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INVITED ESSAY

Arguments Over Anomalies: II. Polemics

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Abstract—Arguments over different anomalies have common elements. An awareness of those commonalities can be useful in considering the possible reality of a particular anomaly. As in all arguments, beliefs and not facts are at issue; but the participants do not recognize that, and so red herrings abound and opponents are not persuaded. Again as in all disputes, the longer the disagreement persists, the more polarized the issue becomes, which further encourages the antagonists to become preoccupied with irrelevancies. Within science, disputes are to some degree constrained by the existence of a widely shared paradigm and by widely accepted conventions, supported by entrenched institutions and by consensus over how and when disputes become settled; but arguments over anomalies are not so constrained: they are messy and may continue long after they—on purely epistemic grounds—"should". Insofar as arguments over anomalies take place in the public domain, they involve not only proponents and opponents but also pundits and an audience; however, a purported pundit may behave more like a disbeliever (or, more rarely, like a proponent). Some features of these arguments result from the fact that the believers are usually amateurs (though they commonly include a few maverick experts in the presumptively relevant fields of mainstream science). Although most of the experts tend to be disbelievers or at least non-believers in a given anomaly, the converse is by no means true—most of the disbelievers have little or no expertise in related areas, and they may not even be particularly knowledgeable about the given anomaly. Typically, both sides claim that the evidence is already conclusive when—virtually by definition—it is evidently not. Believers tend to close ranks, even with quite unwelcome bedfellows, for fear that their subject will seem even less respectable if the existence of frauds or hoaxes or incompetence becomes widely known; and that enhances the tendency for outsiders to view the believers as unanimous on all major points, which is anything but true. Both sides (and also the pundits) typically appeal to the authority of science; and typically they misunderstand the nature of science. Also characteristic of these arguments is ignorance of matters that (but only by hindsight) are highly relevant.

Introduction

If one's interest is in the truth about a given anomaly, is there any reason why one would take an interest in what may be common with other anomalies? Is not the possible reality of a Loch Ness Monster quite unrelated to the possible reality of a Bigfoot, let alone to the possible reality of clairvoyance, say?

Quite generally, recognition of commonalities can deepen one's understanding of singularities. In chemistry, for example, an appreciation of the periodic law provided firm ground for deciding whether a claimed new element was real or only a pseudo-element, for predicting the existence of yet undiscovered elements, for understanding chemical behavior and atomic structure: knowing what is common to elements results also in a deeper understanding of individual elements. So too can knowledge of what is common among anomalies help in the study of any given anomalous claim. The study of anomalies as a class was dubbed by Wescott (1980) "anomalistics"; but this seemingly useful term has not come into general usage. A number of important generalizations within anomalistics have been ventured, notably by Truzzi (1977, 1987) and Westrum (1976, 1978 [with Truzzi], 1982).

Arguments over anomalies have in common that they are controversial (Bauer, 1988); and they exemplify misunderstanding of scientific activity and certain typical illogicalities.

In "scientifically exploring" anomalous phenomena, it is well to recognize that controversiality has different consequences in these matters than in the intra-scientific disputes with which scientists are familiar. For instance, in science there is always tacit agreement that any matter will ultimately be settled when enough of Nature's facts become understood, and so there can often be a consensus to leave the matter undecided until then; whereas with anomalous phenomena one finds both sides arguing that the crucial facts are already in. In science, resolving controversial issues involves chiefly substantive matters of method and theory and data; over anomalies, resolving the issues means chiefly or primarily disentangling arguments. In science, the relevant data are accessible to all, whereas with anomalies one often has to be content with the hearsay of others about the data that are claimed to exist (or to have once existed). In science, the experts are readily identified and are (within their specific specialties) generally trustworthy, whereas with anomalies it is never obvious who is expert, and some of the most knowledgeable about a given matter may be quite untrustworthy and not even particularly competent in their knowledge.

Arguments in General

Beliefs, not Facts are at Issue

Arguments are over differences in belief but are carried on as though they were over facts. Arguments are attempts to persuade people to change their opinions, but the protagonists typically seem not to understand what must happen if beliefs are to change. Most of us know little about how human beings acquire beliefs or about what causes us to maintain or to change beliefs once we have them. We tend to assume that we believe a thing because it is true and we tend further to assume that the truth of the matter is

convincingly demonstrated by looking at the facts. Consequently we cannot comprehend how other people can fail to share our own convictions.

Forming Beliefs

In actuality, however, our beliefs are formed under a variety of influences. At first, we are enormously influenced by parents, peers, and teachers, and we act and react out of emotion rather than intellect; only after a time do we learn to become somewhat analytical and critical, to choose opinions for ourselves among the available alternatives or to generate genuinely individual ones. Thus at first we typically follow our parents' and our society's religious and political and social inclinations—or we reject them and embrace diametrically opposite views; only later and only occasionally do we evolve significantly individual opinions through judicious thought. So too in formal and higher education, we begin by accepting what the instructors and the texts say and only later become able to judge and to investigate matters for ourselves. (And even then we usually maintain to some degree the biases of those with whom we learned: that is why, for instance, one can identify research traditions associated with particular laboratories or eminent individuals, and why scientific genealogies are constructed and are instructive.)

Beliefs and Evidence

Since each of us grows and learns under a unique set of circumstances, we become predisposed to find plausible different things, or a given thing to different degrees. Each of us forms opinions on specific issues through some interplay between individual predispositions and the available evidence; since the predispositions vary, we reach different conclusions on the basis of the same evidence. Yet, because we harbor the illusion that our opinions are formed by the evidence, we argue as though only the evidence were at issue; simply because we happen to believe, we assume that we have only to make the evidence clear whereupon others will adopt the same belief. Therefore arguments typically focus overtly or apparently on the evidence; yet that is only one of the factors that influences belief and often not the decisive one. That has important consequences. For participants, it means that they usually argue ineffectively: to alter the antagonists' opinion, it would need to be shown that the evidence *as seen in the light of their own prejudices* can lead to our conclusion; or that their prejudices are inappropriate in this instance; or that their prejudices should altogether be altered (and none of those would be easy, of course). For observers or analysts of arguments, it is necessary to look not only at the purported evidence but also to identify the biases that inform the opposing sides; without that, it may not even be possible to determine what the actual evidence is.

In considering the available evidence we usually fail to make the crucially important distinction between weight of evidence adequate to support one's

own belief and weight of evidence adequate to convince others. Since preconceptions vary, estimates of plausibility and probability also vary: what seems likely to some seems quite unlikely to others—for example that human behavior is genetically controlled, or that God exists, or that acupuncture could work. When something occurs that seems *to us* plausible, we accept it even on quite slender evidence (or actually no evidence at all, for example the pre-1956 belief that parity would be conserved [Bernstein, 1967, p. 52]); when however a thing seems implausible to us, we demand a mass of proof before accepting it and may even then remain unconvinced, as for example those who still hold that the Earth has existed for only some thousands rather than billions of years. So two people can look at the same evidence and reach opposite conclusions because their initial estimates of probability differed; even more to be emphasized, they can disagree over whether the evidence is slender or compelling, superficially an "objective" point over which antagonists ought easily to be able to agree.

