with this tainted work. Even though in the large bulk of psi studies concerns over deception are minor, the field as a whole shares the responsibility for the unsavory reputation. This problem is not a result of a few naive, overenthusiastic proponents.

The purpose of this paper is to discuss the salient issues concerning deception by subjects in parapsychological research. I will focus on experimental and quasi-experimental research and will not address issues of deception and fabrication in spontaneous psi reports (e.g., Ejvegaard & Johnson, 1981). My procedure will be to describe a few major conceptual

issues and then illustrate them with specific examples from the parapsychological literature. I will take the bulk of the examples from journal articles (rather than abstracts and conference proceedings) because they comprise the most important and respected data of psi research. Most of the cited abstracts and conference papers are included because they are recent and give an indication of the current state of the field. As suggested by Child (1987), I have endeavored to make my criticisms as specific as possible rather than allude to abstract issues. Some of my examples will be drawn from historical cases, but the bulk will be from the recent literature. A number of my comments and evaluations will be extremely negative; however, I hope that I have followed the suggestions made by Stevenson and Roll (1966) for providing useful criticism.

The topic of deception is quite vast. I hope that discussion of these issues will provide the reader some insight as to when fraud control is of paramount importance in psi research as well as when it is of little or no concern. Also, I will outline the characteristics of methods that are either more or less effective in dealing with fraud. I will restrict myself to the topic of subject cheating; the issue of experimenter fraud in science has been dealt with extensively elsewhere (e.g., Broad & Wade, 1982). Further, I will not discuss the speculations on the role that trickery might play in eliciting or disguising psi (e.g., Reichbart, 1978). Nor will I enter the debate as to whether researchers should or should not work with known cheaters. The term "magic" will be used synonymously with conjuring or trickery and not in the anthropological or occult senses.

The rest of the paper will deal with three major topics: the subjects, the nature of safeguards, and the role of magicians. When evaluating controls against fraud, one must first assess the likelihood of fraud being attempted. This depends on the nature of the persons studied; thus, a considerable section is devoted to the research subjects. In implementing controls against deception, there are several strategies one can take, and these are discussed in detail. Two primary approaches are identified: "subject-based control" and "target-based control." Some precautions needed in psi research necessitate having a background in magic or consulting with conjurers. Because such consulting has received virtually no useful treatment within the parapsychological literature, an extensive section is devoted to the matter.

SUBJECTS

In evaluating the controls against deceit, one needs to consider the likelihood that trickery might be attempted by the subjects. When one examines some parapsychological experiments, subject cheating is virtually unthinkable. In others, however, it is only reasonable to start with the assumption that fraud was attempted and make an evaluation accordingly. For most studies, the plausibility of attempted trickery lies somewhere between these two extremes.

There are several possible ways of assessing the likelihood of trickery. Many researchers seem to think that motivation is a deciding factor. Although it is certainly important, considering it alone may lead to false conclusions. There have been cases of well-respected individuals who perpetrated fraud for no readily apparent gain (e.g., see Dingwall, 1963; Feilding, 1905/1963, pp. 1–8; Podmore, 1897; Sidgwick, 1894). Motivations may often be quite hidden to the casual (or even close) observer. Further, tricksters will sometimes go to lengths nearly inconceivable to more ordinary persons; for instance, a number of magicians have died because of the risks they took (e.g., Robinson with White, 1986), and many card cheaters have risked being killed (DeArment, 1982). Another consideration in evaluating the likelihood of deception is the history and background of the subject (some, like Eusapia Palladino, seem to cheat at any opportunity). Still another is the specific claim being made. Even a cursory examination of the literature shows that studies involving macro-PK have a much higher rate of fraud than those involving ESP (Carrington, 1907/1920, provides a useful, if dated, overview).

RELATIVE RISK

Researchers face two types of risk in cheating by subjects. The first, of course, is the likelihood that cheating was attempted. Perhaps the single most outstanding factor that has implications for the likelihood of fraud is the number of subjects in a study. In nearly all cases, concerns about trickery have been expressed only for investigations involving one (or, at most, a few) individual(s). Readers having any doubts about the probability of trickery with single subjects might examine the chapters in *A Skeptic's Handbook of Parapsychology* (Kurtz, 1985). Virtually all discussions of cheating center on cases of single (or pairs of) individuals being tested or claiming psi powers. There is essentially no discussion of deception in experiments in which groups of subjects are involved. The second risk involves the impact on conclusions. In studies with only one subject, cheating could totally invalidate the results. However, when working with groups of subjects, conclusions need not be based on a single individual.

Perhaps the highest risk research is that which depends entirely on the results of a known trickster. The likelihood of trickery and the potential threat to conclusions are extremely high. In some instances, entire research programs have been based on phenomena produced by one person. The preeminent historical example might be Eusapia Palladino; she freely admitted that she cheated, yet many investigations were conducted with her. Even in more recent times parapsychologists have been very willing to invest considerable effort in working with people who they know practice trickery. This willingness is encountered in poltergeist cases, which have had a high frequency of cheating (e.g., Owen, 1964, pp. 27–87; Podmore, 1896).

When a research program is heavily invested in an individual unknown subject, it is only reasonable to conduct the research assuming the subject will attempt trickery. In some such cases, the risk may be as high or even higher than working with a proven trickster. A preeminent modern example is the work with Bill Delmore conducted by Kanthamani and Kelly. Diaconis (1978) reported: "I am sure that B.D. used sleight of hand several times during the performance I witnessed" (p. 133). (It should be noted that Kelly, 1979, took issue with this, to which Diaconis, 1979, responded. Diaconis is an extremely capable, knowledgeable magician [Kolata, 1985], whereas Kelly apparently had no magic training whatsoever.) Several factors make this case especially interesting. Kanthamani was an experienced investigator, and the studies were conducted at a major laboratory. The work produced a number of refereed journal articles, and these are now frequently cited. McConnell (1983) described a number of the reports as "having unusual evidential interest regarding the reality of psi phenomena" (p. 311). Schmeidler (1977) reported the tests as being "under excellent control of conditions" (p. 93). One paper was reprinted in Rao's (1984b) *The Basic Experiments in Parapsychology*.

Research studies on macro-PK are especially risky, partly because they often involve only one or two subjects. A recent example of such risks is the work of the McDonnell Laboratory for Psychical Research with Mike Edwards and Steve Shaw (e.g., see Phillips & Shafer, 1982). In this case there was ample reason to suspect trickery might be attempted as there was an article about Shaw in the *Skeptical Inquirer* reporting his use of magic tricks (McBurney & Greenberg, 1980). Another example would be the extensive investigations carried out with Masuaki Kiyota (e.g., Eisenbud, 1982; Kasahara, Kohri, Ro, Imai, & Otani, 1981; Keil, Cook, Dennis, Werner, & Stevenson, 1982; Shafer & Phillips, 1982). Kiyota later admitted to having used trickery although not in these experiments (Stevenson et al., 1985; see also Phillips, 1987; Stevenson et al., 1987; Uphoff, 1987a, 1987b).

A more moderate risk is encountered when designing an experiment with a single subject who is well known to the investigators and for whom no suspicion of trickery has been raised. Leonora Piper would be a historical example. More recent examples might include Malcolm Bessent, Hella Hammid, Keith Harary, Ingo Swann, and Olga Worrall. To my knowledge, no questions have been raised about the integrity of any of these subjects. Further, Bessent, Harary, Piper, Swann, and Worrall have been tested by several different investigators in separate laboratories. However, investigators should realize that because such research involves only one subject, the potential threat to conclusions remains high.

A somewhat lower risk situation is found when a relatively small number of subjects are involved in a research program in which the conclusions are based on individual subjects rather than the group as a whole. In such cases, the researchers devote considerable time and attention to each individual. The individuals are typically highly motivated and inter-

ested in the research. Probably the best current example of this approach is the remote-viewing work of Princeton Engineering Anomalies Research (PEAR) (Nelson, Jahn, & Dunne, 1986). Due to space limitations, only their overall results are presented in journal articles; however, data for individual performance are presented and discussed in technical reports available from the laboratory. It might be argued that the risk is high in this type of research because more subjects are involved. However, when a number of subjects show similar effects, the conclusions are strengthened.

An even lower risk situation would involve using a small to moderate number (say between 10 and 50) of highly motivated subjects in which conclusions are based upon performance of the group. An example might be a Ganzfeld experiment in which each subject contributes only one or two trials (e.g., Honorton & Schechter, 1987).

There seems to be only a small risk of subject trickery when using large groups of unselected subjects, such as in a classroom situation (e.g., Tadonio, 1976). The testing is done in groups. The experimenter gives rather little attention to any individual subject. Each subject may contribute only one data point (out of several hundred). Usually the subjects have little or no prior awareness that the test is to be conducted, thus there is little time to prepare.

Probably the smallest risk exists when subjects are not even aware of being in an experiment. Examples of these can be found in Schechter's (1977) review of nonintentional ESP studies. Experiments in which the subjects are plants (e.g., Edge, 1978) or inanimate objects are not open to accusations of cheating by the subjects.

The bulk of the research in parapsychology is done with unselected subjects, and thus the issue of subject fraud is usually of minor importance. Akers (1984) has written: "In my own experience, I have rarely encountered sophisticated trickery, even among subjects claiming psychic skills" (p. 137). Even critic C. E. M. Hansel (1966) has stated: "It is unlikely that more than a small number of experiments on ESP are affected by cheating" (p. 234). However, the most heavily publicized studies are those involving individual subjects with dubious reputations.

