

Science Versus Showmanship: A History of the Randi Hoax

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ABSTRACT: On January 28, 1983, the professional conjuror James Randi announced that he had masterminded a hoax against the McDonnell Laboratory for Psychological Research at Washington University in St. Louis. In this paper, the hoax is described and discussed in some detail, along with some of the controversial methodological and other scientific issues to which the episode has drawn attention.

MR. MAC AND PARAPSYCHOLOGY

James S. McDonnell was an undergraduate at Princeton when there took root in him a deep interest in parapsychology. He even considered becoming a professional in the area, but his father told him that no money was to be made in it (he was undoubtedly correct), and that he should continue in engineering. Young James took his father's advice, and went on to become Founder and Chairman of the McDonnell-Douglas Corporation.

Despite his choice of career, he never lost his fascination with psychical research. By the mid 1970s "Mr. Mac" had become one of the most generous and important financial supporters of parapsychology in the U.S. Through his McDonnell Foundation-which is independent of the Corporation-he made grants to support researchers at Princeton, Duke, Virginia and Syracuse, as well as to other organizations and individual parapsychologists (see also Phillips, 1980; Stevenson, 1981). However, being a St. Louis man at heart, Mr. Mac conceived the idea of setting up at Washington University (with which he had a long and important association) a multidisciplinary center for the study of the paranormal. Its staff would be drawn from the ranks of physicists, engineers, physicians, psychologists, anthropologists, and biologists: Mr. Mac thought that by pool-

This is a revised version of an earlier manuscript that circulated privately. Because of this limited circulation current bibliographies (such as the one made available by J. Lippard via e-mail on January 25, 1994 under the title "Project Alpha") do not list this paper. Owing to Mr. Randi's continued misrepresentations of his Hoax (see, for example, the postscript), it was thought desirable to publish the paper in a wider forum, albeit some years after the events it describes. Many persons provided valuable advice and criticism in the course of the preparation of this paper, but particular thanks go to Peter Phillips, Janet Jungkuntz, Michael McBeath, Mark Shafer, and Linda Evans. Other papers that look at the Hoax from different perspectives are Randi (1983b,1983c) and Truzzi (1987).

ing their scientific expertise and resources, such a team could not only settle the question of the *existence* of psi, but also *explain* psi and assimilate it into mainstream science.

In December 1977, Mr. Mac offered a large grant to Washington University to establish such a center for parapsychology. It became clear, however, that something so ambitious as a multidisciplinary center was not feasible, for too few persons at the university were interested in conducting psychical research. It looked as if the grant could not be accepted.

However, after a period of negotiation, the one person at the university who was in any way involved with scientific parapsychology was persuaded to become director of a smaller center, his mandate being to set up a laboratory and find suitable staff from outside the university to undertake research. This person was Dr. Peter R. Phillips, Professor of Physics. His attention had been drawn to parapsychology in 1970, and he soon after became convinced, by personal experience, of the existence of certain psychic phenomena. He read extensively in the area, attended conventions, and became an associate member of the Parapsychological Association (PA)—the professional organization of parapsychologists, which since 1969 has been affiliated with the American Association for the Advancement of Science. Professor Phillips could be described as being at the time a well-read, sympathetic observer of the psi scene. His primary interest and commitment was (and still is) to his teaching and research in physics. On top of these duties he accepted the role of Director of the new lab, anticipating that most of the research would be carried out by others. Administratively, the lab was to be attached to the Department of Physics because it was there that Dr. Phillips was located.

Thus, on August 8, 1979, Washington University officially announced that the McDonnell Foundation had awarded it a grant of \$500,000 to establish the McDonnell Laboratory for Psychical Research—or “Mac-Lab,” as it came to be known colloquially. This half-million dollars was intended to be spread over five years. (To set this figure in context, the University’s five-year research budget for all fields was, at the time, about \$360 million.)

There is a misconception current that this was the largest amount of money ever awarded for parapsychology in the U.S. The truth is that comparable or larger grants had been given by the McDonnell Foundation, but they did not receive similar media coverage.

RESEARCH BEGINS

In the publicity surrounding the creation of the MacLab, Dr. Phillips had mentioned to reporters that as a physicist he was particularly interested in directly observable psychokinesis (or “macro-PK”), such as the paranormal bending of metal. He was especially interested in finding *children* who had this ability. Such young “psychokinetes” were at the time being studied by other physicists overseas, such as Hasted (1981). In addition, a

(weak) case could be made that very young children would not in general have had the opportunity to have learned conjuring skills (though of course this did not preclude the possibility of their cheating). About a dozen persons, both locally and from around the country, contacted Dr. Phillips with claims to have exerted macro-PK. Unfortunately, none of the persons Dr. Phillips investigated at this time demonstrated the sort of ability that was strong enough to permit detailed study in the lab.

Then, in November 1979, a 17-year-old high-school senior from Marion, Iowa, Michael Edwards, telephoned Dr. Phillips and claimed to possess abilities that seemed more promising than those of any of the others. He also sent a copy of a statement by a Dr. David Whitsett, Professor of Psychology at the University of Northern Iowa, to the effect that he had witnessed Edwards' abilities in a classroom demonstration, and he was virtually convinced that they were paranormal. Dr. Phillips accordingly decided to fly the young man down to St. Louis to see for himself.

During Edwards' "introductory" visit to St. Louis, Professor Phillips held a 45-minute session in his own home, to which several interested friends were invited. The meeting was fairly informal—a "look-see" rather than in any way a definitive test. The psychological conditions were purposely kept relaxed and without excessive skepticism, as it is widely thought that such attitudes are not likely to be conducive to psi. Edwards demonstrated what *appeared* at the time to be impressive feats of psychokinetic deformations of keys and spoons. Regrettably, video equipment was not available on that occasion to provide a visual record of the proceedings. But certainly, more detailed investigation seemed warranted.