On the whole, those who accept the reality of an anomaly (the "believers") and those who do not (the "disbelievers" or "debunkers") have characteristically different preconceptions: for instance, the believers are somewhat predisposed to doubt the conventional wisdom whereas the debunkers tend to feel (over)confident that the conventional scientific wisdom is correct. In well-known consequence, believers tend to judge compelling an array of evidence that others find less than compelling; whereas debunkers tend to dismiss entirely an array of evidence that others find at least suggestive.

To make sense of these arguments, one needs to realize that the antagonists are always to a certain extent talking past one another: the manifest issue may be a proximate cause of the argument, but the real cause is a different set of preconceptions; the prime mover in a dispute is often a clash of ideologies, a struggle for power, or the like. Thus two siblings are not concerned fundamentally with the toy over which they seem to be fighting but rather with their relative status within the family—no matter what the toy or the issue, they are likely to find themselves in dispute; again, strikes by militant unions may be less about working conditions (economics, the manifest issue) and more about the class struggle—no matter what the wages and benefits and working conditions happen to be, there will be dispute about them. Similarly one can often predict what attitude a given person will take toward anything to do with anomalies: there are inveterate believers and inveterate debunkers.

The influence of preconception concerning anomalies was recently illustrated for this author in the reviewing of his book, *The Enigma of Loch Ness: Making Sense of a Mystery* (Bauer, 1986), which is explicitly an examination of the controversy and not an argument that these creatures do exist or that they do not exist. Reviewers with no great preconceptions (*Atlantic,* 1987; Coburn, 1987; Gragg, 1987; *Kirkus Reviews,* 1986; Martin, 1987; McMahon, 1987; Oldberg, 1986; *Snowy Egret,* 1987; Stein, 1987;

Williamson, 1987) commented on the impartiality of the discussion as an analysis of the controversy; but believers in UFOs thought they found disparagement of the believers' viewpoint (Clark, 1987; Earley, 1987) while inveterate debunkers (Kelly, 1987) were offended by the author's admission that in his personal opinion there are strange, large creatures in Loch Ness.

For all these reasons, an observer rarely finds the antagonists' arguments to clarify an issue: the antagonists are primarily concerned to make their point, to win the argument, to win converts, even when they claim to be attempting clarification—a claim they may quite honestly believe. Again, this is why arguments are replete with what are evidently to observers (but not to the participants) red herrings, *non sequitur,* illogicalities of all sorts, arguments *ad hominem.*

Polarization

Characteristically, arguments become entrenched and polarization grows rather than lessens as time goes by. The nominal issue becomes spoken of increasingly in terms of "either-or", and the antagonists come to feel that those who are not explicitly for them are assuredly against them. Every small detail becomes controversial, and no one will yield an inch lest that lead further; thus in the Velikovsky affair not only did the antagonists go to specious lengths to avoid acknowledging errors, they made much of such trivialities as their opponents' mistakes in punctuation when giving quotes (Bauer, 1984a, pp. 196–198, 241–242). Arguments may begin as attempts to establish the objective soundness of (two opposing) positions; but after a while, the antagonists' purpose becomes to discredit their opposition.

The tendency to polarization can seriously obfuscate significant points through the creation of false dichotomies (whose falsity of course is not evident to the disputants). In cryptozoology there is a characteristic argument about folkloric references: are they symbolic and mythic or are they founded on snippets of information about real animals? That is a false dichotomy, for it is patently possible for real animals to have symbolic and mythic attributes, the Biblical serpent for example. (Indeed, as Bayanov [1982] has pointed out, ape-men or wild-men would almost inevitably, if they exist, become incorporated into folklore, legend, and myth: their *absence* from folklore would speak against their actual existence.) Similarly with the Loch Ness monster, it is often asked, is the monster real or is it an invention of journalists and tourist entrepreneurs? Again a false dichotomy, for both may be true: if such rarely seen creatures exist, entrepreneurs would surely seek to exploit them, and some might actually come to believe that they had invented them (Bauer, 1986, pp. 155–156).

Committed believers or debunkers may not even be willing to acknowledge that polarization and false dichotomies are undesirable. Thus Gardner (1983a; 1983b, p. 55) argues for making sharply polar distinctions between science and pseudo-science: that night slowly gives way to day does not

vitiate the distinction between night and day, he says, and that unorthodox-yet-legitimate science and pseudo-science are but extremes of a continuum still leaves it possible legitimately to call some things pseudo-science. This might well be called the Gardner *non-sequitur* (by analogy with the Fulton *non-sequitur* [Gruenberger, 1964]): there is never significant disagreement over whether it is night or day, but there *is* typically disagreement over what can legitimately be called pseudo-science. That extreme instances of rank pseudo-science do exist cannot validate the labeling as pseudo-science of matters that are highly and contemporaneously controversial and that therefore seem only to some people and not to others to be pseudo-science (Bauer, 1984b); it is as though someone were to comment on the beauty of the evening twilight and Gardner would seek to brush that aside by insisting that it is already night-time—that Gardner can see things as black or white hardly excludes others from perceiving greys. The Gardner *non-sequitur* is common in disputes over anomalies (though it is not so commonly defended): one side alleges that the other fails to accept some unexceptionable generality, whereas the quarrel actually is over whether that generality can be applied to the specific instance at hand. Steuart Campbell (1988), for instance, accuses others of not invoking Occam's Razor when the difference of opinion is actually over which of the several explanations really is the simplest one.

The degree of polarization that occurs has been nicely illustrated by Howe (1983) for the case of parapsychology; the fervent skeptic cites only skeptical sources; asserts parapsychology to be a pseudo-science; considers only normal explanations or resorts to personal attack, ridicule, or evasion ("we will never know the answer"); asserts that the acceptance of psychic phenomena will let loose a tide of irrationality that will lead to a collapse of society; applauds the description of the typical fervent proponent but is irate over this characterization of the fervent skeptic; has more dogmatic humanist or philosophic beliefs than the average humanist or philosopher. On the other hand, the fervent proponent cites only psychic or parapsychological sources; asserts parapsychology to be a radically new science; considers only paranormal explanations without first evaluating normal ones; asserts that acceptance of psychic phenomena will revolutionize society for the better; applauds the description of the typical fervent skeptic but is irate over this characterization of the fervent proponent; has more dogmatic religious beliefs than the average religious person.