IMPLICATIONS FOR SAFEGUARDS

To date, there has been no established code or philosophy that provides guidelines as to level of security needed when working with various types of subjects. The views expressed have often been contradictory. Some have demanded equally stringent controls for all psi experiments. However, that is not a workable approach.

Some Conflicting Views

Rhine (1938, p. 151) has stated: "The goal of the experimenter is the complete exclusion of all possible sensory cues, which includes assuming

the dishonesty of the subject," and "if under the conditions deception is humanly possible, the conditions are not adequate to establish the degree of confidence required for so weighty a conclusion as the occurrence of extra-sensory perception." Although many people subscribe to this approach, it is not universally shared. Some believe that the burden is on the skeptic to prove cheating occurred in an experiment rather than it being the responsibility of the experimenter to safeguard against trickery. Alfred Russel Wallace (1891) argued that magician S. J. Davey should be considered to be a genuine medium unless all his tricks were publicly explained!

The critics likewise show little agreement on the topic. In discussing tests with groups of subjects, Hansel (1966) writes: "It is necessary to take as stringent safeguards against spurious high scores as in experiments with single subjects" (p. 166). However, others indicate that the necessity of controls and safeguards is a matter of degree. For instance, Gardner (1983-84) writes:

Even among psychics, very few claim such fantastic powers as the ability to bend metal by PK, translocate objects, and levitate tables. It is only when exceedingly rare miracles like these are seriously investigated that it is essential to call in an expert on the art of close-up cheating. (p. 115)

Safeguards Based on Risks

I do not completely agree with any of the positions outlined above. It should be recognized that the likelihood of trickery and level of safeguards are both matters of degree. The cost of controls must be considered in any study. It is often not practical to impose maximum controls. The safeguards needed when testing a known trickster are drastically different than when testing in a typical classroom situation with unselected subjects. On the other hand, there are many instances in which controls can be implemented at little or no cost. In such cases, there is little reason not to do so.

In the highest risk studies (i.e., those with single subjects who are known tricksters or who are unknown to the investigators) the experimenter should plan the experiment as though cheating definitely will be attempted. From a purely pragmatic standpoint, the researcher should assume the subject to be a fraud. A qualified magician should be consulted; more will be said about this later. Extraordinary precautions need to be taken, especially in macro-PK studies or when using newly developed procedures. Researchers should be warned not to become too complacent when considering the possible extremes a trickster might undertake. For instance, Mary Toft fooled physicians by "giving birth" to rabbits that she had previously placed in her vaginal passage (see Price, 1931, p. 40; Seligman, 1961). It was suggested that Margery's uterus may have been surgically enlarged and possibly concealed parts of fetal corpses in order to simulate ectoplasm (see Tietze, 1973, pp. 117, 167). Majax (1975/1977) has suggested that some card cheats may have radio receivers im-

planted inside their skulls. Chari (1973) has reviewed several other bizarre stunts.

When working with small groups of highly motivated subjects, there is ample cause for concern. For instance, Pamplin and Collins (1975) tested six school children who claimed metal-bending ability. Five were found to cheat when they thought that the controls were reduced. However, they did not report any evidence of prior planning of deception. On the other hand, Keil (1979) worked with 30 subjects who had previously reported PK metal bending, and he obtained negligible results, with no suspicious behavior. One of his tests involved attempted movement of a compass needle, but no clear movement resulted in 29 of the 30 cases. Movement in the one case was later found to be due to an elevator in the building. Keil realized that using a compass in this preliminary work might allow surreptitious use of a magnet, yet there was no evidence of this in any of the 30 people he worked with. Nevertheless, it seems reasonable to insist on rigorous controls when conducting *formal* trials with small groups of highly motivated subjects.

Studies with well-known psychic practitioners of high integrity might be conducted a bit more leniently. However, such an approach is not completely fair to the subject. Aggressive skeptics are likely to cast doubt on the findings and sometimes question the honesty of the psychic. The experimenters have a major responsibility to protect the good reputation of the subject. This necessitates strong safeguards against cheating.

When research is conducted with a number of unselected subjects, experiments have frequently been “designed under the (reasonable) assumption that a trickster would not be present. Usually, this would be a safe assumption” (Akers, 1984, p. 137). Akers points out that a number of published psi experiments did not implement controls that excluded “cases where ordinary subjects might have cheated spontaneously, without much forethought” (p. 137). In many instances, little time or expense is required to implement more stringent controls; in such cases, there seems little reason not to do so.

REPORTING REQUIREMENTS

When preparing research reports, one should give some information about the background of the subjects. This is the only way a reader can make a reasonable evaluation as to the likelihood of attempted trickery. This is particularly crucial when special subjects are used. Guidelines for reporting have not received much attention in the parapsychology literature. It seems only reasonable to insist upon including all information that would bear on the likelihood of fraud.

Previously, it has been common practice not to report a subject’s known ability and practice of deception. In the case of Eva C., several prominent researchers protested publishing discoveries of fraud (e.g., see Lambert, 1954). In the very first issue of the *Journal of Parapsychology*, Pratt

(1937) did not report that he had caught Mrs. M. cheating in one of his sessions (J. G. Pratt, personal communication to C. Akers, October 24, 1978). Similar practice continues to the present day. For instance, Subject #4 in Baumann, Stewart, and Roll’s (1986) study was Tina Resch, who had received extensive media coverage and had admitted to trickery in the past (Stewart, Roll, & Baumann, 1987), yet none of this was mentioned in the report. In the papers at the 1986 PA convention involving reportedly fraudulent subjects, none of the authors acknowledged the fact in their reports. One of the papers even failed to give the subject’s name, in spite of the fact that he (Olof Jonsson) had received extensive publicity for his personal claims (e.g., Dunninger, 1974, pp. 195–216; Mann, n.d.; Steiger, 1971). I am personally acquainted with the authors of these recent papers and am sure they had only the best intentions (believing their controls to be adequate). However, to those not familiar with the researchers, such practices can appear to be deliberate attempts to mislead the reader. This has long been understood. For instance, Verrall (1914), speaking of reports of Eva C. wrote: “The omission of any such statement [regarding *alleged* trickery] would naturally be interpreted as implying that she had [an absolutely clean record]” (p. 344). The Parapsychological Association’s “Ethical and Professional Standards for Parapsychologists” contains a section on such issues. It reads:

Ordinarily, in the case of a subject widely known for claims of personal psi ability, the investigator with clear knowledge of psi simulation by that subject has an obligation, once an investigation is completed, to make public that knowledge along with any other information gained from the investigation that may bear upon the public’s perception of the psi abilities of this public figure. It is deceptive to issue a report on such a person and not include all the findings that bear upon how the person’s claims of psi ability are to be interpreted. An investigator who deliberately hides knowledge of such psi simulation in making a public report on any subject is doing a disservice to the public and the scientific community, and is acting in an unethical fashion. (Parapsychological Association, 1980, pp. 14–15)

In many situations with special subjects, serious questions may have been raised about trickery, but no iron-clad, legal proof has been forthcoming. Nevertheless, researchers still have an obligation to address the matter in reports. If a subject has made public claims about psi abilities, it should be noted, and the name of the subject should be given. If there are published accounts of psychic feats, these should be cited. For instance, some accounts might be impressive to laypersons but might strongly suggest trickery to a magician. If there are published reports of unsuccessful studies that could raise suspicions, these also should be cited. For instance, some investigators of Masuaki Kiyota did not cite the report of Scott and Hutchinson (1979). Researchers do not often need to obtain *proof* of prior trickery by a subject. If there is any information that might raise suspicions, the investigator has an obligation to impose stringent controls. This is to assure the integrity of the results as well as to protect

the reputation of the subject. The primary focus must be on the adequacy of the controls and not on whether there is absolute proof of prior deception.

When describing a study with a number of participants, the reporting requirements need not be as stringent as with a special subject. However, it would be wise to give results for each individual such as in the "Illustrative Study Summary" table suggested by Hyman and Honorton (1986) or that of West and Fisk (1953). If someone examining the report later felt that cheating was a possibility, one could reanalyze the result by eliminating one or more of the high scorers (in doing so one should give some defensible estimate of the percentage likely to have attempted cheating in the situation). The result could then be examined to see if the overall conclusions were still supported. This would provide some measure of the strength of the findings. An analogous procedure was used by Honorton (1985) in his meta-analysis of the Ganzfeld. He calculated the significance level after deleting the two most successful and prolific researchers. The result was still strongly significant.

CONTROL

Parapsychologists have investigated a variety of phenomena under a wide range of conditions. Two of the broad categories of research are laboratory investigation and field study. These are largely distinct. Laboratory work typically allows considerable control over observations and conditions, whereas field studies permit a range from little or no direct control to a great amount. For example, poltergeist phenomena are typically sporadic, whereas the phenomena of "psychic surgery" are stable, repeatable, and allow more systematic observation.

In some cases, field studies are quasi-experimental, as in Western scientists' observations of Kulagina (e.g., Keil, Herbert, Ullman, & Pratt, 1976). In this case, the subject was amenable to controls; however, the time available for experiments was very limited. It seems fair to say that the researchers were able to participate more in demonstrations rather than fully controlled experiments. In other instances, macro-PK subjects may allow no controls whatsoever (e.g., Sai Baba).