ENTER, MR. RANDI

A month later, Dr. Phillips received a letter from James "The Amazing" Randi. Born in Canada as Randall James Zwinge, Randi is a well-known conjuror. In fact, his Diner's Card states his occupation as "Professional Charlatan"! He was also until very recently a member of the Committee for the Scientific Investigation of Claims of the Paranormal (CSICOP)—an organization devoted to promulgating a highly skeptical viewpoint concerning purported anomalies such as PK (as well as such nonparapsychological claims as those surrounding astrology and UFOs). Some time ago, Randi issued a \$10,000 challenge, offering that sum to anyone who could demonstrate to him, under a protocol mutually agreed upon in advance, a genuine paranormal phenomenon. Despite a number of applicants, Randi has not yet been satisfied that any of them has succeeded. Moreover, he appears in at least some cases to insist that if a person does not perform psychically on an occasion when they are under the protocol conditions, then it is a valid generalization to say that therefore they do not perform psychically on *any* occasions under *any* conditions (Collins & Pinch, 1982, pp. 156-157).

In December 1979, Randi wrote Professor Phillips a courteous letter

requesting information about the MacLab's research to date, ostensibly for a report in CSICOP's magazine *The Skeptical Inquirer*. He also offered help and advice from the Committee regarding the PK studies, adding (somewhat ironically, in view of subsequent events), "nor is there any intention of the Committee to interfere with your work in any way" (Randi, personal communication, December 30, 1979).

Dr. Phillips replied shortly after, equally courteously, informing Randi that research was at much too embryonic a stage for a report. He emphasized that preliminary reports of work done under informal conditions too easily lead to misunderstandings about the competence of parapsychologists. (How prophetic this statement turned out to be!) Randi (personal communication, January 19, 1980) sent four more letters to Phillips over the next 10 months, inquiring about progress, reiterating his offer of help, and sending his list of 11 "Precautions Suggested for Examination of Psychic Claims in Children." Perhaps the most controversial of these precautions is #8: "Above all, a conjuror *experienced* in such matters should *always* be present, whether objected to by the subjects or not." The express purpose of Randi's pursuit of this matter was supposedly "to prevent you from conducting tests that will not stand scrutiny of critics and to provide you with the information that you need to properly control the experiments" (Randi, 1980).

Professor Phillips felt no need to reply to these four letters; nor did he at this stage adopt the suggested precautions or invite Randi to collaborate in the research, even though Randi had offered to visit St. Louis at his own expense. Mr. Randi has charged that there were two reasons why his help was refused. First, "because most of those who were offering assistance lacked academic credentials" (Randi, 1983a). Though it is true that Randi lacks such credentials, it is nonsense that that was a reason why the MacLab refused his offers, especially as later on the Lab indeed did accept some of his advice and in fact actively sought it on occasion. This red herring led to an unnecessary misunderstanding in which a local St. Louis magician, having heard Randi's claim, told the MacLab staff ruefully, "Oh I guess you wouldn't be interested in *my* advice: I don't have a college degree." He was quickly set right. Randi likes to debunk psychics but shows even more relish for "debunking" professors, for whom he reserves special opprobrium if they fail to take him sufficiently seriously.

Randi's second purported reason can be summarized in his statement that "scientists think they are too smart to be fooled." This claim is certainly not something I have heard any parapsychologist make, but of course the attitude may be evidenced in other ways, such as a refusal to accept advice and constructive criticism from *any* quarter. The extent to which this charge is true of the Director of the MacLab and his staff will become clearer as we proceed in this account.

Dr. Phillips, in fact, gives quite different reasons for his silence at that time. One was that despite Randi's courteous letters to him, the magician had (and still has) a reputation among parapsychologists as an unduly

vociferous and occasionally irrational skeptic possessed of an unfortunate tendency to distort the truth so as to obtain favorable publicity for himself and his crusade against psi. In short, Randi is a showman rather than an unprejudiced critic. Parapsychologists find it difficult to see the merit of working with someone who makes a living out of debunking paranormal claims and who would in fact suffer loss of face and finance if psi came to be more widely accepted as an established scientific fact. (Randi frequently puts the evidential status of ESP on a par with that of unicorns and Santa Claus.) Randi has admitted that he makes a good living out of such debunking through his books and speaking engagements. A second reason is that Randi is violently and implacably hostile towards persons whom he deems to be fraudulent psychics; yet many reasonable persons believe he tends to reach the verdict of "pseudopsychic" much too quickly and on insufficient evidence. Many parapsychologists believe that he is so incapable of suspending his belief that the claimant is *not* psychic that his attitude will spill over into interaction with the subject and be counterproductive. There are many human situations where the presence of antagonistic observers can be detrimental to performance in some way, auditions and sexual behavior being cases in point. The St. Louis physicist thus preferred to seek the help of parapsychologists. And though he certainly did not dismiss Randi's suggested precautions as useless, he decided that during the *exploratory* phase of research they were collectively so much overkill, to be used by someone more paranoid about being deceived than dedicated to creating optimal conditions for psi to occur.

ANOTHER METAL-BENDER?

Meanwhile, another apparently promising psychokinete had appeared on the scene: 18-year-old Steve Shaw of Washington, Pennsylvania. Edwards had mentioned reading about Shaw in an Iowa newspaper, and, "coincidentally," the MacLab received a letter from Shaw within a week, sometime during January 1980. Arrangements were eventually made that the two young men would be flown in for a weekend of investigation in mid-February.

During this joint visit there were three sessions, each two hours long. As a move towards greater experimental control, a professional video expert was employed, and each session was held in the TV Studio at the George Warren Brown School of Social Work on the main campus of Washington University. By this time, Dr. Phillips had been joined on the MacLab staff by Janet Jungkuntz, whose usual role was that of Laboratory Manager—a combination of administrator and secretary. She possessed no scientific training but rather acted as a nontechnical assistant to Dr. Phillips in his investigations.

Compared to the informal performances Dr. Phillips had witnessed prior to these filmed sessions, the effects observed on this occasion were, he says, discouraging. A great deal of time had to be put in before any

metal-bending events of potential interest occurred; and, frustratingly frequently, what events did occur tended to happen off-camera, defeating the object of this newly introduced and tighter condition. The young men complained that the electronic equipment and the impersonal studio dampened their abilities. Another weekend visit was therefore arranged for late April, in which a protocol similar to the previous one would be used except that the sessions would be held in a private home-which would perhaps provide a warmer atmosphere.