The fierceness of an argument does not necessarily parallel the degree to which the opposing viewpoints differ; indeed, the most spirited battles take place between individuals or groups whose views differ relatively little, a phenomenon classically termed "odium theologicum" (I. J. Good, personal communication, June 11, 1986) since the Church has so often been more tolerant of heathens than of schismatics; so too, most homicides in the U.S. are committed by relatives or friends; Australian or British travelers tend to be more critical of what they encounter in the U.S. than of what they

experience in Asia; the Soviet Union finds it easier to strike accommodations with the West than with China or its Eastern European fellow-travelers; the Velikovskian journal *Kronos* will at least argue with those who reject Velikovsky's ideas entirely but treats as beneath contempt and ignores Velikovskian deviationists. Once this tendency has been recognized, for passions to be exacerbated by closeness rather than by distance, it can be readily enough understood. One tends to assume implicitly that another person's similar views are actually identical views, and when the recognition comes that this is not so, the disappointment can be shocking and can produce a violent reaction. Moreover, those who are closest can thereby seem the most threatening: given that our differences are so slight, might that not make it very easy for them to syphon support and supporters away from us? And perhaps, as we compare our opinions with only slightly different ones, there comes the fear that we ourselves might be mistaken—for it is surely easier to entertain the notion that we may be slightly wrong than that we might be grossly wrong (yet it is still unpalatable to admit it).

In the rare cases when true believers (Hoffer, 1951) do happen to recognize their error, they do not then adopt a more balanced or judicious stance, rather they typically go to the other extreme. Arthur Koestler's disillusionment with Communism left him implacably anti-Communist; Whittaker Chambers coupled that shift with conversion from atheism to Catholicism. Maurice Burton gave credence to the Loch Ness monsters for nearly three decades and then became a determined debunker; Razdan and Kielar (1984–1985) became injudiciously critical after their own search for the monster was unsuccessful. The switch from believer to debunker is easier than from either of those stances to uncommitted, because the extreme positions are so similar to one another, psychologically and (il)logically. Thus the believers and the debunkers are equally dogmatic—for example, that *the* scientific method exists and that they know precisely what it is; and they can be incisively logical about their opponents' fallacies while themselves committing similar ones (Bauer, 1984a, pp. 223–250). Debunkers, in fact, just as much as believers belong to the type described by Hoffer (1951).

Arguments Within Scientific Disciplines

Though the points just made apply to all arguments, including controversies within science ("intra-scientific" ones) as well as those about claimed anomalies, there are quite important differences in degree. The conventions of science serve to moderate the non-rational aspects of intra-scientific dispute, whereas arguments over anomalies are not so constrained.

In intra-scientific disputes, there is less opportunity for covert ideologies to influence the general course and particular events of a controversy. Most such arguments concern quite well-defined, specific issues: a particular observation or set of results, the efficacy of an instrument or method, the fit of one aspect or corollary of a theory. Overriding the dispute is a shared para-

digm. All the disputants approach the manifest issue in much the same way, with much the same biases and preconceptions about them and with much the same goals: to resolve the dispute and thereby get past that impediment to further progress. Moreover, continuing and explicit lip-service is paid to the ideals of objectivity and empiricism, and consequently there is more opportunity for logic and data actually to be taken seriously.

Scientists are trained not only to discover but also to find sponsors for their work and to publish their discoveries; and getting support and getting published means impressing and convincing others. Through early training, therefore, scientists learn something of the difference between personally holding beliefs and convincing others; they learn to be aware of and to deploy themselves the criteria that others will apply to their proposals and manuscripts. So scientists are trained to become their own critics and learn to become relatively logical and empirical in their work; within science, convincing oneself becomes not so different from convincing others.

Pieces of science ultimately must pass the test of Nature itself. Scientists take enormous risks if personal or ideologic motives lead them into logical fallacy or into ignoring or fudging data; at any time the honest work of others, in the same or in a similar field, could expose them as incompetent or dishonest and their whole career could be ruined. Adherence to the ideals of science, to the extent humanly possible, is for scientists a plain matter of self-interest.

Arguments within science, then, are somewhat less at the mercy of human vagaries than are other arguments. When scientists become involved in disputes about anomalies (or in other public arguments, or in arguments about politics or social questions), their experience of intra-scientific dispute can prove to be a handicap: they are not aware that there exists no shared paradigm, and so they are not prepared for the degree to which the disputes are actually about other things than the nominal question, they are not accustomed to disputes that are so much over beliefs and so little over facts; they do not understand why or how there can be such a multitude of illogicalities and red herrings. And so scientists are often ineffective in such arguments: over creationism, over Velikovsky, over Star Wars.

It is not being suggested that intra-scientific disputes are less intemperate or raucous than others, only that within science the arguments stick more closely to the manifest issues and are quickly settled once the relevant facts are in. By contrast, arguments over anomalies can persist, through the activities of small groups of proponents, long after the objective facts are in: to take an extreme case, there still exist Flat-Earth societies. Outside the disciplines, there is no system or authority to settle intellectual disagreements and to keep them settled.

Public Arguments

In public arguments as in all others one finds ideologic differences behind the manifest issues, one finds the antagonists seeking to win the argument

rather than to clarify the matter, and one finds progressively greater polarization over time. In public debates, though, one's ego may seem even more at risk than in private argument and saving face may become even more essential. It would make sense, therefore, for antagonists in public debate to look for ways by which it might become easier and less self-threatening for their opponents to recant; but examples of that seem to be rare. Thus Velikovsky was ridiculed harshly, misrepresented sloppily, paid scant respect; and so—if for no other reason—Velikovsky's critics were quite ineffective in swaying Velikovsky or his supporters.

Speaking to an Audience

A partial explanation for such apparently inept behavior as that of Velikovsky's critics is that they were not in fact trying to persuade Velikovsky or his followers; the aim was to discredit Velikovsky in the eyes of the public. In all arguments there are proponents and opponents, but in public arguments there is also an audience to be reckoned with. Most commonly, what the disputants say is actually addressed to that audience, the public, even when they may seem to be addressing one another; and things are said differently —or different things are said—than when it is the opponents who are being directly addressed. Sometimes, to be sure, the disputants do substantively address one another; but at other times again they appear to be talking only to themselves. Thus the typical publication by believers (for example, *MUFON Journal, Kronos, Nessletter*) or by debunkers (for example, *Skeptical Inquirer*) serves only the purpose of speaking to the already converted: those writings, as also conferences arranged by such groups, are better understood as rituals of self-motivation and self-reassurance than as attempts to make a case that might persuade opponents or the general public, let alone as attempts to clarify the substantive issue. For example, *Skeptical Inquirer* has published several debunking pieces about the Loch Ness controversy but was not interested in a survey of the strongest evidence, giving as reason that the magazine's purpose is not to consider what the best evidence for anomalous claims might be but to argue against them (Kendrick Frazier, personal communications, February 26 and October 6, 1984).

Pundits

In public debates one also encounters another type of role besides those of proponent and opponent: that of the observer or pundit, who takes an active part in the argument as an explicator of the issues or as a mediator between the extremes: ideally, the pundit serves to educate the public and to help it reach the truth of the matter. But pundits have their own opinions too, and those are likely to be more to one side of the issue than the other, and so it is by no means always easy to distinguish pundits from disputants—as for instance with the social scientists and humanists who entered the Velikovsky debate in the 1960s (Bauer, 1984a, pp. 52–56).