Some have suggested that in certain circumstances it may be unwise to impose rigorous controls in order to "prove" the phenomena. This position is suggested occasionally and usually only with regard to macro-PK effects. In fact, William McDougall (1926/1967) advocated this in relation to Margery. He wrote:

This third method consists in accepting all the medium's conditions, faithfully abiding by them, in the hope that thus, if genuine supernormal phenomena occur, one may attain a conviction of their reality; and that also, if trickery is used, close observation on repeated occasions will discover the fact and something of the *modus operandi*. (pp. 182-183)

McDougall specifically argued against a "trick-proof test" in this case. Others have argued that experimental control may be premature in some cases, such as Batcheldorian sitter groups. They argue that not enough is yet known to implement effective controls and still retain the phenomena. Such an approach creates a very ambiguous situation. Researchers may become unwilling or unable to acknowledge the ambiguity and weakness of the evidence. Remaining "scientifically objective" in such circumstances is extremely difficult. Some have been critical of those who undertake this approach that imposes few safeguards. Critics suggest that those who follow such a path are gullible or "true believers." Proponents argue that the critics do not wish to understand the phenomena. Given the provocative (albeit preliminary) results of Cornell (1961), it is not clear just whose view is naive.

This paper is particularly concerned with evaluating quality of research and procedures for establishing strong evidence. The vast majority of parapsychological research consists of studies attempting to implement rigorous safeguards.

CONTROL MODE

The question of fraud prevention and detection raises the issue of reliability of human observation. All scientific work requires human observation at some level, of course. The real question is how reliable the observing is likely to be at crucial points, and that depends upon the nature of the object or event being observed as well as on who is doing the observing.

A psychical researcher can take two basic approaches to preclude cheating. The scientist can focus attention on the subject and try to spot tricks, perhaps using cameras and other aids. Alternatively, effort can be spent in securing the target system (the item to be influenced by PK or perceived by ESP) so that the subject, and possible accomplices, do not have access to it. I refer to these two as "subject-based control" and "target-based control." By these I mean the aspect on which the experimenter concentrates attention and resources in order to secure against trickery.

The focus of experimental control is an issue that has received little explicit attention. However, this aspect of methodology determines the reliability of observations. The adequacy of security measures can largely be defined by how easily and efficiently crucial observations can be made. As might be expected, target-based control lends itself to more reliability.

On the continuum of target-based control to subject-based control, extreme examples of subject-based control can be found in the historical investigations of physical mediums. For instance, in sittings with Margery, a sitter would grasp an arm or leg (Tietze, 1973). The lights would be put out, and objects would move about some distance from the me-

dium. At times, as in some sittings with Florence Cook, the medium would be restrained by ropes or monitored by being placed into an electrical circuit (e.g., see Stephenson, 1966). The strength of the evidence for genuine PK depended entirely on how well the restraint and observation of the medium (or others, such as hidden accomplices) were assured and documented. With this approach it was often extremely difficult to come to conclusions (either that fraud did or did not occur); this fact is well illustrated in Dingwall's (1922) reflections on his work with Eva C. At times, observers of mediums reported completely opposite things (e.g., Feilding & Marriott, 1911). Although this historical approach was not particularly successful, it is still not an uncommon laboratory practice to use subject-based control. The work with Ted Serios (Eisenbud & Associates, 1967, 1968), Bill Delmore (Kanthamani & Kelly, 1974a), and Joe Nuzum (Schwarz, 1985b) contains recent examples. The drawing in Green and Green (1977, p. 214) showing Swami Rama attempting to move knitting needles is an especially good illustration. The swami is shown bound, with a mask over his face to preclude his moving the needles by blowing on them. The needles themselves were not protected.

With the laboratory work of Rhine, security measures started to focus more on the target. For instance, ESP cards might be placed in a box, and the subjects would be asked to guess the order. An extreme example of target-based control can be seen in a recent study by Weiner and Zingrone (1986). Subjects were asked to guess a list of Zener symbols (the targets). Later, a list of targets was generated for each subject by obtaining a random entry point into a table of random numbers; the digits thus obtained were converted to specify the targets. In some instances, the random entry procedure was done several days after the guesses were made and over 1,000 miles away from the subjects. This method has been used for a number of other successful projects and reasonably precludes subject cheating.

In most cases, the type of control ranges somewhere between the case of purely subject-based control and purely target-based control. The position of a particular experiment relative to these two extremes will depend upon how secure the target system can be shown to be, as well as the level and type of observation necessary to safeguard against and detect trickery. Evaluation of such security measures requires professional judgment. The quality of that judgment will depend on the level of knowledge of other studies in which trickery has been attempted, technical knowledge of the target system (e.g., computer systems [e.g., Brand, 1987; Morgan, 1988], piezoelectric PK sensors), and a background in conjuring.

SUBJECT-BASED CONTROL

When the primary focus of experimental control is the subject himself, the quality of eyewitness testimony is of utmost importance. In order to evaluate the reliability of a given report, two broad categories of

issues need to be considered: first, the nature of the event observed, and second, the characteristics of the witnesses. In recent years, there has been considerable empirical investigation of eyewitness testimony (e.g., Hall, McFeaters, & Loftus, 1987). Much of this material is pertinent to issues in psychical research, and below I have drawn on some of the ideas of Loftus (1979).

Event Factors

There are a number of aspects of any event that will determine how accurately it is likely to be observed and reported. The duration, frequency, and forewarning should be considered when making evaluations. For instance, some poltergeist events are extremely short and occur without warning to the observers. Thus the evidence for such a specific occurrence is rather weak. On the other hand, some macro-PK evidence is far stronger. There were many levitations of Joseph of Copertino, and some of these lasted more than a quarter of an hour (Dingwall, 1947).⁶ Another factor to consider is whether there was any warning that the event was about to occur. Magicians and fraudulent poltergeist agents make practical use of this concept. A good magician will usually not tell an audience what is to occur next (e.g., Fitzkee, 1945/1975). Most effects will generally not be repeated for the same audience.

Witness Factors

A number of factors determine the reliability of a witness to an event. The expectations, perceptual set, prior beliefs, personal relationship with the subject, background, culture (Segall, Campbell, & Herskovits, 1966), and knowledge of conjuring all can influence perceptions. Surprisingly, these have sometimes been totally neglected when discussing visual validation techniques (e.g., Hasted, 1976, pp. 367–368; Hasted, 1981, pp. 34–35).

Perceptual set. The perceptual set of observers is important. If one knows that one is definitely watching a trick, what one sees may be quite different than when one believes that one is seeing genuine paranormal phenomena (see Nardi, 1984, for discussion). Gardner (1983–84) has pointed out that Eisenbud's challenge to Randi to duplicate Serios's psychic photography is unreasonable. If Randi did undertake the challenge, he would not be subject to the same type of observations as Serios. It is not clear that there would be any meaningful way to compare them because Serios is no longer active. (Sidgwick, 1886, p. 66, made a similar point in another case.) However, it should be noted that Randi did actually

⁶ It should perhaps be noted that the late Eric Dingwall was a member of the Committee for the Scientific Investigation of Claims of the Paranormal (CSICOP) as well as of the Magic Circle.

accept the challenge and then backed out (Eisenbud, 1975; Fuller, 1974). Eisenbud (1983, p. 118) has issued his own \$10,000 challenge.

Of course, a skeptic's perceptions and controls can be as biased as a believer's. For instance, psychologist Edwin Boring was given some responsibility for controlling Mr. Code during a séance in which Code was to simulate some of Margery's phenomena. Boring (1926) later candidly admitted that he allowed Code to get away with cheating because he wanted him to succeed. Arthur Ellison conducted an experiment in which he levitated a bowl of flowers with a hidden electromagnet. Yet five of six witnesses refused to admit the evidence of their eyes (described in Inglis, 1986, pp. 266–267). Steiner (1986) described how he fooled many skeptics at a CSICOP conference by claiming not that he was psychic but that he could detect extremely subtle cues. Also, Hyman (1964) has described how magicians can misperceive tricks because of their expectations.

Social factors. Social factors need to be kept in mind when making evaluations. Were the witnesses under any social pressure to believe or accept a certain result? The widely cited research of Solomon Asch (1951/1963) showed that simple judgments such as comparing lengths of lines could be dramatically altered by (quite subtle) peer pressure. Gregory (1982) has pointed out that in poltergeist cases, "one is almost invariably precipitated into a disturbed human situation in which it is impossible, even if it were desirable, to maintain impersonal neutrality" (p. 14). Those examining poltergeist reports should be especially attentive to these types of pressure. It is quite common for investigators to spend several days in a household with those who have experienced the phenomena. Strong interpersonal relationships develop. There have been cases in which the poltergeist agent has lived for a time with one of the researchers who served as therapist (Prince, 1926; Stewart, Roll, & Baumann, 1986). It would be desirable to have such factors explicitly addressed in reports.

The number of witnesses to an event must be considered. If several persons made reports, how independent were they? Did they discuss matters among themselves before preparing reports?

The issue of social factors extends to the laboratory as well. The work with Delmore is a good illustration. Kelly and Kanthamani (1972) state:

Despite his ability, B.D. presented formidable difficulties as a subject. He is quite temperamental and not particularly sympathetic with the aims and methods of experimental research. . . . Accordingly, productive sessions in the laboratory were typically coupled with varied amounts of argument, sometimes heated, regarding the utility of experiments, the present and future organization of parapsychology, and related subjects. (p. 188)

When using a subject-based control methodology, it is often difficult to be completely sure of just who is specifying experimental conditions. I am not aware of anyone else using most of the experimental procedures used with Delmore. They appear to have been established largely by the whim

of the subject. The test procedure suggested by Delmore (Kanthamani & Kelly, 1974a, pp. 364–365) gives the impression of being from a book of magic tricks rather than a formal laboratory test.