But the results during the April visit were even more disappointing. Fewer effects were captured on camera, and even the video record was of poorer quality. Discouraged by the difficulty of eliciting the phenomena on camera, Dr. Phillips thought seriously of abandoning the project. Just as there are gifted teachers who are able to reach students in ways the other teachers cannot and physicians with wonderful bedside manners and the knack of putting a person at ease whereas other doctors are aloof, so parapsychologists entertain the notion that some experimenters are psi-conducive and others psi-inhibitory. Dr. Phillips thus began to wonder whether, if the quantity of the phenomena exhibited by the young metal-benders was declining, he himself might be exerting some inhibiting effect, or at least that he might not possess the "green thumb" for cultivating psi. Thus, from May 1980 until March 1981 inclusive-a period of 11 months-no research of any kind was carried out with the two young men.

A NEW RESEARCHER TAKES OVER

September 1980 saw the arrival of a new researcher in the person of Mark Shafer. Shafer was in the process of finishing his Ph.D. in psychology from the University of California at Irvine and had conducted laboratory research on psi. Moreover, he had a strong interest in paranormal metal-bending, and he had run a group for metal-benders in California. He was convinced of the existence of the phenomenon and believed that he possessed a personal style that could elicit macro-PK.

Dr. Phillips asked Shafer to take over the research with Edwards and Shaw. This was a critical turning point in the project. As formal research on phenomena that had witnessed a decline was obviously a poor bet, the appropriate strategy seemed to Shafer to be to re-elicite the phenomena and, at the same time, find out the range of feats that the ostensible psychokinetics could perform. If they could bend metal by the power of their minds, why not move objects or affect equipment or photographic film? Shafer also tells me that he assumed that Professor Phillips had made sure that the subjects were genuine and not fraudulent. Certainly, though incontrovertible scientific evidence was lacking for the paranormality of the Edwards/Shaw phenomena, Dr. Phillips was *personally* persuaded as a result of his exploratory sessions that the youths were genuine. This private belief was tacitly communicated to, and was eventually shared by, Mark Shafer.

The research thus continued as exploratory. Shafer was convinced that in the absence of obvious evidence for fraud, the best attitude an experimenter could adopt was one of warm trust without any overt suspiciousness or hidden cameras or traps. For him, this was a *sine qua non* for psi to occur. For a time, he also preferred to work as solo experimenter, believing the presence of others to be potentially inhibitory. (This practice was soon discontinued; even exploratory research is usually better for being witnessed by more than one researcher.)

In his wide-ranging exploration of the youths' capabilities, Shafer was able to observe and videotape not only metal objects bending but a rotor turning under an apparently secure glass dome and a small alarm clock sliding across a table without apparent contact from Shaw. Heeding the anecdotes that fuses seem to blow more frequently in the houses of psychics, a first attempt at building a "fuse-blowing" device was made. This novel PK-testing device was built by Michael McBeath, who had joined the staff in August 1980—just a month before Shafer. McBeath had a joint degree in engineering and psychology as well as a masters degree in instrumentation. With this early machine, an electrical voltage of increasing magnitude was applied across a fuse, which eventually blew. A subject's task was to cause, paranormally, the fuse to blow earlier than an average time that had been determined from the outcome of many control trials. Edwards and Shaw were able to cause the fuses to blow more quickly in a quite dramatic way. It was realized, however, that there were some inadequacies in the experimental protocol, and improvements were constantly being devised. Helpful advice in this matter was obtained not only from Randi but also from parapsychologist and magician Dr. (now Professor) Robert Morris. For instance, a fuse device completed two years later had not one but two fuses at a time, with a computer to select at random which fuse was to be the target for that trial and which the control. The computer also controlled the application of the voltage across the fuses and objectively and precisely recorded the blow-times. In these later experiments, the ostensible psychokinetite was not permitted to touch the fuses, either before or after blowing, and each fuse was identified with a daub of special paint that was kept under lock and key. In such ways, the researchers sought to minimize the possibility of the subject substituting a fuse that had been tampered with or prepared beforehand. This is not an exhaustive list of the formal conditions later imposed (see Shafer, McBeath, Thalbourne, & Phillips, 1983), but even so, it should give the reader some idea of the difference between exploratory research and formal study.

During the period from March to July of 1981, Edwards and Shaw made three joint visits to St. Louis. On the May visit, at a time when several other ostensible psychics were also in St. Louis as guests of the MacLab, two unusually bizarre episodes occurred. On two occasions, at night, when (it was believed) no one was on the Lab premises, a variety of metal objects were bent and discovered so by the staff next morning. It was not

clear by whom or by what means these events had been carried out. Peter Phillips later wrote of the episode: “We explained to Shaw and Edwards that video records of the process in action were what we needed, and set up targets and a camera the following night. There were no further disturbances of this sort” (Phillips, 1983b, p. 2).

EXPLORATORY VERSUS FORMAL RESEARCH

It is time to halt for a moment in our chronicle and render explicit some methodological and philosophical distinctions relevant to investigations of this kind. Why, it may be asked, were not rigorous, Randiesque precautions against possible fraud utilized from the very first meeting? One reason is that formal conditions are generally more difficult, more time-consuming, and usually more costly to assemble than are informal conditions. One would be misspending one’s resources to insist upon setting up these formal conditions—which may, for example, involve expensive equipment—before getting some preliminary idea as to whether the phenomenon one is after gives some appearance of being present. If a new acquaintance, met at a party, claims to be able to bend a fork psychically, and one playfully challenges the person to demonstrate, to see what he or she can do, one cannot call this a proper experiment, but at the same time it may yield useful information—an indication as to whether to stop there or to go on. If “something,” even of a currently ambiguous nature, seems to be happening, then the researcher proceeds to narrow down the several explanations possible, with due regard for the fact that a complex, conscious human being is being studied. It is essential to bear in mind this distinction between “exploratory” and “formal” research. Much later, when Randi criticized the MacLab research with Edwards and Shaw, he made the error of neglecting to observe the distinction between these two styles of research with their different objectives.