A common ploy by debunkers masquerading as uncommitted observers is to declare that educating the public is the same as having the public accept the debunking view: for example, in several instances the only criterion used by debunkers to judge whether students had become critically minded was whether they had lost their willingness to contemplate the possible reality of certain anomalies (Bainbridge, 1988; Blackmore, 1984–1985; Lee, 1985). That illustrates the fact that in intellectual disputes (as generally in democratic societies) lip-service only is paid to the notion that all are entitled to their own opinions: in practice, on any given issue one finds few people actually content to have others believe differently from themselves. Thus liberals tend to doubt that free speech for fascists or creationists is really good for society—or, what has the same effect, liberals become very agitated when widespread credence is given to, say, creationist views. Again, science popularizers tend to doubt that publishers should put out books like those of Charles Berlitz or Eric von Daniken. Liberals, scientists, and science writers profess to believe that aberrant views ("error") would disappear if only the public were well enough educated, in particular in science; yet they usually take part in public dispute not as educators and uncommitted explicators but as proponents of specific views, and they tend to view the public as "educated" only to the extent that it accepts their opinions.

Arguments About Anomalies

Controversies over anomalies are often public arguments, so what has just been said applies here too; but one can be more specific about who are the adversaries, about typical overstatements, and about the manner in which science is brought into the dispute.

The Disputants

Those who push the reality of an anomaly are typically amateurs or laymen with respect to the most apparently relevant disciplines, as are some of the most vociferous of their opponents. The experts and professionals are almost all skeptics or opponents or debunkers: thus physicians typically do not practice faith-healing or homeopathy or acupuncture (although some do); nor are many psychologists active in parapsychology (though a few are). That many of the proponents of an anomaly are amateurs has corollaries that make widespread acceptance of the anomaly more difficult (Bauer, 1984a, pp. 189–193; 1986, pp. 77–98): their work is not coordinated, many are less than competent, their reports are not rigorously refereed, the literature is inchoate; studying an anomaly is not doing science, and the investigations cannot realistically be judged as pieces of science.

Although most of the experts are skeptics or debunkers, one usually finds a few maverick professionals who espouse an anomaly or call for further investigation of it: Wilhelm Reich has followers among physicians and so-

cial scientists, Hynek was an astronomer and ufologist, Grover Krantz is an anthropologist who believes Bigfoot to be real; Velikovskians counted some physicists among their number; biologists Roy Mackal and Denys Tucker (among others) believe that Loch Ness monsters are real. There is almost always a distribution of belief among the experts about the reality of an anomaly; and that is one reason why anomalies cannot be dismissed simply by reliance on intellectual authority—that authority, insofar as it exists, is not sufficiently consensual on the matter.

Overstating the Cases

Disputants in all arguments tend to overstate their cases. With anomalies, the claimants typically say, "It is so", when it should be patently obvious that the evidence is insufficient to convince many intelligent, educated, appopriately qualified people, among them most of the professional experts. It would be more palatable to the pundits and even to the experts if the proponents were merely to say, "There is good evidence for this, it is not unreasonable to entertain the possibility". Here is an instance where the distinction between personal belief and demonstrable proof cries out to be applied: it may often be quite reasonable for the proponents to believe the anomaly to be real, but that does not necessarily make it reasonable for them to attempt to convince others of that or it (Bauer, 1986, Ch. 10).

The opponents, on the other hand, do not content themselves with making the eminently reasonable statement, "The evidence is not strong enough; and anyway the matter is implausible in the extreme; there is no reason for me to take an interest". No: they typically say, "It is not so, because it cannot be so"; and thus they lend support to the charges typically brought by the proponents, that scientists are arrogantly dogmatic, unwilling to examine revolutionary new phenomena or theories, and (unwarrantedly) confident that science already knows all the important things.

Deceptive Unity

Because the proponents of an anomaly can readily see themselves as a beleaguered minority, there is a tendency to close ranks against the debunkers even when the ranks are then closed on bed-fellows that the proponents would rather not have. For that phenomenon in the Loch Ness controversy, see Bauer (1986, pp. 76–77, 81–84); similarly in parapsychology (Hoebens, 1981–1982, p. 39): "The psi community has never completely freed itself from the pernicious *idée fixe* that overt criticism of a colleague may damage the cause and play into the hands of the enemies of parapsychology. Some parapsychological researchers began to suspect Tenhaeff long ago. Seldom, however, did they voice their doubts openly. And, when they did, some sociological mechanism seems to have prevented an adequate follow-up". Surely the cause is damaged more in the long run when the proponents close ranks with frauds or incompetents rather than admit

that some participants cannot be relied upon. On the other hand, since there is no overarching community of proponents of any anomaly—all are free publicly to declare themselves believers or investigators—no discipline can effectively be enforced, and the attempt to keep out undesirables may become hopelessly ineffective as well as unpleasant. It is a typical dilemma: the believers tend to close ranks because they do not trust outsiders to be capable of distinguishing—or even willing to distinguish—honest from fraudulent or competent from incompetent anomalists; but as the ranks are closed it becomes even more unlikely that such distinctions will in fact be made by anyone.

At any rate, one ought to be clear that—quite apart from instances of dishonesty or incompetence—sharp internal disagreements, both intellectual and personal, commonly exist among proponents of a given anomaly even when there are no obvious external signs of it. Thus much of the tension among Velikovskians was long known only to insiders; and though the monster-hunters at Loch Ness indulge in little public criticism of one another, in private it is quite a different matter, and one soon finds that there is little mutual respect or trust—let alone practical cooperation—among a number of competing individuals and groups. The penchant that debunkers and the media have for lumping together all proponents of a given anomaly reveals a lack of knowledge and understanding.

The Role of Science

In arguments over anomalies, science is typically appealed to: the claimants think that science ought to accept the existence of the anomaly sufficiently to study it, while the debunkers call the anomaly pseudo-*science.* That issue, in point of fact, is a red herring: the question, after all, is not what science should do or say but whether or not the anomaly is real. By insisting that science ought to take an interest, the claimants imply that science ought to study something simply because it may be real, despite the fact that it appears not to be consonant with the prevailing state-of-the-art, and despite the fact that there is no obvious way efficiently to obtain useful data about the matter; thus they display a misunderstanding of the way science actually works. For their part, the disbelievers also misunderstand the nature of science when they label an anomaly pseudo-science just because they think it is not real: as though science only studied matters known beforehand to be real! That both sides—and also the pundits and the media and the public—so readily succumb to this red herring of appeal to science illustrates that this is an age of scientism (whether or not it be, as so often claimed, an age of science).