Memory factors. When evaluating reports, one should consider when the report was made. Did the observer make notes (or an audiotape recording) of the event as it was taking place? If after the event, how long after? The longer a person waits to make the report, the more errors of memory are likely to be made. Magicians will sometimes make subtle suggestions about what the audience has already seen. At times they will misstate the order of events and thus change the audience's memory of what had happened. Virtually all magicians have had the experience of hearing an audience member later recount an effect, making it sound far more fantastic than it actually was. Even written accounts based on original published material sometimes contain similar distortions. An amusing example is Rogo's (1982, pp. 33–34) account of the well-known photos of the "levitating" yogi (Plunkett, 1936; see also "Levitation Photographed," 1936). Rogo reports that the yogi "right before the eyes of the startled onlookers—rose laterally up into the air" (p. 33). It was not clear from Rogo's account that the yogi was under a tent that shielded the audience's view while he was rising. (It is surprising that Rogo presented this as though it might plausibly be considered a genuine miracle. The trick has been explained many times [e.g., Brandon, 1983, pp. 207 (facing), 273; Proskauer, 1936; Rawcliffe, 1952/1959, pp. 209 (facing), 281; Zusne & Jones, 1982, pp. 84–85].)

Background of witnesses. The knowledge and background of witnesses must be considered. Some people will be more reliable observers than others, and this is especially true when attempting to detect trickery. For instance, most experienced magicians have watched thousands of simulations of paranormal events. Further, they have spent years studying such methods. Hodgson (1894) wrote:

It is, of course, not to be doubted that a conjuror, or a person familiar with the devices of conjurors, is more likely to discover the *modus operandi* of a trick than the ordinary uninitiated observer,—and even if he fails to discover the trick, he may by virtue of his knowledge of various trick-devices, write a better record than another person who is not superior to him in other respects. (p. 360)

I think most reasonable people would agree with Hodgson's statement. However, a few parapsychologists even discount the value of magicians in psi research. For instance, Gregory wrote: "A magician's word *as such* is not necessarily more acceptable to, say, academic people in any case. . . . The kind of experiments we are planning are not, I would hope, such as could be at all easily counterfeited by a magician" (cited in Manning, 1982, p. 353). However, this seems to express a minority opinion in the field. In cases in which trickery is a possibility, observations made by magicians must be given much more weight than others. Although having

a magician observing an event is no guarantee that fraud will be detected, it does improve the chances. When conducting work where observation of the subject is crucial, it only makes sense to seek high-quality witnesses, that is, ones who are knowledgeable in legerdemain.

It is surprising then, that most psychical researchers have had no such background. Many poltergeist researchers have not educated themselves in magic. Yet the entire strength of some of their cases rests entirely on the investigators' personal observations (e.g., Stewart et al., 1986). However, even an accomplished magician may not necessarily detect bogus poltergeist activity. Poltergeist phenomena usually happen without warning, thus it is difficult to direct one's attention where needed in order to detect the trick. As Maven (in Singer, 1987) noted in his observations of psychic surgery, the practitioner was not especially technically adept but was very good at timing (i.e., making the crucial move when it would not be detected). I have had a similar experience with a "poltergeist agent" engaged in trickery. This is not to say that magicians are useless in these situations, but realistically, they may be of limited value for actually catching fraud in the act.

In other types of field investigations, knowledge of magic tricks is important as well. In certain cases, investigators can impose little or no control and must be content with merely watching. The only way a reader can evaluate a report from such a study is to consider the knowledge, background, and expertise of the researcher. One must consider the likelihood that the researcher would catch a trick if it indeed occurred. For instance, Pratt and Keil (1973) observed Nina Kulagina and reported: "We never observed any behavior suggesting that Kulagina was preparing a trick" (p. 387). Haraldsson and Osis (1977) watched Sai Baba and reported: "We were not able to detect any evidence of fraud" (p. 40). In these cases, given other statements made in the reports, it seems quite clear (to a magician) that the observers had no such relevant expertise whatsoever. When scientists report their observations in professional journals (as were these), they imply that they have the technical competence to make the observations and the expertise to evaluate them. Failure to report the lack of such background is deceptive to the reader. In the case of Sai Baba, it can be noted that Christopher (1979, pp. 114–116) described a number of events suggesting trickery.

Experience in conjuring is important not only in poltergeist cases and other field studies but in laboratory research as well. In fact, it is probably even more important for laboratory work. Poltergeist investigators usually do not witness paranormal events themselves but rather rely on accounts of others. In contrast, laboratory workers strive to elicit phenomena so they can observe them for themselves. Thus it can be especially crucial for these researchers to be trained in conjuring or consult with someone who is.

In some instances, experimenters have not appreciated that special training is required to make adequate observations. This is illustrated in

acute form by the work of Kanthamani and Kelly (1974a). In this study, Delmore was to shuffle a deck of cards with the goal of matching a prearranged target deck. After the shuffling was completed, the experimenter recorded the target deck. The subject then turned over the cards from his deck. The report reads: "The subject was generally allowed to turn the cards of the call deck one by one, because he enjoyed doing so and the presence of experimenters and observers rendered it extremely unlikely that he could at this point change the order of the cards" (p. 361). This cannot be considered a procedure of a preliminary, isolated experiment; a virtually identical statement was made in Kanthamani and Kelly (1975, pp. 207–208). It is hard to believe that the authors, editors, or referees had any experience in conjuring because there are dozens of methods for accomplishing the feat. I recommend the book *Gambling Scams* by Ortiz (1984) for a good discussion of what can be accomplished with playing cards.

Another example of this type is Eisenbud's work with psychic photography (for overviews, see Eisenbud, 1974, 1977b). He has acknowledged that the strength of his investigations depends upon his ability to spot trickery, and in at least one circumstance he has claimed having "complete confidence" and "considerable experience in this type of situation" (Eisenbud, 1977a, p. 303). One wonders whether Eisenbud has ever detected attempted faking of psychic photography. Randi (1981) has shown that Eisenbud's (1981) knowledge of trickery is apparently minimal. Eisenbud's work on psychic photography utilized a subject-based control methodology and relied heavily on his direct, personal observations to control against trickery; as such, only a very weak case has been made for the genuineness of the phenomenon.

The above are not isolated examples. Many others could be cited. For instance, Stevenson and Pratt (1968) wrote: "We never observed any movement on the part of Ted [Serios] which was in any way suspicious" (p. 109). The essential problem in evaluating such reports is that the reader (and the experimenter) has no "baseline." That is, if trickery really did occur, what was the probability of it being detected? As mentioned earlier, it is difficult in cases like these to establish an empirical control condition because of varying "perceptual set." Further, one might wonder whether either Pratt or Stevenson had any training in magic. For instance, they wrote: "We are fully aware of the inadequacy of ordinary human vision in detecting quick hand movements such as conjurors employ" (p. 125). The notion of "quick hand movements such as conjurors employ" is largely a myth (see Christopher, 1962, p. vii); anyone familiar with conjuring realizes that. It is worth noting that magicians had observed suspicious movements on the part of Serios (Eisendrath, 1967; Reynolds, 1967), and Brookes-Smith (1968) and Rushton (1968) have suggested normal methods for producing the photos. There is also considerable earlier literature on how to simulate psychic photographs (Black, 1922; Price, 1925a, 1925b, 1933, 1936; Prince, 1925), and MacCarthy gave an im-

pressive demonstration of fake psychic photography while handcuffed (Editor, 1935; Wendlandt, 1935). All the evidence suggests that untrained researchers are most unlikely to detect trickery. The burden is upon those who suggest otherwise.

Still another study in which no competent magician was actively involved resulted in one of the poorest reports of recent years. Schwarz's (1985a, 1985b) descriptions of macro-PK phenomena produced by Joe Nuzum read like descriptions from the conjuring literature. The reader may wish to compare the pictures of the match under the glass on page 21 of Schwarz's (1985a) article with that of page 45 in Tannen's *Catalog of Magic No. 15* (Louis Tannen, Inc., 1985). One might also compare the third feat of Affidavit A (p. 17) with the effect called "Rupert's Pearls" (sometimes known as "Devil's Tears") advertised on page 17 of the June 1986 issue of *Linking Ring*. Many other equally striking comparisons could be made. To Schwarz's credit, he did consult with several other magicians; however, those he spoke to seemed unaware of a number of standard magic tricks.

Strength of Eyewitness Reports

There have been some attempts to evaluate how effective human observation can be in detecting trickery (e.g., Besterman, 1928, 1932; Crocker with Prince, 1930; Hodgson, 1892; Hodgson & Davey, 1887). Virtually all those who have conducted systematic investigation have concluded that human observation is quite unreliable. Those who have suggested that direct observation can serve as a reliable detector have been reduced to rather feeble arguments with no empirical data.

It has been suggested that certain ostensible paranormal phenomena have been so spectacular that the reports cannot reasonably be attributed to misperception or trickery. In some cases this is probably true. However, it is rather difficult to specify exactly what criteria apply. That is, how spectacular must such an event be? Perhaps one approach that could be taken is to gather reports of paranormal events and compare them to reports of known magic tricks with similar results. By analyzing the two groups, some distinguishing characteristics might be found. This, however, is not quite as easy as it might first seem. Wallace (1893) claimed (after the fact) to point out differences between Mr. Davey's trick performances and effects seen in séances with professional mediums. Hodgson (1893) was unconvinced by Wallace and suggested that any comparisons between "real" and simulated phenomena need to be made by persons blind as to condition.