This methodological distinction has existed in parapsychology for decades. J. B. Rhine wrote of it in the mid-1960s, noting an additional benefit of informal research:

A large range of research needs to be carried out under relatively free exploratory test conditions. . . . Heavy emphasis in present-day research in this field is on the provision of appropriate psychological conditions, aimed at favoring good [psi] test performance by the subject. . . . Free exploratory conditions permit a wider range of possibilities for this important condition. Better controlled conditions can then be introduced when the stage is reached at which it is worth while and important to increase the precautions against counter explanations. (Rhine, 1964, p. xl)

Of course, whereas formal research should be conducted under the most impeccable conditions possible, even exploratory research may contain one or more security conditions, perhaps in increasing number as the investigation proceeds (rather than jumping straight into the most rigorous

conditions of the formal, confirmatory phase). For example, in describing how he intended to investigate ostensible psychokinetes, Dr. Phillips told a reporter in 1979:

I let them do it first in their own particular style . . . Then they gradually learn to refine it and make it more foolproof. For example, first they may say, "Well, I can only do it in my own room when no one's watching." Obviously, that's no good. We would work with them until first they can do it in the presence of others, then perhaps without touching the object they are affecting. Then maybe they would work up to the point where they can affect the object when it's covered by a sealed glass cover. (Swanson, 1979)

The distinction between formal and informal investigation—between exploratory and confirmatory—is not confined to psychical research. Take, for example, geologists searching for some valuable mineral such as oil. They survey the land (or sea), choose the areas most likely, on the basis of its appearance and their knowledge, to be oil-bearing, and recommend exploratory drilling in the most promising places. As is all too well known, sometimes oil is found, but often it is not. The "formal" setting up of an oil well depends upon the results of the exploratory search.

A second parallel may be drawn from the autonomic conditioning work of the distinguished psychologist Neal Miller. Having discovered an important and apparently reliable phenomenon in the psychology of learning, after a large number of studies the effect "disappeared" and could not be replicated (see McBeath, 1984). Miller therefore decided to switch research strategies from formal to exploratory. In 1974 he wrote:

In investigating the possible therapeutic applications of visceral learning, most investigators, including myself, have adopted the strategy of concentrating first on trying to produce a significantly large and permanent effect, deferring the time-consuming effort of running suitable control tests until we are sure that we have a phenomenon to control for. (Miller & Dworkin, p. 325)

One final analogy is from the world of medicine. In a case of which I know, a dentist discovered inside his patient's mouth a small, unusual area of white termed "leukoplakia." A second opinion was sought from an oral surgeon at the finest hospital in St. Louis. His reaction was one of great concern, and he recommended a biopsy. Much later, the patient learned that the surgeon was, privately, 99% sure that he had cancer. But imagine the patient's relief when the pathology tests revealed that the surgeon's opinion was wrong, and that all he had had was a similar-looking but innocuous form of dermatitis. All scientists formulate tentative hypotheses on the basis of preliminary data: They assign differential probabilities to those conjectures and work with them as best they can to obtain confirmation or disconfirmation. It is not uncommon in any part of science for these early conjectures to be wrong. More important than an occasional error is the ability to *correct* that error.

THE POSSIBILITY OF SUBJECT FRAUD

When one witnesses an ostensible paranormal phenomenon, it goes without saying that a scientific attitude will demand postulating the possibility that one has seen not psi but some form of deception. Mentalists and magicians ply their trade by simulating apparent marvels of telepathy and psychokinesis. Many psychical research investigations have been salutary (and acceptable even to the staunchest critic) inasmuch as they have exposed the fraudulent activities of false mediums and other charlatans. History has been such that now, when a newcomer presents him- or herself as psychic, the possibility must be considered that the person's abilities are bogus.

If an *experimenter* is caught cheating, then it is customary to treat *all* that researcher's data as "unreliable"; that is, the data are "excommunicated" from the corpus of scientific information. As it is usually not known for sure whether the worker tampered with all their data or just some, the conservative view is generally taken that all the data must be regarded as suspect. Experimenter fraud is a drastic event and usually spells the loss of reputation and the end of a scientific career, harsh though that verdict may seem to some (see Rhine, 1974).

Now consider the case where an ordinary person tells a white lie, fails to declare all their income on their tax return, exceeds the speed limit on an interstate highway, or photocopies an entire book. Are we justified in saying of such a person that as he or she has been dishonest once, it is reasonable to assume that the person is always dishonest? For certain persons this may be so, but usually we do not deem it a gross error of logic to suppose that for most of the time such a person can still be a reasonably honest, reliable, and law-abiding soul: *errare humanum est*.

Now take the case in which a *subject*, after a series of parapsychological studies of different kinds, is caught cheating in an experiment. What do we conclude about the subject and about his or her performance in the previous tests where no fraud was detected? Even at this time, parapsychologists have not reached a consensus on this controversial issue. The strictest and most conservative point of view is that such subjects, along with all their data, should be treated no differently than a fraudulent experimenter. All work with such a subject should cease forthwith and an "expose" published. Such an opinion would probably be endorsed not only by Randi but also by many psychical researchers. If, for instance, the Israeli stage performer Uri Geller is even once discovered using conjuror's tricks, then no matter if at least three magicians have reported observing genuine anomalous phenomena around him on other occasions (Cox, 1975; Zorka, 1976; A. Dickson, personal communication to U. Geller, June 3, 1975), the view would hold that none of Geller's feats can be considered scientifically anomalous: he would thereafter be a "proven fraud." It is too dangerous to allow the latitude that there might be real psi phenomena amongst the trickery.

One bizarre implication of this conservative strategy is that it implies that following discovery of fraud, there does not thenceforward exist a set of experimental conditions stringent enough to permit conclusive demonstration that the person really *has* produced psi. One fraudulent action can destroy forever the ability of competent scientists to preclude fraud with that subject. Nevertheless, this conservative viewpoint tends to prevent pseudopsychics from being given the stamp of scientific approval.

A different viewpoint identifies the subject more closely with the ordinary person who commits some dishonesty: Though the subject may in point of fact be a thoroughgoing rogue all the time, this viewpoint recognizes that a single misdeed (or even several) does not suffice to prove that chicanery occurs on all test occasions. A good many parapsychologists agree that there have been a number of persons in the past who would cheat shamelessly if given half a chance but who could nevertheless sometimes produce the most extraordinary paranormal phenomena under the most rigorous of conditions. This *tertium quid* has come to be known as “mixed mediumship.”