Scientism means taking science as *the* arbiter of truth, and nowadays we all do that though we are not necessarily aware of it. The public is assured by advertisers and by politicians that what is being pushed has been scientifically shown or tested—"scientific tests have shown . . ."; what function is served by "scientific" or "scientifically" in such a context except to deliver the seal of certainty?

No one disputes the power or truthfulness of science. The believers insist that science should enter the picture because, perhaps without even knowing it, they believe the imprimatur of science to be the ultimate guarantee that they are not mistaken. The believers may often seem to be moved by resentment of science, and they may often seem to be criticizing it harshly, but the criticism turns out on closer examination to be only that science has gone wrong in this particular instance, science would be quite all right if only it rectified that error: so Velikovskians sought *reform* of science, and saw themselves as building a *correct* Velikovskian science; and fundamentalists are not content to have the divine revelation of creation, they require the reassurance of "creation science".

Misconceptions About Science

Arguing over whether an anomaly is pseudo-science, and appealing to science as the arbiter of truth, though actually a red herring nevertheless serves to make the nature of science a common theme in these arguments, one about which wide ignorance and confusion are then displayed: over whether science is a quest, or the application of a defined method, or a body of reliable knowledge, or an aristocratic pursuit; over how reliable scientific knowledge actually is, especially when laws or theories are applied under novel circumstances; and, in consequence, over how applicable "science" may be to such fields as history, or in everyday life, or in particular to discussions of anomalies. And always there arises the question, who can legitimately claim to speak in the name of science? In fact, one of the most indubitably instructive consequences of looking at arguments over anomalies is that one becomes aware of how little understood is science, never mind that it is universally appealed to.

Ignorance

Ignorance of various sorts, not only about science, plays an important role in arguments over anomalies. By definition, of course, we are ignorant about the main question, whether the anomaly is real or only apparent; but we are also usually ignorant about what sort of knowledge might actually help to answer the question, and certainly we are ignorant about how to obtain that knowledge. Since we are ignorant about which discipline might be relevant, it can happen that useful information exists without the disputants being aware of it: for instance that some species of spiders let masses of web fall from high in the atmosphere. Ignorance is a central characteristic of arguments over anomalies. The ignorance may be chiefly on one side or the other: for instance on matters of physics in respect of the flat-earthers or (specifically regarding electromagnetism and gravity) the Velikovskians. Or, both sides may be ignorant: for instance about what science is and how it works, where both Velikovskians and their critics were notably wrong, albeit in different ways.

Most interesting, of course, are those anomalies over which argument persists because we all—humankind—are still ignorant of what is needed to settle the argument; and that ignorance is surely sufficient reason to explore, as scientifically as is feasible, the domain of anomalous phenomena.

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INVITED ESSAY

Anomalies: Analysis and Aesthetics

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Abstract—In properly allying itself with traditional scientific tenets and procedures, anomalies research also risks encumbrance by scientific stodginess, scientific segregation, and scientific secularity. In particular, the contemporary rejection by established science of its own metaphysical heritage and essence precludes its further evolution into physical and biological domains where consciousness plays demonstrably active roles. Some orderly rapprochement of subjective and objective experience and representation within the scientific paradigm will be required to make it effective in such arenas.

Three epistemological premises prompted the conception and birth of our Society for Scientific Exploration, and have guided its early life:

1. Empirical anomalies in any scientific sector can be precious indicators of the limits of established wisdom and can open trails to better understanding.
2. Study of such anomalies must be pursued with uncompromising rigor and critical conservatism.
3. Contemporary anomalies research needs an objective interdisciplinary forum for comfortable professional discussion of the phenomena and their implications.

By our membership policies, the structure and conduct of our meetings, and the design of our publications, we have attempted to implement these ideals to insure that SSE shall indeed propagate its research with at least as high a level of technical rigor and critical judgment as prevail in most mainstream scientific organizations. Yet, in this lofty commitment there lurk possibilities for severe pragmatic tensions, if the premises are not profoundly interpreted and carefully balanced.

The problem, of course, is not with the principles, per se, but with their abuse. As Dorothy Sayers reminds us, the familiar adage "the road to hell is paved with good intentions" may not so much refer to noble aspirations left unfulfilled by neglect, distraction, or incompetence, as to those pursued so slavishly that they become ends in themselves, to the point of negating, or even inverting, their original virtues. The sacred tenets of science are by no means invulnerable to such distortion by excess. As a present and pertinent

example, we might point to the prevailing plethora of criticism concerning the conduct and interpretation of the various forms of anomalies research in which we are engaged. To the extent that such commentary is informed, fair, and constructive, it provides important restraints along the paths of understanding, and nothing that follows here should be construed to contradict the essential role of such critical analysis in the scholarly progress of our Society. But in its uninformed, unfair, or self-serving misapplication, such criticism becomes distracting, divisive, and counterproductive, and must be courageously resisted.

So also with excessive deference to other canons of the scientific process. In allying ourselves too assiduously with the prevailing content, methodology, and standards of science, we can become bound by its dogma and limited by its self-imposed horizons; in over-valuing scientific caution, we can become mired in scientific inertia. Three categories of such encumbrance that bear quite directly on the SSE situation might be termed "scientific stodginess," "scientific segregation," and "scientific secularity." Let us consider the first two very briefly, and the third in more detail.

Scientific Stodginess

Many of us have witnessed, and possibly even contributed to, legitimate professional disagreements that have escalated to matters of principle, then into ad hominem personal conflicts, and thence to outright bigotry and inanity. Many of the greatest minds of science have similarly blundered into such foibles. History records a dreary sequence of cases where scholars of immense stature, themselves having broken through entrenched pedantry to open new horizons, later obstructed scientific progress with their own brands of bombast. We think of Ernest Rutherford, who first showed the world the nuclear atom, subsequently fulminating:

> It is a very poor and inefficient way of producing energy, and anyone who looks for a source of power in the transformation of atoms is talking moonshine. (Rowland, 1957, p. 129)

With equally misplaced authority, the Astronomer Royal, Richard Wooley, proclaimed one year before Sputnick, that "space travel is utter bilge." Lord Kelvin assured us that x-rays would prove a hoax and that heavier-than-air travel was impossible. Ernst Mach decried both atoms and relativity. D'Alembert distrusted probability theory, and Lavoisier and Ostwald disputed atomic chemistry. The list of such derailments of scholarly judgment is long and humbling.

More modern examples of similar abuse of scientific conservatism also abound. In the particular fields of our interest, we find them displayed not only by individual critics, but by a number of fully-blown professional organizations. And the tragedy lies not only in the direct encumbrance of

the research they decry, but even more seriously in the adulteration of legitimate criticism that could constructively separate valid evidence from fantasy.

One obviously should not claim that short-sighted authoritarian opposition of this sort automatically guarantees the validity of the concept criticized. Nonetheless, there may well be some subtle correlation between the emotionality of a critical reaction and the viability of its target, especially when the latter seriously threatens some deeply entrenched professional or personal position. In this respect, "the lady protesteth too much" criterion may occasionally apply to Madame Science, as well.