Filming

It should also be noted that the problem of baselines applies not only to direct observation. The study of the Princeton-Dartmouth football game

by Hastorf and Cantril (1954) showed the effect of viewers' biases on their perceptions when viewing film. In my personal experience, a number of researchers have suggested that simply obtaining film or video recordings of a paranormal event would provide strong evidence for genuineness. This is clearly not the case. At the 1981 PA convention, Peter Phillips and Mark Shafer (1982) presented video recordings of some small objects apparently being affected by PK. Neither Phillips nor Shafer had any background or baseline by which to judge the events filmed. Although they did not claim that the films provided strong evidence, they clearly had been personally impressed with what they had observed. (It should be noted that at that time many parapsychologists were very critical of the work [see Truzzi, 1987, p. 83].) Later, it was revealed that the filmed effects had been entirely due to trickery by the subjects (Randi, 1983b). Keil and Fahler (1976) filmed the movement of objects near Nina Kulagina. They claimed that their case was strong; however, no commentary was provided by someone with a background in magic. The descriptions read very much like effects revealed in *The New Invocation*, a periodical devoted to weird and bizarre magic.

Simply having photographic evidence of a seemingly paranormal phenomenon is probably not enough to establish its reality. There are many ways to fake such results (e.g., "levitation" photos by Herbert, 1939; see also Crawley, 1982, 1983, on the Cottingley Fairy photographs). And there is a vast literature on trick photography. Thus it is desirable to have reliable witnesses to vouch for the essential accuracy of the photos or films. Anyone familiar with UFO or Bigfoot research is well aware of this.

Filming and videotaping have not been particularly successful in validating paranormality of phenomena, but they have been used to establish evidence of trickery. For instance, May and Jahagirdar (1976) filmed the supposed "materialization" of kum-kum, a red, sacred powder, by an Indian spiritual leader. Their conclusions were somewhat guarded, and they suggested that the action caught on film appeared to be fraudulent (see also May with Bonewits, 1976). Singer and Ankenbrandt (1980) gave a good description of the difficulties encountered in their attempt to document psychic surgery by videotaping. Singer also filmed a holy man who allegedly materialized objects but who was apparently not fully cooperative with the filming (Bharati, 1986). At the 1982 PA convention, Berendt (1983) presented a film of metal bending by Rony M.; Berendt appeared to be persuaded by the evidence. However, John Palmer was not, and after viewing the film, described it as "rubbish" and requested that he be quoted (Hoebens, 1982-83). Peterson (in Singer, 1987) directed one of the most elaborate attempts to document psychic surgery. He found clear evidence of trickery, and his report gives some detail as to the difficulties in using film and video. Pamplin and Collins (1975) were able to catch the actions of cheating metal benders on camera. However, in a later discussion, Collins and Pinch (1982) pointed out that it was quite difficult to

pinpoint unambiguous incidents of cheating on videotape. Establishing evidence of paranormality in such instances is likely to be even more difficult.

Randi (1978) briefly described a protocol to validate metal bending using filming. Osborne (Keil & Osborne, 1981) achieved some success with a procedure rather similar to that suggested by Randi. These procedures primarily focused on monitoring the target.

Blindfolds

Another method for controlling the subject is the use of blindfolds; it has long been recognized that these typically provide little security in ESP experiments (e.g., Sidgwick, 1884). Although they are rarely used today, researchers new to the field sometimes try them. There is considerable information about the topic within the conjuring literature. Some of the difficulties with blindfolds have been explained by Christopher (1975, pp. 77–103) and Gardner (1966).

TARGET-BASED CONTROL

When an experiment's focus of security shifts to the target system, two main concerns should be addressed. The first is the target's actual level of accessibility to the subject; this is important not only during the experiment but before and after as well. The second concern is that if the target was clandestinely accessed, what is the likelihood that this would later be detected by the experimenter?

The level of accessibility will depend on factors such as physical proximity of the subject to the target. In some experiments, the targets may be 1,000 miles away; the previously discussed experiment by Weiner and Zingrone (1986) is an example. In other cases, the subject is allowed to physically touch the targets. Temporal proximity is important too. In some cases, only a second is necessary to gain pseudo-ESP information or manipulate an object for a fake PK effect. In other instances much time may be needed.

The actual level of target security will depend upon the details of the particular study, and as such, all-inclusive general rules are difficult to formulate. Evaluation of such matters requires judgment, and a crucial factor in the quality of the judgment depends upon one's knowledge of conjuring as well as one's familiarity with technical aspects of the target. The types of needed expertise will depend on the particular circumstances. Sometimes experimental reports provide sufficient detail to allow the reader to conclude reasonably that trickery was excluded, but sometimes reports are insufficiently detailed.

Some specific examples may help the reader to develop an understanding of these issues. There are a number of formal psi experiments involving special subjects in which the actual level of target security is

unclear or doubtful. I will focus primarily on studies with special subjects because security measures are of crucial importance in these cases. Such issues become paramount when the experimenter believes that the target system is quite secure but actually lacks the expertise to make a reliable judgment.

An example in ESP testing is the work of Kanthamani and Kelly (1974b) with Bill Delmore. In several of the series, the experimenter randomly removed a card from a box and placed it in an opaque folder. This was done out of the line of sight of the subject. The folder containing the card was then held up to show the subject. He then made his guess. However, there is no mention of any precautions to assure that reflective surfaces ("shiners" or "glims" in magicians' and gamblers' parlance) were not available for the subject. For instance, a window behind the experimenter might have been used for the purpose. Further it was reported that "interested visitors were also allowed to watch during some sessions" (p. 19). Perhaps one of them was able to catch a glimpse and signal the subject. Palmer (1985) criticized this work but concluded that the experiments resulted in a genuine anomaly that merited scientific attention. This evaluation is too generous. The work with Delmore used controls far weaker than customary parapsychology experiments, and the risks were far greater. Within the parapsychological literature there have been a number of discussions of the use of reflective surfaces; see Nicol (1979) for several historical cases. Tart (1968) and Stokes (1982) discussed cheating methods using mirrors. Bergson noted that the cornea of a person's eye might reflect concealed images to another person (Myers, 1887), and Coover (1917) investigated this empirically.

An example in PK testing is the work of Taylor (1975) in preparing metal bars inside tubes. He reported successful paranormal bending of the bars but gave very few details. Randi (1975) showed that it was quite easy to circumvent Taylor's security measures; Taylor (1977) initially disputed this, but now seems to disavow his earlier report (Taylor & Balanovski, 1979).

Safeguards Needed Before and After Tests

As mentioned earlier, the experimenter needs to be conscious of security not only during the experiment but before and after as well. Akers (1986) briefly noted that some of the reports dealing with Hubert Pearce and Pavel Stepanek were deficient in these details. Several other examples can be cited.

The study of Warren and Don (1986) with Olof Jonsson seems to have had insufficient security precautions prior to testing. They placed five Zener cards inside envelopes, and these were stored in the jacket pocket of one of the researchers until the time of the experiment. It may have been possible for the subject to have gained access to the envelopes before the experiment, determined the contents, and marked the envelopes. The re-

port gave no details about the formal security measures in effect before the test was underway. It should be realized that envelopes by themselves afford little protection. There are a number of published methods reported in the parapsychological literature for seeing into them (Besterman, Soal, & Jephson, 1931; Morris, 1986b); other sources also describe methods (e.g., Stewart, 1988). Some take only a second or two to accomplish and leave no traces. There are also published ways of opening envelopes without detection (e.g., *CIA Flaps and Seals Manual* [Harrison, 1975]). Further, simply storing the envelopes in a jacket pocket has been shown to be inadequate. Martin Johnson (1976) sealed a drawing in an envelope and placed it in his pocket. Yet a magician was still able to discern the drawing.

Target material must be secured not only before and during the test but often after as well. Hansel (1966) points out that in the extremely high-scoring experiment of Riess (1937, 1939), target sheets were kept in an unlocked desk drawer in his home some time after the test was conducted. The subject, a friend of one of his students, did not submit her calls until the day after the sending period. Hansel alleged (without apparent documentation) that it may have been possible for someone to have gained access to the record sheets because a servant in the home was known to the students. Another example occurred in the Project Alpha scam (Randi, 1983b); the subjects broke into the laboratory and were able to manipulate PK target items. Thus, simply locking up critical material in a laboratory may not always be sufficient.

At times, simply securing the target system itself is not adequate. If hidden security measures are employed, it may be necessary that the details of these too be kept secure. For instance, if tiny marks are put on a bar of metal to identify it (to avoid substitution), the subject and possible accomplices must not be told about such measures. Some investigators have taken unusual precautions in similar circumstances. For instance, when working with the Society for Research on Rapport and Telekinesis (SORRAT), Hansen and Broughton (1982) kept the documentation regarding security measures hidden in the home of one of the experimenters rather than in the laboratory.

Detection of Trickery

An important question is whether the experimenter could reliably detect cheating if it occurred. One of the main advantages of target-based control over subject-based control is that this detection might be done after the fact, and a variety of checks might be made over a period of time. Outside parties can be used to help in such verification.

There have been a number of schemes used to help assure target security. For instance, if a researcher is working with sealed envelopes, one needs to be sure that they were not surreptitiously opened. In such cases,

secret markings can be made at seams or other locations in order to detect opening or substitution. Another method to detect cheating was used by Zenhausen, Stanford, and Esposito (1977). They placed undeveloped photographic paper (previously exposed to a unique pattern known only to the preparer) next to the target page and wrapped them both in aluminum foil. These were then placed in a manila envelope. Thus, if the envelope were opened, the pattern would be destroyed. This would be revealed when it was developed (52 unselected subjects were used, and no evidence of cheating was found). Rogo (1977) has briefly described several other methods to detect cheating. Tests can also be made after the fact in some studies of psychic surgery. Lincoln (1975) and Lincoln and Wood (1979) reported tests on blood that supposedly came from human patients. The results indicated bovine and porcine origin. Others have analyzed claimed cancerous tissues and found them not to be from a human source ("Psychic Surgery," 1974).