One might fear that this notion would prevent a researcher from ever concluding that a prince is but a pretender. Not so. If deception occurs often enough, and if there is an utter dearth of evidence of psi under optimal and rigorous conditions, then the weight of inductive logic would probably tip the scales towards the conclusion of outright fraud. If, however, a researcher believes that a subject has produced convincing evidence for psi elsewhere, then the discovery of a fraudulent act does not necessarily vitiate the results of other tests conducted under different conditions. This was Mark Shafer’s attitude when he discovered Michael Edwards surreptitiously re-pushing the starter-button of a psychokinesis testing device known as a Schmidt Machine (on the off-chance that a higher score would result). Detection of any fraudulent act should, of course, lead to greater wariness in the future and a more skeptical attitude towards results thenceforth obtained under informal conditions. It could even be argued that if formal research has not occurred already, then it is high time for at least some such to commence. If a subject has cheated once, he or she may try it again. *Caveat investigator!*

THE CONSERVATIVE VERSUS THE EXPANSIONIST

Take any case of an alleged psychic phenomenon. Is it a genuine case of psi? Or can it be explained without resorting to that controversial, and indeed subversive, hypothesis? The process of evaluation is the attempt to arrive at a yes or no decision about these questions on the basis of the evidence and our reasoning. Owing to the difficulty of this process, such decisions may of course be right or wrong. The four possible combinations of these state of affairs are: (a) correct acceptance that it was psi; (b) correct rejection of it as nonpsi; (c) incorrect acceptance of the event as psi; and (d) incorrect rejection of the event as nonpsi.

Every good parapsychologist wants to maximize situations #1 and #2 and avoid the errors of #3 and #4. Statisticians have names for mistakes analogous to #3 and #4. The former is known as a "Type I Error" and the latter as a "Type II Error." I suggest that scientists differ from each other in the extent to which they will risk making these sorts of error. In the one case—the "Type I Error" (if I may coin a term)—the researcher is more willing to risk false positives: He or she sees it as more undesirable to *fail* to discover genuine psi phenomena when they are really there than occasionally to accept the evidence as pointing to psi when, it turns out, psi was really not there. These researchers I term "expansionists." They prefer to spread their net wide in the hope of discovering novel phenomena and thereby conquering new territory for science. As Louis Lasagna (1984) puts it: "I'd rather err occasionally than miss a real live miracle" (p. 12). Expansionism is an essentially liberal scientific attitude.

In contrast, we have the "Type II Errors," who are extremely apprehensive about making false positives. I call these persons "conservatives" (or perhaps "conservationists"). Such researchers are more apprehensive about-or see as more costly-their making the mistake of saying that an event was psychic when in reality it turns out not to be so. They would rather occasionally miss out on the real McCoy than approve a fake. Better to have an incomplete catalog of nature than one bulging with interlopers.

Neither the conservative nor the expansionist attitude is "wrong." Rather, each involves making different types of errors about nature, in the one case of commission, in the other of omission. These stances may of course vary not only between researchers but also within one and the same investigator, depending upon the psychic claim under consideration. Yet, in general, it seems that a parapsychologist can be characterized as leaning more towards one attitude than the other. (Mark Shafer, I suggest, behaved as a classic expansionist, Peter Phillips somewhat less so.) Fear or tolerance of a "false positive" is, I strongly suspect, at the root of the different views taken on how to deal with fraud if it occurs in a subject.

THE 1981 PARAPSYCHOLOGICAL ASSOCIATION CONVENTION

One of the most important events in the year for a professional parapsychologist is the convention held annually in August by the Parapsychological Association (PA). Presented at such meetings are finished research, symposia, and reports of research in progress. With certain exceptions, the proceedings of the conference are subsequently published in abstract form in the annual *Research in Parapsychology*. But although it is a useful record of the convention, *Research in Parapsychology* is not considered to be on a par with a proper, peer-reviewed journal. All conference submissions are of course examined closely by the Program Committee, and a number are rejected, but in the nature of the case, and like many scholarly conferences, the standards are such as to permit both exploratory and formal results to be reported.

In 1981 the convention was to be held at Syracuse University. Phillips

and Shafer decided to submit to that year's Program Committee a report of the macro-PK work then in progress at the MacLab. A research brief was submitted and accepted for presentation.

The researchers also had plans to try to assemble a videotape containing the sequences they deemed most highly suggestive of apparent macro-PK. This would be shown at a "workshop"-a name which well reflects the unfinished nature of the material presented; in fact, workshops are not even published in *Research in Parapsychology*. The idea of the researchers was most certainly not that the tape would provide incontestable scientific evidence for macro-PK; rather, they wanted to obtain the opinions of the parapsychological community on how to go about improving their evidence in future work.

In early June 1981, Professor Phillips wrote to Mr. Randi asking whether the latter could provide him with a tape of *faked* psychokinetic metal-bending along with explanations of the tricks. The idea was to show the two tapes consecutively at the convention workshop. "We hope that this will enable us to make a better judgment about the strength of the evidence which our tape provides," wrote Phillips (personal communication to J. Randi, June 8, 1981). "If trickery is possible within our protocol then one should suspect that trickery was, in fact, used. That is why we want to educate ourselves in methods of deception, about which your tape will be most enlightening" (Phillips, personal communication to J. Randi, June 22, 1981). Randi generously assented, though requesting at the same time a pre-convention copy of the tape to be made by the MacLab so that he could study it with a view to making suggestions.