Scientific Segregation

Modern science has proven supremely effective in systematically subdividing complex problems into more manageable portions, discriminating among potential mechanisms and competing concepts, analyzing elemental processes, and cataloging the results. In general, however, it has proven considerably less effective in putting the pieces back together—in synthesizing new systems and unfamiliar interactions, especially when these have involved multidisciplinary aspects. To confirm this imbalance of competence, one need look no further than the number of extant specialist and sub-specialist professional societies and journals compared to those addressing interdisciplinary topics or strategies, or examine the relatively primitive states of such fields as human factors engineering, operations research, complex systems analysis, etc., or note the essential absence of any basic discipline that might qualify as "systems science."

Along with this severe conceptual subdivision come equally esoteric languages that further inhibit transdisciplinary dialogues, engender professional chauvinisms, and even raise suspicions fostered by unfamiliarity and exclusion. This "Babel" of hyperspecialization is becoming a progressively greater obstruction to the comprehension and application of much conventional modern science; in the fields of research that our society encompasses, it could be quite fatal to the entire enterprise. The cartoon of Figure 1 (courtesy of Henry H. Bauer) is a reasonably apt caricature of some comments overheard in the corridors of SSE annual meetings and, if we are totally honest, of our own private hierarchies of credibility. How often do we feel that the courageous experimentation and blazing insight featured in our own research deserves the most broad-minded respect and admiration from our colleagues, while the work some of them pursue is just too controversial and too extreme to be fully credible? Yet it is quite possible that each of our topics remains anomalous precisely because we lack the breadth of perspective to put it in that larger context of understanding wherein the phenomena can be accommodated naturally, and wherein more comprehensive theoretical models could pertain. Are we not more likely to unfold that broader

Fig. 1.*

comprehension collectively, rather than individually? And do we not need to learn how to converse collectively before we can reason collectively?

Scientific Secularity

The third, and possibly most severe, class of impediment imposed by the excessively rigid stance of modern science, and the one on which we shall dwell a bit, devolves from its categorical and contradictory denial of its own metaphysical essence and heritage. The very word "metaphysical" has come to carry distasteful or suspicious connotations for most scientific purists, and is usually applied pejoratively in any research context. As we use the term here, however, it is simply meant to encompass all subjective, intuitive, impressionistic, or aesthetic aspects of a scientific situation which, while not submitting comfortably to prevailing catalogues and formalisms, nonetheless are found empirically, or hypothesized heuristically, to be relevant to the given event or process. The historical precedents for inclusion of such factors in scientific study and applications are floribundant beyond question. Ancient science, from prehistoric civilizations through the Egyptians, Babylonians, Orientals, and classical Greeks, was an inextricable admixture of mystical, magical, and analytical manipulation that served for millennia to undergird the technological needs of those societies. Medieval alchemy likewise propagated as a sacred marriage of the Hermetic philosophical tradition with the early methods of analytical chemistry. Even the first

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* Reproduced with the permission of, and appreciation to, Professor Henry H. Bauer.

echelon of the analytical astronomers—Copernicus, Tycho Brahe, Kepler, Galileo—now popularly represented as breaking through the suffocating fog of theological dogma with sound scientific methodology, actually carried forth much of the same metaphysical tradition. Note how Copernicus justified his heliocentric universe:

> In the middle of all sits the Sun enthroned. In this most beautiful temple, could we place this luminary in any better position from which he can illuminate the whole at once? He is rightly called the Lamp, the Mind, the Ruler of the Universe: Hermes Trismegistus names him the Visible God, Sophocles' Electra calls him the All-Seeing. So the Sun sits as upon a royal throne, ruling his children, the planets which circle around him. (Bronowski, 1973, pp. 196–197)

Kepler similarly rationalized his orbital geometry:

> . . . when intersected by a plane, the sphere displays in this section the circle, the genuine image of the created mind, placed in command of the body which it is appointed to rule; and this circle is to the sphere as the human mind is to the Mind Divine . . . (Pauli, 1955, p. 161)

Even the mighty Isaac Newton, on whose classical mechanics and optics modern science is irrevocably based, has been accurately described by one biographer as:

> . . . not the first of the age of reason. He was the last of the magicians, the last of the Babylonians and Sumerians, the last great mind which looked out on the visible and intellectual world with the same eyes as those who began to build our intellectual inheritance rather less than 10,000 years ago. (Keynes, 1963, p. 311)

and by another as that premier scientist who regarded the ultimate mechanism of change in the universe to reside in the "mystery by which mind could control matter" (Kubrin, 1981, p. 113).

Ah, we say, but did not Sir Francis Bacon, the Age of Enlightenment, the Scientific Revolution, and the formation of the Royal Society clean all of this naïve metaphysical junk off of the work tables of modern science? Not quite. It is true that Bacon, the acknowledged father of the modern scientific method, insisted on a critical dialogue between hard empirical evidence and sound analytical logic, but he then proceeded to apply such methods to the study of telepathic dreams, psychic healing, and "experiments touching transmission of spirits and the force of the imagination" (Walker, 1972, p. 127). In his Charter for the Royal Society, Robert Hooke indeed rejected "meddling with Divinity, Metaphysics, Moralls, Politicks, Grammar, Rhetoric, or Logick" (Lyons, 1944, p. 41), but then went on himself to study, write, and lecture on keenly metaphysical topics. His colleague Robert Boyle, author of "The Skeptical Chymist," retained an intense commitment to the Hermetic heritage (More, 1962), and the Royal Society as a whole

promoted scientific study of astrology, alchemy, prophecy, magic, and witchcraft.

To be sure, toward the close of the 19th century, the profound triumphs of electromagnetic wave theory prompted the prevailing physics establishment to wax rather smug about the omnipotence of deterministic, causal science, apparently overlooking the intuitive conviction of natural symmetry that had led Maxwell to propose his subtle, but all-important, displacement current. But the complacency was short-lived, for over the following decades there erupted a host of new physical anomalies—black-body radiation, atomic and molecular line spectra, photoelectric and Compton effects, specific heats of solids, and numerous others, that simply could not be swept under the classical scientific rug, and the enigmatic era of modern physics was at hand.