Empirical Check of Controls

Hastings (1977) suggested that controls be directly tested, and Hansen (1982) proposed a formal scheme for doing so. Briefly, it requires preparing a number of identical test items in advance. One of these would be randomly selected and given to a subject. If the subject was successful, another of the items would be randomly selected and given to a magician to attempt to duplicate the feat. Later the two items would be compared with the previously prepared security documentation. This approach provides the experimenter with motivation to design the controls well in order to thwart the magician. Part of this method was put into practice by Hansen and Broughton (1983); however, indications of cheating were found before a magician became involved.

Special Safeguards

Also on the matter of target-based control, one study is especially exemplary, that of Schmidt, Morris, and Rudolph (1986). Schmidt had publicly proposed this type of experiment earlier (Schmidt, 1980; Schmidt, Morris, & Rudolph, 1982), conducted the study, and found strong positive results. The procedure involved a retroactive PK effect and provided convincing controls against not only subject deception but experimenter fraud as well. This experiment probably has the best controls against fraud of any in the history of parapsychology. The details are rather involved, and the reader is encouraged to consult the original report.

There have been a few articles and experimental attempts addressing controls against experimenter fraud; these, of necessity, involve target-based control. Johnson (1975) has discussed a model of control. Akers (1984) listed various experimental attempts along these lines.

Evaluation Issues

Evaluating security measures of a study requires judgment. This should be based on experience and knowledge of similar attempts in the past. Perhaps one of the most important factors in evaluation is the level of detail given in a report. This often gives a good indication as to the thought and attention given the task. Several examples can be cited; for instance, Eisenbud (1982) sealed film and spoons in a lead-lined container. The loading of the container was done while the subject was still in Japan, and the loading was witnessed and videotaped. The seals of the container were photographed close-up. In another recent report, Randi (1983a) sealed metal bars in tubes and took photographs with polarized light that showed the internal stress patterns of the plastic tubes. He also very accurately weighed the tubes; this was witnessed and documented. These reports demonstrated good forethought and attention to detail. The controls were documented and witnessed prior to the experiment. They allowed others the possibility of examining documentation before and after the completion of the study.

In marked contrast are Hasted's (1976) validation techniques with psychic metal-bending. In some experiments, Hasted placed paper clips inside glass spheres; these clips then were bent by the subjects. A surprising aspect of the experiment was that each sphere had a hole in it. This allowed ready access to the paper clips. Nevertheless, Hasted concluded that the paper clips had been bent by psychic means because the clips were so tightly "scrunched" (he provided no data of an empirical assessment). Later, Alabone and Hasted (1977) seemed to acknowledge that this was an entirely subjective judgment. In another example, Cox (1984b, 1985) placed items in a fish tank and then sealed it. Later, some of the items "dematerialized." Hansen (1985b) described how easily the precautions could have been overcome. By being familiar with such reports, someone evaluating other work can posit reasonable alternatives and check to see if the author addressed such issues. By such comparisons, reasonable judgments may be formed.

TELEPATHY EXPERIMENTS

Telepathy experiments involve special considerations with regard to subject trickery, and as such, deserve a separate section devoted to them. In fact this difficulty has been long recognized. Rhine and Pratt (1957/1962) state:

With GESP and pure telepathy, precautions have to be elaborate and have to be adapted to the special needs of the experimental situation. This methodological problem is often taken too lightly; as we have said, GESP is the hardest psi-test procedure to control adequately against error, especially error due to deception. (p. 37)

There have been a number of historical instances that have shown this to be a serious problem. The early SPR investigated the team of G. A. Smith and Douglas Blackburn and found evidence for telepathy (Gurney, Myers, Podmore, & Barrett, 1883). Later Blackburn issued a confession as to how they had deceived the researchers, but Smith denied it (see "Confessions of a 'Telepathist,'" 1911; Coover, 1917, pp. 477–495; Gauld, 1965a, 1965b, 1968, pp. 179–182; Hall, 1964, 1968; Nicol, 1966; and Oppenheim, 1985, pp. 285–286 for extended coverage of the affair). Whatever the ultimate truth of the matter, the experimental precautions did not rule out the possibility of collusion between agent and percipient. Barrett, Gurney, and Myers (1882) also investigated the alleged telepathic abilities of the Creery sisters. Here, too, precautions were not always sufficient to rule out collusion, and later the sisters confessed to cheating (Gurney, 1888; for discussion see Coover, 1917, pp. 463–477; Hall, 1964; "Psychological Literature," 1887). Soal and Bowden (1960) conducted numerous telepathy studies with several Welsh children; during a few of the experiments, two boys were detected using codes to signal each other. This led to some discussion as to how much cheating was actually involved (e.g., Mundle, 1959; B. H. Nicol, 1960; J. F. Nicol, 1960; Scott & Goldney, 1960; Soal, 1959, 1960; Thouless, 1961).

The most severe problem faced by experimenters is the possibility of the sender signaling the receiver. There are innumerable ways of doing this, and a huge number of methods have been incorporated into commercially available magic tricks. Dingwall (1956) has given a very brief historical overview of simulations of telepathy. Some suggested methods for use in psi experiments include high-pitched whistles (Hansel, 1959; Scott & Goldney, 1960), subtle changes in lighting conditions (Estabrooks, 1947, pp. 122–126), radio transmitters (Soal & Bowden, 1960), and even transmitters hidden in teeth (Targ & Puthoff, 1974).

Parapsychologists have not always realized the subtlety of some of the methods. For instance, Thalbourne and Shafer (1983) suggested a radio transmitter was unlikely to have been used in their experiment because there were no semantic correspondences between target and response. However, even a brief glance at some of the advertisements for these devices in magic periodicals would reveal that such correspondences would not necessarily be semantic in nature. Familiarity with the classic *Thirteen Steps to Mentalism* (Corinda, 1968) should also have been sufficient to keep from making such statements.

Morris (1978) has pointed out that a signaling method might be implemented in forced-choice situations in which the sender signals the receiver for the next trial. One could send a slightly longer or shorter signal depending upon the target. An alternative method is to send the signal at certain times. Annemann (1938) suggested that the interval between signals could be used as a code. On the other hand, in typical psi experiments it is acceptable for the receiver to signal the sender when ready for the next trial, as noted by Rhine and Pratt (1957/1962).

Another possibility for trickery arises when the sender is allowed to pick the targets or determine their order. For instance, if the sender is to shuffle a deck of cards to be used as targets, he or she may arrange them into an order previously agreed upon with the receiver (Price, 1955, has discussed similar methods). If the target is selected by the sender without randomization, there is no control against potential collusion between sender and receiver (Scott, 1988), as well as possible deception by the sender acting alone. (The virtually unique modern example of nonrandom, sender selection of targets occurs in the remote-viewing work of Nelson, Jahn, & Dunne, 1986.) The sender might deliberately pick a target that conforms with the response biases of the receiver. Similar problems exist when descriptor lists are filled in by the sender at the time of the experiment, even when the target is randomly selected (e.g., Jahn, Dunne, & Jahn, 1980). The receiver could use a similar strategy; Morris (1982), in his discussion of fraud, noted: "Unless selected randomly from an equally attractive target pool, targets are likely to have certain sensible, preferable characteristics that would allow a psychic familiar with whomever chose the target to infer rationally the nature of the target" (p. 21). The current Nelson et al. (1986) remote-viewing statistical baseline includes information from other percipients' responses. If a percipient said very little (or very much), the empirical baseline might not be appropriate. For any given response, we cannot be sure that the mean chance expected Z score will be zero under the null hypothesis. An optimal guessing strategy may exist; this could allow a sophisticated form of cheating. This was not a problem with the earlier evaluation method presented in Jahn et al. (1980). I should point out that I have no reason whatsoever to think cheating actually took place in the Princeton research, but it should be noted that it is a highly visible research program and has served as a role model for other experimenters (e.g., Rauscher & Houck, 1985).

There are several approaches in dealing with the various problems in such situations. One could use a sender who is also one of the experimenters, as suggested by Rhine and Pratt (1957/1962, p. 161). The researchers could also report the results of the individual subjects. Thus an assessment could be made as to the generality of the results.

The problem of radio transmitters and similar devices is a real one. There are many different types that are inconspicuous; some are hidden in ordinary household items. These are readily available from magic dealers as well as advertisers in certain popular magazines (see Free, Freundlich, & Gilmore, 1987, for an overview). Virtually no parapsychology laboratory currently has the resources to convincingly exclude the use of such devices. The use of Faraday cages and electrically shielded rooms does provide some, but not complete, protection. In situations when security is especially crucial, laboratory personnel might be used as senders.

It seems inappropriate to conduct telepathy experiments without a formal random selection of the target. Stanford (1986) has written that random selection "is presently regarded as a sine qua non of adequate

ESP-test methodology" (p. 14). It is surprising that one parapsychological journal and a major laboratory still accept ESP tests without formal, random target selection.

MAGICIANS

To overcome some of the problems addressed above, scientists will need further contact with magic and magicians. Although conjurors are clearly of limited value in psi research, they do have their place. Indeed there is much to be gained by having them more actively involved. There are a number of barriers that will need to be overcome before this can be fully effected. I will discuss these obstacles and then consider some of the special issues that arise when a scientist wishes to consult with a magician.