The research brief that Phillips and Shafer presented at the convention reflected the personal belief that each shared as to the probable authenticity of the events seen under exploratory conditions. They made it clear on the tape, however, that the evidence could not yet be regarded as *scientifically* acceptable: the tape was provisional, and advice was being sought from peers as to how to make a better one. Extremely strong criticism of the tape was expressed by the parapsychologists gathered. Mr. Randi later talked with the two MacLab researchers, discussing possible loopholes in the conditions and pointing out where fraud might have occurred. As a result of all this feedback from the convention participants, Phillips and Shafer came to the conclusion that their data were much less evidential than they had thought. They accordingly recalled the copies they had distributed of their research brief, inserting where appropriate such disclaimers as "apparently" and "ostensible." The abstract ultimately printed in *Research in Parapsychology* (Phillips & Shafer, 1982) reflected this conservative reappraisal. Edwards and Shaw, when they saw the revised abstracts, made it known that they were most upset at the doubt implied about their abilities.

THE RUMOR

At some stage during the convention, the rumor was heard that Edwards and Shaw were in fact conjurers sent by Mr. Randi to "infiltrate" the

MacLab. The rumor seemed unlikely to be true for several reasons: the two young men came from different states and had never met before being brought to the MacLab; if they were both conspiring with Randi, then the plot had gone on for 21 months. What critic would be so persistent in engaging in fraud and conspiracy on such a time-scale? There seemed to be no precedent. Nor was it possible to track down how reliable the rumor might be. There are some parapsychologists who aver that research with the two youths should have ceased forthwith because of the rumor. At last! A way of instantly causing someone's psi research to grind to a halt: just spread the rumor that there is a fraud planted in it. Rumors should have some evidence if one is to take them seriously. The fact that so many people did so even then testifies both to the ambiguous quality of the macro-PK evidence and to the belief that Randi was capable of anything.

The researchers thus returned to St. Louis neither believing the rumor nor dismissing it. Mr. Randi continued to be amiable and helpful, and he even offered faint praise, as in this letter of August 22, 1981:

I feel that your present efforts, even if not resulting in positive conclusions concerning sought-after phenomena, can be useful in that you demonstrate the ability of researchers to pursue a course diligently and properly to its end, and can offer (at the very least) caveats for future efforts in this direction.

Somewhat disappointingly, in the case of at least two requests for advice—one concerning a new fuse-device and the other on contacting competent local magicians—there is no record that Mr. Randi responded.

A request was made from a member of the press for information about Edwards for a forthcoming newspaper article. Dr. Phillips and Mark Shafer therefore prepared a public statement about *each* subject, dated September 1, 1981 (see Appendix for the statement regarding Edwards). These notices clearly stated that although the researchers had witnessed many unusual events, they were not confident that any of them were psychokinetic because alternative normal explanations could be found for them all.

On their next visit to St. Louis in October, Edwards and Shaw were told of the reaction at the convention, and it was made clear that only experiments of a formal kind would persuade the parapsychological community. They were told as well about the rumor, which they laughed off. They were not asked outright, "Are you frauds?" For if they had consistently lied on previous occasions, what reason was there to expect them to be truthful on this one? And, as we have discussed above, even an affirmative answer could conceivably be consistent with mixed mediumship. Nevertheless, a question relevant to the matter was indeed asked. During the youths' visits some piece of bent cutlery would usually be discovered in unoccupied rooms. Upon finding such a bent specimen in her office, Janet Jungkuntz brought it to Shaw and said, very jovially but nevertheless with serious intent, "Did you do that?" making a gesture to suggest bending it by all too normal means. Shaw protested his innocence. He later claimed that he understood her to mean bending it psychically. Yet no one else had

understood it that way. I am a witness to this, having been present on that occasion. (I joined the staff of the MacLab in December 1981.)

THE FINAL PHASE OF RESEARCH

The two young men were brought to the MacLab on just four more occasions. Altogether, however, in this formal phase of research they clocked considerably more hours than in the exploratory phase. If attempts at metal-bending were made, they were understood to be purely “home-work” exercises or “warm-ups” prior to a formal session: In no sense could effects under such conditions be considered proper scientific evidence. Only formal experimentation was to count: the aim was for good effects under unimpeachable conditions. The boys tried a test of precognition, with complete lack of success. They were tested on the new computer-run fuse-device: Shaw gave little evidence that he could influence the machine; Edwards showed what looked to be significant results, but they were very marginal. There was also a complicated picture-guessing ESP experiment with four conditions. The only results of note were in the conditions with Shaw as “percipient,” and again they were of a tiny magnitude; such marginal results could have been due to chance. Finally, there was an experiment in which saline solution sealed in bottles was held by the subject and later used to water rye-grass seeds. The seeds receiving the water thus “treated” by the boys actually grew to a significantly *lesser* extent than did the controls, which had been watered by untreated saline! If the duo still had psychic ability, it was not proving very effective.

These unspectacular results were reported in research briefs at the 1982 PA convention held in August in Cambridge, England (Shafer, McBeath, Thalbourne, & Phillips, 1983; Thalbourne & Shafer, 1983). The briefs were of course eventually published in the conference proceedings, but apart from that there were no plans to submit fuller reports to refereed journals. The methodology was more noteworthy than the results. When a newspaper requested details for a story about the ESP picture-guessing experiment, the reporter was told that the evidence was not strong enough to merit the conclusion that psychic ability had been shown. The information was thus withheld. Since Edwards and Shaw lived so far from St. Louis, it was deemed that it was no longer worthwhile to fly them in, if they were only going to produce such meager results. They would call the Lab every so often (reversing the charges) to tell us when they were available for future work and to suggest tests they would like to do. But they were gently dissuaded from raising their hopes; it was thought more appropriate that *other* researchers attempt to validate whatever talents they had. We did not conclude that they must be frauds, but only that after extensive testing, they were not behaving nearly so psychically as they had led us to expect. Though of course we sought to prevent fraud in the formal studies, we were not in the business of exposing a person caught in fraud.

We aimed instead to come up with reliable conclusions about the quality of a subject's psi performance under the best possible conditions.

A STRAIGHT SPOON AWARD¹

In the Fall 1983 issue of the *Skeptical Inquirer* there was a news item entitled "Uri Awards: A Straight Spoon Joins Three Bent Ones in '83." The word "joins" *might* imply to an unwary reader that this was the first occasion that a Straight Spoon Award had been presented. This would be an incorrect deduction, since several such awards were made in previous years.