An enigmatic era indeed, featuring quanta and photons, wave/particle dualities, uncertainty and exclusion principles, probability-of-observation wave mechanics, and countless other counter-intuitive concepts that reimbued physical science with a distinctly metaphysical aroma. And none recognized the philosophical inescapability and pragmatic impact of this dimension more profoundly than the patriarchs of modern physics themselves. The father of their clan, Max Planck, courageously broached the fundamental issue:

> Once we have decided that the law of causality is by no means a necessary element in the process of human thought, we have made a mental clearance for the approach to the question of its validity in the world of reality. (Planck, 1932, p. 117)

Neils Bohr responded with his own radical conviction:

> . . . causality may be considered as a mode of perception by which we reduce our sense impressions to order. (Bohr, 1961, p. 116)

Erwin Schrödinger took a yet more vigorous metaphysical position:

> The world is given to me only once, not one existing and one perceived. Subject and object are only one. The barrier between them cannot be said to have broken down as a result of recent experience in the physical sciences, for this barrier does not exist. . . . Mind has erected the objective outside world of the natural philosopher out of its own stuff. (Schrödinger, 1967, p. 137 and p. 131)

And Louis de Broglie, the Prince of particulate probability, closely presaged our own present convictions about the role of consciousness in the establishment of reality:

> Science is therefore a strange sort of penetration into a world which through human consciousness and reason has learned to become aware of itself. (de Broglie, 1962, p. 220)

> Even in the most exact of all the natural sciences, in Physics, the need for margins of indeterminateness has repeatedly become apparent—a fact which, it seems to us, is worthy of the attention of philosophers, since it may throw a new and illuminating light on the way in which the idealizations formed by our reason become adaptable to Reality. (de Broglie, 1939, pp. 281–282)

There is no substitute for thorough reading of the extensive personal writings of these and the other pioneers of modern physics to acquire full appreciation of the implicit and explicit mystical dimensions of this era of science. And it is an era that is far from closed. Even now, we continue to be confronted by latter-day EPR paradoxes and action-at-a-distance experiments that severely contradict the premises of local causality. In our conceptualization and linguistic representation of sub-nuclear phenomena on one extreme—quarks, gluons, strangeness, charm, and so on—and of astrophysical and cosmological processes on the other—quasars, black holes, cosmic strings, pulsating bubble universes, etc.—there smolder some of the same metaphysical propensities that were more explicitly enflamed in Hermes's precepts or the alchemist's forge.

And we certainly must include in this list the research encompassed by this Society, which in many of its projects addresses frankly metaphysical effects. For example, data on man/machine anomalies like those shown in Figure 2 have been presented in this forum on several occasions (Jahn, Dunne, & Nelson, 1987). The particular case shown pertains to the interaction of one human operator with a microelectronic random event generator (REG) in a very carefully controlled sequence of experiments extending over nine years. Plotted are the accumulated deviations of the output of the machine from chance expectation, obtained under a tripolar protocol wherein the operator alternately attempted to achieve a high number of

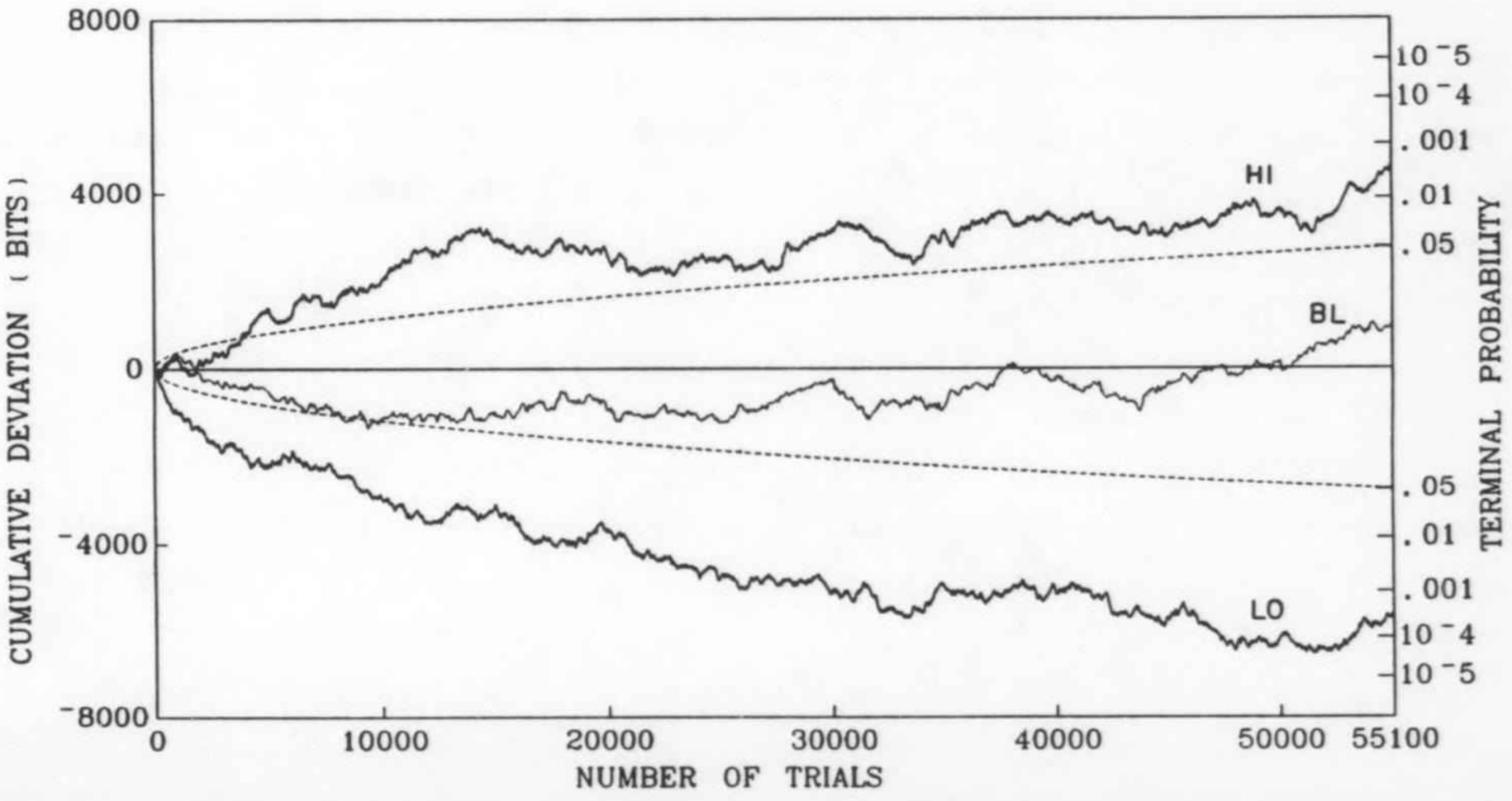

Fig. 2. REG cumulative deviations from chance, one operator.

counts (HI), a low number of counts (LO), or the chance number of counts (BL), interspersed in a random sequence of efforts, with all other technical and procedural aspects of the experiment held identical. As can be seen from the figure, the null-intention or baseline effort yields a string of data oscillating stochastically about the theoretical chance mean. The high-intention efforts produce results displaying the same sort of stochastic oscillations, but now superimposed on a systematic trend toward ever increasing excess above chance. The low-intention efforts show a similar, but even more substantial trend in the opposite direction. On this figure, the dashed parabolas represent the loci of .05 likelihood of achieving the given excursions by chance, and the scale on the right ordinate shows the full range of terminal probabilities against chance for this huge sequence of data. Specifically, for the more than 30,000,000 bits processed in the more than 50,000 tripolar trials of this operator's program, the likelihood of obtaining the displayed split of the HI and LO data by chance is less than a few parts per million.