LIMITS

There are clearly limits on the value of magicians. Certainly, having a magician involved in the design and execution of an experiment is no guarantee of fraud-proof conditions. In fact, as Hyman has written: "Even if one assembles all the world's magicians and scientists and puts them to the task of designing a fraud-proof experiment, it cannot be done" (1981, p. 39). Critics have frequently called for magicians to be involved in psi experiments. However, a number of the greatest magicians in history have endorsed particular research, but the critics seemingly found it no more acceptable. For instance Robert-Houdin, often referred to as the father of modern magic, endorsed the clairvoyant abilities of Alexis Didier (see Houdini, 1924; Podmore, 1902/1963, Vol. 1, p. 143). J. N. Maskelyne ("Spiritualistic Expose-II," 1885; Maskelyne, 1885) acknowledged the paranormality of some table-turning, noting that Faraday's explanations were not adequate. Professor Hoffman (Lewis, 1886) indicated that he thought some slate-writing phenomena were genuine. Harry Kellar (1893) observed what he considered to be genuine levitations of the medium Mr. Eglinton (however, Prince, 1930, p. 158, reported that Kellar retracted certain statements). Howard Thurston (1910) endorsed the table levitations of Palladino. In one study of apparently genuine telepathy, several members of the Magic Circle attempted to detect a code between a mother and son but were unable to do so (Recordon, Stratton, & Peters, 1968). Abb Dickson and Artur Zorka performed some tests with Uri Geller and reported positive findings (Zorka, 1976). These magicians were well known and well regarded by their colleagues. Obviously, some critics will not be satisfied with evidence even when it is certified by a magician.

Further, magicians can be fooled. An example occurred during Soal's work with the Welsh schoolboys. Jack Salvin, a professional magician and Chair of the Occult Committee of the Magic Circle, was in charge of some of the experiments (with Soal not present); strong results were obtained (Soal & Bowden, 1960). Later, however, Salvin was presented with a

similar test that involved trickery, which he was unable to detect (Scott & Goldney, 1960).

BENEFITS

Given that critics will dismiss evidence even when qualified magicians certify the phenomena, some may conclude that a magician's approval is worthless. For instance, Inglis (1986) has written: "Even if experienced magicians vet projects, and give the seal of their approval, it will bring little benefit" (p. 259). This is incorrect. All evidence is a matter of degree. Having a magician consult on a research project enhances the strength of evidence as well as overall credibility. Certainly, technical improvements in studies can be achieved if competent advice is obtained. There are a number of ways in which consultation with magicians can be of benefit.

In experimental work, magicians may be able to suggest how safeguards can be overcome with magicians' methods. Experience in magic can help a person detect procedural flaws in psi experiments. For instance, certain subtle sensory cues are similar to those on which magicians sometimes capitalize. I suspect that Ray Hyman's sharp eye for flaws (e.g., Hyman, 1977) is in part due to his background in conjuring. Also, Martin Gardner (personal communication, January 18, 1988) is preparing a critique of the Stepanek work based on potential loopholes in experimental procedure.

Morris (1986a) has suggested that parapsychology laboratories have a competent magician review procedures every year or when a new area of research is undertaken. This precaution may uncover weaknesses and also would serve to keep laboratory personnel more alert to the possibilities for trickery.

In field investigations of regularly occurring extreme phenomena (e.g., physical mediumship, psychic surgery), magicians may detect methods used. Some of the methods of fraudulent psychics are well known to magicians and may be spotted immediately. Even if a method is new, a magician is in a much better position to suggest how the feat was accomplished. Nearly all magicians have had lots of practice at this because they frequently watch new effects being performed. The information provided by the magician can be used by a researcher to make a preliminary evaluation as to whether further investigation is warranted.

Magicians can, at times, provide information about the background of certain psychic claimants. There are a number of such claimants who have studied magic, and this can often be learned by utilizing the informal networks in the conjuring fraternity. For instance, magicians in Steve Shaw's home area undoubtedly would have been familiar with him because Shaw (of Randi's Project Alpha) received considerable news coverage for feats as a mentalist (e.g., "Young Mentalist," 1978; Hazlett, 1979). If a particular claimant has practiced trickery in the past, a researcher should then be especially careful.

Parapsychologists could benefit by more knowledge of conjuring. Even if one only does research with large groups of subjects, one may be called on to referee papers in which knowledge of trickery is needed. Such training is useful when preparing review articles as well; a reviewer needs to be able to evaluate the soundness of studies. Background in conjuring can help researchers avoid making silly public statements on the topic (see Hansen, 1987b, for a listing of examples). Further, many researchers teach courses or give lectures on parapsychology in which the issue of trickery is likely to be raised.

BARRIERS TO COMMUNICATION

There are four major factors that have presented difficulties in effectively consulting with magicians. Probably the most important obstacle is parapsychologists' lack of knowledge of magic and magicians. Most researchers do not know enough about conjuring to establish and maintain effective communication. A second problem, much related to the first, is that information on conjuring is not readily available. The third factor is that there has been no effective network or institutional channel to promote communication. The fourth factor is the belief of many scientists that most magicians are hostile to psychic research. This stereotype is false.

It should be noted that many parapsychologists have consulted with magicians in the course of their work (e.g., Beloff, 1984b; Bender, Vandrey, & Wendlandt, 1976; Bersani & Martelli, 1983; Crussard [Randall, 1982]; Eisenbud, 1967; Haraldsson & Osis, 1977; Hasted, 1981; Recordon, Stratton, & Peters, 1968; Rhine, 1934; Roll & Pratt, 1971; Ryzl, Barendregt, Barkema, & Kappers, 1965; Schwarz, 1985a; Shafer & Phillips, 1982; Targ & Puthoff [Marks & Kammann, 1980]). Parapsychologist Eberhard Bauer even appeared on the cover of the January–February 1980 issue of the German magic magazine, *Magische Welt*. Further, it can be noted that Richard DuBois ("Obituary: Richard DuBois," 1965), former president of the Society of American Magicians (SAM), and Gerald L. Kaufman (see "Gerald L. Kaufman," 1968), past president of the parent assembly of the SAM, both served on the Board of Trustees of the American Society for Psychical Research (ASPR). Kaufman was especially active (Murphy, 1969). However, there is reason to think that these interactions have not been as fruitful as they might have been. It seems that magicians have played an extremely minor role; further, only rarely did they prepare written reports of their participation.

Level of Conjuring Knowledge

A certain amount of knowledge is needed before effective consulting can be done. This should surprise no one, for a similar situation exists when consulting, say, a statistician. If someone with no training in statistics whatsoever designed a complex psychological experiment, collected data, and then consulted a statistician, we might expect the results

not to be very meaningful. Indeed, the design might not even be analyzable. In order to ask the right questions and understand the advice, a researcher needs to be somewhat familiar with the subject matter.

Many of the studies criticized above appeared in refereed parapsychological journals and not just as convention abstracts. This should raise considerable concern. To me, a number of researchers have seemed a bit complacent about the general level of sophistication regarding fraud prevention. To test my perceptions I decided to assess the level of conjuring information available in parapsychology. I conducted two brief oral surveys in which the respondents were informed of the nature of the poll. The data were usually collected in brief conversations at conventions or over the phone.

Survey of PA presidents. The current and past presidents of the PA were queried about their background in conjuring. These people were selected because they are expected to be the most competent; they are the ones who set standards in the field, and many have responsibilities for training students and newcomers. I was able to contact 23 individuals (of 24 now living). Of these, only 4 had ever taken a course in magic; 19 owned 2 or fewer books on the topic, and only one had more than eight books. Only three had reported ever reading any conjuring periodicals.

Library survey. I contacted five libraries at institutions devoted to parapsychology. These each had over 2,000 books and had at least a part-time librarian. Those contacted included the Foundation for Research on the Nature of Man, Durham, North Carolina; American Society for Psychological Research, New York; Parapsychology Foundation, New York; Mind Science Foundation, San Antonio, Texas; and the Parapsychology Sources of Information Center, Dix Hills, New York.

As of January 1987, the largest number of books on conjuring in any of these libraries was approximately 60 (at the ASPR, as listed in the card catalog). Only about half of those were primarily devoted to explaining techniques of magicians, and most of those were quite old. The next largest collection was approximately 20. No library subscribed to any magic periodicals.⁷

Hidden Knowledge

Magic is unlike academic areas because the knowledge is not readily available to outsiders. This poses obstacles that are not fully realized, even by magician critics. If someone wants to learn something about quantum mechanics or biofeedback, for example, one only needs to go to a library or check with experts in the field. The findings and information in such things are "public." One of the norms of science is that knowledge be-

⁷ While James Matlock was Librarian and Archivist at the ASPR, that library started to subscribe to several magic periodicals and undertook a more active acquisition policy.

comes public property; Merton (1942/1973, p. 274) states that "secrecy is the antithesis of this norm." This is quite different from the situation in regard to magic. The literature is difficult to obtain, and there can be penalties for revealing methods to those outside the fraternity. Gloye (1964) has provided a good discussion of this. Further, there is much disagreement within the magic community about revealing methods of mentalism when such is presented as genuine (Rauscher, 1984).

In response to an article by Harry Collins, Martin Gardner (1983-84) has written: "How Collins got the impression that magicians are reluctant to explain secrets of psychic fraud is beyond me" (p. 115). Gardner gives the impression that magicians would be delighted to help scientists. Unfortunately, the situation is not quite so simple. I personally know of several instances in which a researcher has approached a magician and asked about a particular apparent psychic effect; the magician refused to give out information. After such unsettling experiences, some have concluded that magicians are simply not worth consulting. I have also encountered a psi researcher (who worked at a major parapsychology laboratory) who was also a magician. He thought that I revealed too much when I only mentioned (to other parapsychologists) the existence of the book *Confessions of a Psychic* (Fuller, 1975), in spite of the fact it had been discussed in *Skeptical Inquirer!*

Lack of Institutionalized Channels

Another barrier to effective communication with magicians is the lack of established channels. In most areas of research, the needed specialists can be found easily. In a university, if a researcher needs computer programming or statistical assistance, there are almost certainly consultants readily available. The methods for obtaining their help are quite direct. Programmers' and statisticians' positions are institutionalized within academia and are clearly visible. Further, if the researcher knows nothing of computers, the programmer is likely to be familiar enough with aspects of the project to be of help. That is, there is sufficient shared culture. Magicians, on the other hand, are not so easily located through traditional academic channels. I know of no university courses for credit on magic, let alone departments devoted to the topic. Even within theater and fine arts programs, conjuring is considered such a low-status art form that it is rarely mentioned. Because magic practitioners are not readily visible or likely to be known to researchers, they are unlikely to be consulted.