A 1982 recipient of one of these awards appeared both in the nominations and in *Omni* magazine's news release announcing the awards on April 1. For some reason, however, his name did not actually reach print in the May issue of *Omni* or, for that matter, in the Fall 1982 *Skeptical Inquirer*. This recipient was Professor Peter Phillips. Professor Phillips had in fact received a letter from Randi, dated March 10, 1982, dealing in part with that year's forthcoming Uri Awards. The relevant passage reads:

My third Annual Uri Awards are coming up for presentation April 1st on behalf of *Omni* magazine, and I have mentioned your name-in a very kind and rather complimentary manner—in the award speech. [Y]ou are to be presented with a Straight Spoon Award for your cautious approach to your work, recently evidenced.

It is curious, however, that in the *Omni* news release (unlike in the nominations, earlier), Dr. Phillips was not mentioned by name but only as a "researcher at Washington University." Thus, at this point of time, Randi actually praised the MacLab and its research with the two boys. The tune, however, was soon to change.

THE RUMOR IS TRUE

In the last week of January 1983, a press conference was convened in New York City under the sponsorship of *Discover* magazine. There, James Randi announced that Edwards and Shaw were indeed conjurors sent by him to "test the defenses of the parapsychologists" (Randi, 1983a). The subjects in question were present to confirm the truth of the announcement.

Randi had code-named the whole scheme "Project Alpha," preferring to describe it as a "sociological experiment." The one "hypothesis" mentioned at the press conference was that the parascientists would be unable to set up decent testing procedures, and without a competent conjuror present they would be unable to produce and follow an adequate protocol for their tests (Randi, 1983a). In a later, written report this was

¹ This information in this section is taken from Thalbourne (1984–1985).

“rephrased” as “parapsychologists would resist accepting expert conjuring assistance in designing proper control procedures and, as a result, would fail to detect various kinds of simple magic tricks” (Randi, 1983b, pp. 24-25).

Note that Randi does not mention whether these statements are to refer to exploratory or to formal conditions. His hypotheses could be said to be true *only* if (a) he described exploratory work as if it were meant to be formal; and if (b) he omitted as far as possible any mention of the formal work and how proper conditions were designed and implemented *without* a conjuror’s presence, and, for the most part, without a conjuror’s assistance.

Randi proceeded to lambaste and lampoon the early work of the Mac-Lab, ridiculing the lack of security conditions. He thereby completely misrepresented the purpose of the exploratory work and seemed to assume that the only research worth doing was the formal kind: Any other sort was a sign of incompetence and money-wasting.

The revealing of Project Alpha led immediately to widespread media publicity for the hoax, from the *New York Times* and *Washington Post* to the *San Francisco Chronicle*, as well as the local St. Louis press. Randi was featured on numerous TV and radio shows—and is still, as of this writing, being invited to many engagements as guest speaker. Several science magazines, such as *Discover* and *Omni*, eventually carried the story.

ETHICAL PROBLEMS WITH PROJECT ALPHA

The most obvious ethical issue that comes to mind in the case of the Randi Hoax is that of the extensive use of fraud. Certainly, a number of actions committed by Shaw and Edwards would probably be considered criminal offenses. Take, for example, the nights of the unexplained metal-bending at the Lab: The pair had left a window unlocked, returned in the middle of the night, made unauthorized entry, and physically bent all the objects subsequently found.

If it is correct to think of Alpha as in any sense a genuine experiment, then a certain amount of deception might be justifiable on the grounds that deception is sometimes necessary in many social science studies. Psychology students become so accustomed to such “cover-stories” and the like that they expect it when they are subjects. The problem with this argument is that James Randi is neither scientist nor academic (as he cheerfully admits), and therefore does not feel in the slightest way bound to follow the same ethical guidelines that have been evolved in research institutions to protect subjects from exploitation and other harm. (This unwillingness is an excellent reason why he should not be asked to collaborate in parapsychological research.) At Washington University, all proposed research with human subjects must pass the rigorous scrutiny of a special commit-

tee. It seems most unlikely that Mr. Randi's "experiment" would have been approved. Reports William Broad (1983) in *The New York Times*:

Indeed, if Mr. Randi were a psychologist, the hoax might have landed him in hot water. "It sounds like something that would be in our domain," said Dr. David Mills, director of the Ethics Office at the American Psychological Association. "Censure is used by the committee when they feel there is some damage."

If Randi wishes to enjoy the advantages of a scientist—such as speaking before the American Association for the Advancement of Science—then surely in carrying out his psychical investigations he should adhere to the ethical standards of scientists: these are formulated in order to protect people who have been experimented upon. Yet if Project Alpha is *not* to be construed as a scientific experiment, then what was it? A publicity stunt to attract favorable attention to its architect and a way of discrediting parapsychologists? What are we to make of the following report by Ron McRae? "First McDonnell and then, I hope, the other so-called prestigious parapsychology laboratories will endorse M.E. and S.S. as genuine," Randi boasted . . . , thus "discrediting parapsychology research all over the world" (McRae, 1984, p. 38).

But perhaps the most repugnant of Mr. Randi's actions in the whole affair—and the one that alienated not only most parapsychologists but also many critics otherwise sympathetic to the magician—was the way he chose to terminate and reveal his hoax. The humane way to conclude his project would have been to go to Dr. Phillips with his findings, and then perhaps to submit a paper (with keynames changed) to a journal that would be open for evaluation by his peers (cf. Rosenhan, 1973). Only after that ought the public at large be informed. Instead, Randi went straight to the public, calling a press conference in New York. He thereby guaranteed the maximum possible media coverage for himself. More dubious still was the fact that Peter Phillips was not invited to the press conference to give his side of the story. It seems too much of a coincidence that Randi and his hoax were scheduled to be featured on a television special less than two weeks later. Many months after, Randi did publish a report in the *Skeptical Inquirer* (Randi, 1983b, 1983c), but not before selective, idiosyncratic, and frequently erroneous statements unflattering to parapsychology were made in the press nationwide.

It is not surprising, then, that eventually Randi's *motivations* for the hoax were more widely discussed than the hoax itself. Is Randi genuinely concerned to assist parapsychologists? Or was he using his knowledge of conjuring merely to place himself in the spotlight of publicity as a self-proclaimed expert on psi testing? In short, did he behave more like a scientist or a showman?

One incident that may throw light on these questions concerns the two public statements about Edwards and Shaw drawn up immediately after the 1981 Parapsychological Association convention (see the Appendix for the

one about Edwards). These, it will be remembered, stated that the evidence for paranormal ability in the boys was inconclusive because alternative explanations existed for the phenomena thus far observed. These were mentioned in the press release issued in response by Washington University the day of Randi's press conference. They were examples of the important distinction between a scientist's personal conviction and the statement he makes public to the scientific community. They undermined Mr. Randi's claims. Rumor has it that he was extremely upset when he learned of the statements. He even went so far as to suspect that the documents had been forged by Washington University's Public Relations office the day of his press conference. It seems that those who deceive expect to be deceived. Dr. Phillips told the magician in a letter:

I must admit to being puzzled at the difficulty you evidently have in accepting our statement for what it is. It shows that we were much more sceptical and cautious about Mike and Steve than you believed. But for that we deserve your commendation, surely? (personal communication, February 9, 1983)

Mr. Randi eventually accepted the authenticity of the statements. Indeed, he later went on to claim the entire credit for the MacLab's "sudden conversion" to cautiousness.

SOME POSITIVE CONSEQUENCES OF THE HOAX

At least in private correspondence, if not in public, Randi repeatedly conveyed the message that the MacLab had "passed the test" and "recovered itself more than adequately" (personal communication to P. Phillips, January 31, 1983). He has stated, and his young men have signed declarations to the effect, that they were not able to cheat under the formal conditions imposed. So, first on our list of positive outcomes might be the observation that despite a determined conspiracy by three competent conjurers, they were not capable of hoaxing the MacLab to the extent that it published official scientific pronouncements in refereed journals that their powers were real. The scientific method prevailed to prevent the making of a false positive: no new deities were accepted into the parapsychological pantheon. Randi claims that this was *only* because of his intervention in various ways. In so claiming, he ignores other relevant factors, such as the importance of criticism and feedback from fellow parapsychologists and the fact that some relatively formal experimentation had begun even before the rumor had been heard about his pseudopsychics.

Secondly, although parapsychologists have consulted magicians before in their work with ostensibly gifted subjects, it is true that this had not occurred very often prior to this time. Though many protocols for testing apparent psychics are *probably* quite adequate, the realization has now crystallized that an appropriate magician may in some types of experimentation have something valuable to contribute, either in helping to set up

protocols, or more often, by *ratifying* the soundness of the existing controls. A report that mentions that such a consultant was brought in will carry more weight with the critics, and thus it should cause them to take the results more seriously. Nevertheless, there would still be problems: For example, some magicians may question the competence of the consultant--especially if positive results are claimed, but these difficulties in principle should be solvable.

At the 1983 PA convention, the Council adopted the following resolution:

Historically, parapsychologists have availed themselves of the services of experts in fields relevant to their own: statisticians, engineers, and—on occasion—magicians. The PA welcomes collaboration with magicians who, by their past behavior and membership in respected organizations, have maintained high standards of professionalism and have adhered to the ethical code of the fraternity of magicians. We suggest that it is disadvantageous to both parapsychologists and conjurers to interact with magicians who do not meet these criteria and who would exploit such an interaction for personal gain. Therefore, the PA Council has voted unanimously to request from organizations such as the International Brotherhood of Magicians, Society of American Magicians and the Psychic Entertainers Association a list of their members who, regardless of their opinions on the existence of psi, would be willing to consult with PA members regarding adequate controls against fraud. We look forward to a fruitful professional relationship with these individuals.

Thus, for all the dubious aspects surrounding Randi's hoax, *something* positive has ensued for parapsychology. The irony is that by his duplicity and sensationalism, Randi has, in the minds of virtually all parapsychologists, disqualified himself from participating in the process he helped to catalyze.

POSTSCRIPT 1995: TWELVE YEARS ON

Though over a decade has gone by since the public revelation of the Randi Hoax (otherwise known as Project Alpha), its architect James Randi continues to make use of it for entertainment and other purposes. One such use was in an introduction to a performance by Steve Shaw at the banquet of the Dallas meeting of the Committee for the Scientific Investigation of Claims of the Paranormal (CSICOP), held October 16–18, 1992. I have in my possession a transcript of most of this introduction. Given the nature of both his topic and his audiences it comes as no surprise that Randi chose to pour ridicule and sarcasm upon the efforts of parapsychology. More disturbing, however, is the distorted version of events he gives surrounding the Hoax. In particular, and among other incorrect statements, he implies that the MacLab failed to consult him until *after* the Hoax was revealed, and that after consulting him no more experiments were conducted. Both claims are entirely false. Faced with such ongoing misrepresentation, and

in spite of the late date, this paper has been published in order to have on public record the point of view of the MacLab. In his introduction Randi refers to his faulty memory, and we can only hope that it doesn't continue to cause him to misdescribe the facts surrounding his Hoax, as he did on this occasion. As Peter Phillips commented, "when people misremember something they usually do it in such a way as to make themselves look better," quoting Shakespeare's *Henry V*, "he'll remember, with advantages, what deeds he did that day."

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APPENDIX

McDonnell Laboratory For Psychical Research
 PUBLIC STATEMENT ON RESEARCH WITH MIKE EDWARDS
 September 1, 1981

On 6 occasions between late 1979 and July 1981 Mike Edwards participated in exploratory research on observable psychokinetic phenomena at the McDonnell Laboratory for Psychical Research. The outcome of this research is suggestive of psychokinesis but inconclusive, due to its explor-

atory nature. A research brief delivered at the Parapsychological Association Annual Convention in August 1981, at Syracuse University, mentions several events that have occurred, including influence on standard keys, Polaroid photographic film, and electronic fuses. However, ordinary explanations exist for these effects, given the conditions under which they have been observed. Thus, although several events of interest have transpired, we do not claim that evidence conclusive of "psychic ability" has yet been demonstrated in our research. We hope that in the future we and others will be able to conduct tests under conditions which will allow us to draw more definite conclusions.

Peter R. Phillips

Mark Shafer

McDonnell Laboratory for Psychical Research