Another problem scientists face is trying to identify who is competent as a consultant. There is no body within the magical fraternity that "legitimizes" a magician. If one wants to become a lawyer or M.D., one must take specified course work, pass certain tests, etc. Nothing similar exists for conjurors. As a result, a researcher who wants competent advice may be in a quandary. In fact, I know of several scientists who have visited local magic groups and concluded (rightfully) that those people could be

of no help to them. These researchers understandably went on to conclude that magicians would be of little help. A parapsychologist visiting a local magic group a time or two expecting to learn how to rule out fraud is like a magician visiting a psychic fair and expecting to learn about the science of parapsychology.

Stereotypes

Many people seem to think that magicians are quite hostile to parapsychology, and this has led some researchers not to consult magicians. It should be realized that both critics and proponents have promoted this stereotype and antagonism. Cox (1974, p. 12; 1984a, p. 383) has described many magicians as having "open minds [that] indeed might best be 'closed for repairs.'" Reichbart (1978, p. 170) has claimed: "Not all, but most magicians have an anti-psi bias." Gardner (1983, p. 18) has asserted that "conjurers are indeed the enemy [of psychic researchers]." In fact, the opposite is more likely the case. Birdsell (1981) polled a group of magicians and found that 82% gave a positive response to a question of belief in ESP. Truzzi (1983) noted a poll of German magicians found that 72.3% thought psi was probably real. In a major magic periodical, Sansotera (1987) has given a brief account of a poltergeist in the home of a magician.

EDUCATION NEEDED

Before the field of parapsychology can make significant strides in dealing with subject fraud, a major educational program will be needed. It must be realized that currently there is no institutionalized academic training or career path for parapsychology that is comparable to other academic disciplines. The person entering the field must decide for him- or herself what kinds of training to seek. If parapsychology were a fully established academic field, required course work would include education in conjuring. However, few if any university curricula in any field include anything on magic. Thus the student who desires such training must look outside academe and should invest the time, energy, and money in magic that would be equivalent to education in other topics that would be required in a parapsychology curriculum (e.g., statistics). For instance, a student pursuing a doctoral degree in psychology at a private university might accumulate 10 credit hours of statistics. At \$300 per credit hour, this comes to \$3,000. An investment of this size can be used for building personal libraries, dues for magic organizations, lessons on magic, and trips to conventions. This should be a minimum for someone coming into parapsychology.

This amount of training will not make one an expert. However, it should be enough for average researchers to understand their own limits. It would make them more aware of possibilities of trickery and should allow

them to consult effectively with those who are experts. Researchers wishing to specialize in macro-PK studies and certain types of field investigation should obtain much more training.

WHO SHOULD BE CONSULTED

As mentioned above, it is difficult for most researchers to know which magicians might be worth consulting. Some guidelines can be given. First, any scientist wishing to find a consultant should discuss the matter with the PA liaison with magicians' groups (currently Loyd Auerbach serves as liaison). The consultant should be one who has some appreciation for scholarly work; preferably, he or she should have published a reasonable amount. As Singer and Ankenbrandt (1980) have suggested, to avoid questions of competence, the consultant should be nationally recognized (within the conjuring fraternity) as an expert in an area appropriate to the topic of investigation. Ideally, the consultant would have a similar professional background as the person employing him or her (e.g., psychology, physics). This would allow the consultant to appreciate more fully the problems facing the client.

It is equally important who should *not* be chosen as a consultant. During a PA convention roundtable, several magicians recommended that those conjurers who have a public vested interest in the outcome should not be consulted (Truzzi, 1984). This seems particularly apt. Collins (1983) pointed out that magicians do not share the same values as scientists; rather, they are "a group whose values include secretiveness and financial self-interest above the quest for truth" (p. 931). This fact was especially well illustrated by Houdini, who reportedly framed Margery (Gresham, 1959, p. 254; Christopher, 1969, p. 198, questions this, however). Several modern-day magicians seem especially unsuited as consultants. Randi would lose \$10,000 if he validated an effect as paranormal. Further, he has a tendency to be rather inaccurate in his statements (Krippner, 1977b; Rao, 1984a; Targ & Puthoff, 1977, pp. 182-186; Tart, 1982). In fact, Dennis Stillings has demonstrated that "Randi is capable of gross distortion of facts" (Truzzi, 1987, p. 89). He has even been quoted as saying, "I *always* have an out" with regard to his \$10,000 challenge (Rawlins, 1981, p. 89), and he has reneged on similar offers (Fuller, 1979). He has also admitted to deliberately misrepresenting scientific research in the past (Randi, 1975, p. 61). A number of other magicians affiliated with CSICOP would not be appropriate consultants either because they would lose money if they validated a psychic effect (Hansen, 1987a).

As Collins (1983) has pointed out, a magician consultant should be employed only as a consultant and should not be given control of the study. To do otherwise is to abdicate responsibility as a scientist. Giving control to a magician could put a subject at risk by allowing a possibly hostile magician to frame the subject. Unfortunately, at least one investi-

gator (Delanoy, 1987) gave control of an experiment to magician Randi, whose ethics have been questioned (Truzzi, 1987). The magician's only role should be as an expert in recommending means for discovering and ruling out trickery. The parapsychologist must be concerned with many other issues as well. The researcher must try to establish favorable conditions, be aware of other technical problems such as statistical requirements, and be sensitive to ethical issues. None of these are the province of the magician.

Researchers should be aware that effective consulting is not likely to be accomplished in a quick session or two. Extended services may be needed, depending upon the project. At any rate, the magician should prepare written reports if consulting is extensive. The reports by Hoy (1977, 1981) and Maven (Singer, 1987) are excellent examples.

RECOMMENDATIONS

It is clear that the problem of subject fraud in psi research is not something that disappeared when researchers stopped investigating mediums. Indeed, the problem today is as acute as it has ever been, and it appears that the problem is growing. Further, few investigators have made any serious effort to educate themselves on the topic. Thus strong recommendations need to be made. A greater knowledge of fraud and trickery is needed not only by investigators. Journal editors, referees, book reviewers, and those publishing articles and books reviewing the literature also need to become more aware and informed in these areas.

Laboratories and investigators need to regularly consult with appropriately experienced magicians. At a minimum, it would be a good idea to have a suitable magician review laboratory procedures at least once a year. If new methods are being developed or work is done with special subjects, more frequent consulting will probably be necessary. Most magicians will not be qualified to advise psi researchers. The PA liaison with magic societies should be asked to recommend appropriate consultants.

A greater knowledge of magic is especially recommended for researchers who intend to work with gifted subjects or who want to develop new testing methods. Such researchers should take classes in magic, attend conventions, and follow the periodical literature on the topic.

Reports of a study involving a gifted subject should include a statement describing the subject's background in using or studying trickery (if any). In order to evaluate the potential of attempted cheating, this information is required. Even if ironclad proof of trickery is not available, if there has been suspicious behavior, it needs to be reported. Failure to include this, when there is such background, is deceptive to the reader.

Reports should state who was present during experimental sessions. This will help the reader assess the possible role of accomplices.

When working with unselected subjects, the procedures should not allow the subjects to cheat on the spur of the moment (i.e., without advance preparation). This should be the minimum standard; preferably, a higher criterion should be met.

If the validity of a study depends primarily on adequate control or direct observation of the subject, the report should describe the researchers' backgrounds in conjuring and their ability to make the crucial observations. When one is reporting uncontrolled observations of macro-PK phenomena, some discussion should be included about how trickery might accomplish the feat. If the investigator does not have a background in conjuring, it should be so stated.

If a study relies on target-based control, the report should give sufficient detail to allow an evaluation of the level of security. Empirical tests of security measures might be included.

Journal editors have a special responsibility to select referees that have competence to evaluate reports for controls against subject cheating. This is especially important when the paper involves a special subject, a new type of psi test, or subject-based control.

Referees should alert editors as to the limits of their areas of competence.

If the validity of conclusions of a study depends upon the results of a few subjects, data for individuals should be given.

If the authors make comments as to lack of possibilities of trickery in their experiment, they should provide convincing evidence of that claim.

Formal ESP experiments should include random selection of targets and not leave this to the whim of the sender.

CONCLUSIONS

Parapsychology investigates a wide range of phenomena under a variety of conditions. Sometimes full experimental control is possible, whereas in other cases, researchers are merely bystanders with no say at all. The research strategies and statements of conclusions must vary accordingly. With much of the laboratory work, the scientist can focus on target materials and achieve good controls against deception. When one focuses on controls of the subject, security is more problematic.

Today most professional research in parapsychology is done with unselected subjects and with good controls against deception. However, there is growing interest in working with special subjects, and a number of people have advocated that further attention be given such subjects even when they have been shown to cheat. Much of the publicity given the field involves research with such dubious claimants. Until the researchers establish greater technical competence in conjuring and make use of outside consultants, the field will continue to enjoy a less than optimal reputation.

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