More than 30 other operators have performed this same experiment. Some achieve much like the example shown in Figure 2; some are successful in only one direction of effort, or in the other; some display only chance results; a few achieve extra-chance results in directions opposite to their intentions. But despite these major differences in detail, in most cases each operator's pattern is serially consistent with itself, i.e., internally replicable in the statistical sense, so much so that we refer to the individual cumulative deviation graphs as operator "signatures."

In some cases, these signatures are sensitive to secondary technical parameters of the experiment, such as whether the operator is allowed to choose the direction of effort or is instructed by some randomization criterion, or whether the operator is allowed to initiate each trial at his comfort or is presented with a regularly spaced sequence of automatic trials, or whether on-line feedback is provided and in what form. In other cases, however, the signatures appear insensitive to such options. Nonetheless, if the results of all operators, obtained under all permutations of these secondary parameters, are combined in a grand concatenation, the cumulative deviations still compound to highly significant statistical departures from chance behavior (Figure 3).

Although these REG data are clearly operator-specific, intention-specific, and in some cases parameter-specific, curiously they seem to be much less device-specific. Several other similarly extensive experiments have been performed using different microelectronic noise sources, pseudo-random sources constructed from arrays of microelectronic shift registers, programmed computer algorithms, and even macroscopic mechanical analogue devices. In a number of cases, an operator's signature of performance is found to transfer with remarkable similarity from one class of device to another. For example, Figure 4 shows a comparison of the cumulative deviation signature of one operator on a microelectronic REG, a shift-register pseudo REG, and a macroscopic Random Mechanical Cascade (RMC)

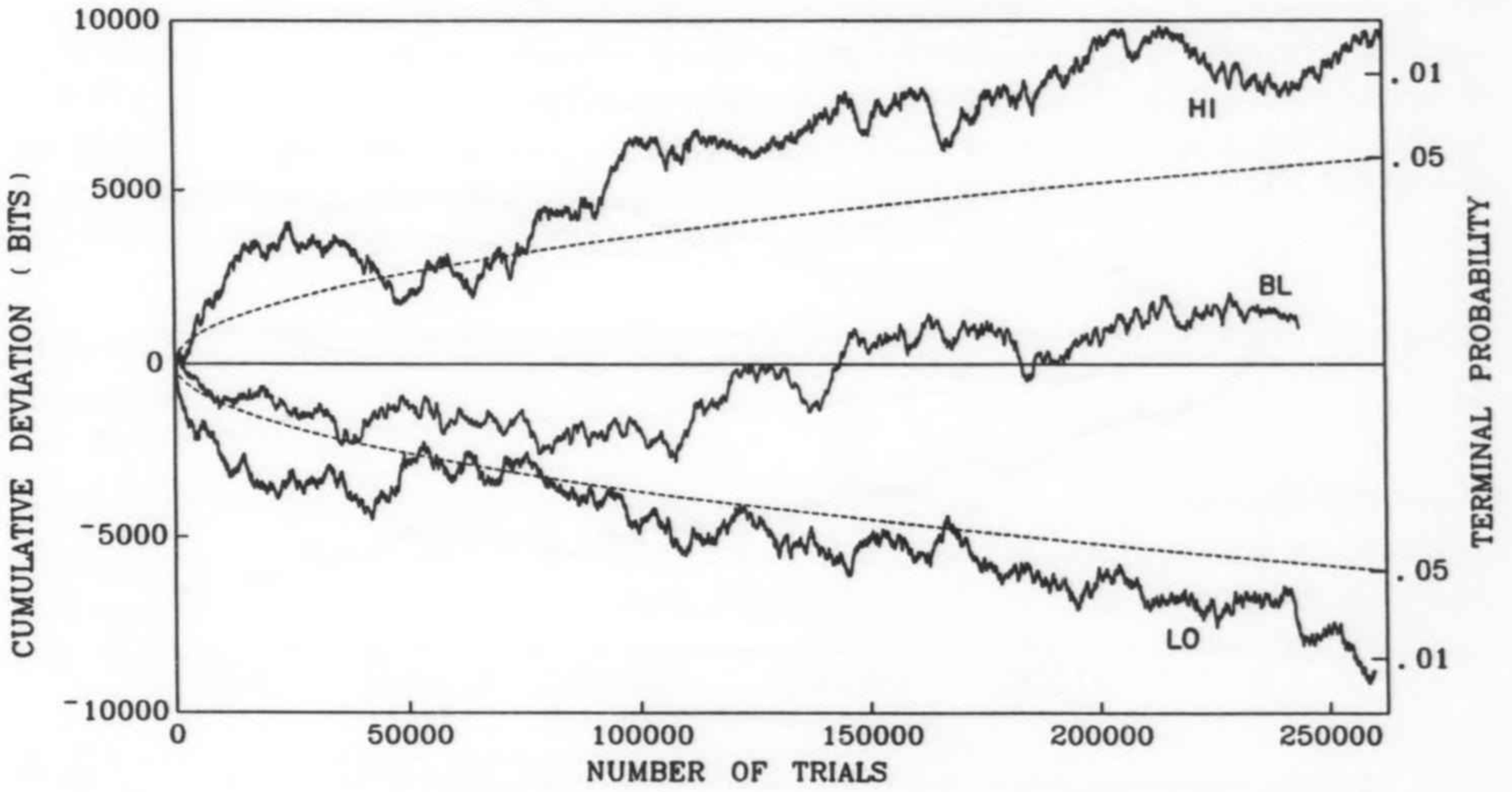

Fig. 3. REG cumulative deviations from chance, all operators.

respectively. Note the perseverance of the substantial low-intention efforts, the less substantial but characteristic high-intention trace with its initial strength and subsequent decline, and the well-behaved null-intention results.

A great deal more data from experiments of this sort could be shown, where the only independent variables of consequence are the individual operators and their directions of effort (Dunne, Nelson, & Jahn, 1988). Although these illustrations have been drawn from our own research base, many other studies (referenced in Jahn, Dunne, & Nelson, 1987), including some presented to this Society over the past several years, would seem to lead toward similar conclusions. Obviously, many more experiments of this class, including independent replications of those already reported, are now required, for unless such results can be directly discredited, the need for some metaphysical component in any model attempting to explicate, or even to correlate, the data seems unavoidable.

Summary

What is the point of this potpourri of historical, philosophical, and scientific musing? It is not, of course, a plea for return to blind superstition or superficial mysticism, nor for compromise with soft-shell science of any form. Rather, it is a suggestion that research such as our society fosters would be better served by a more complementary balance between our objective and subjective perspectives, of much the same sort that Bohr proposed:

> . . . we must, indeed, remember that the nature of our consciousness brings about a complementary relationship, in all domains of knowledge, between the analysis of a

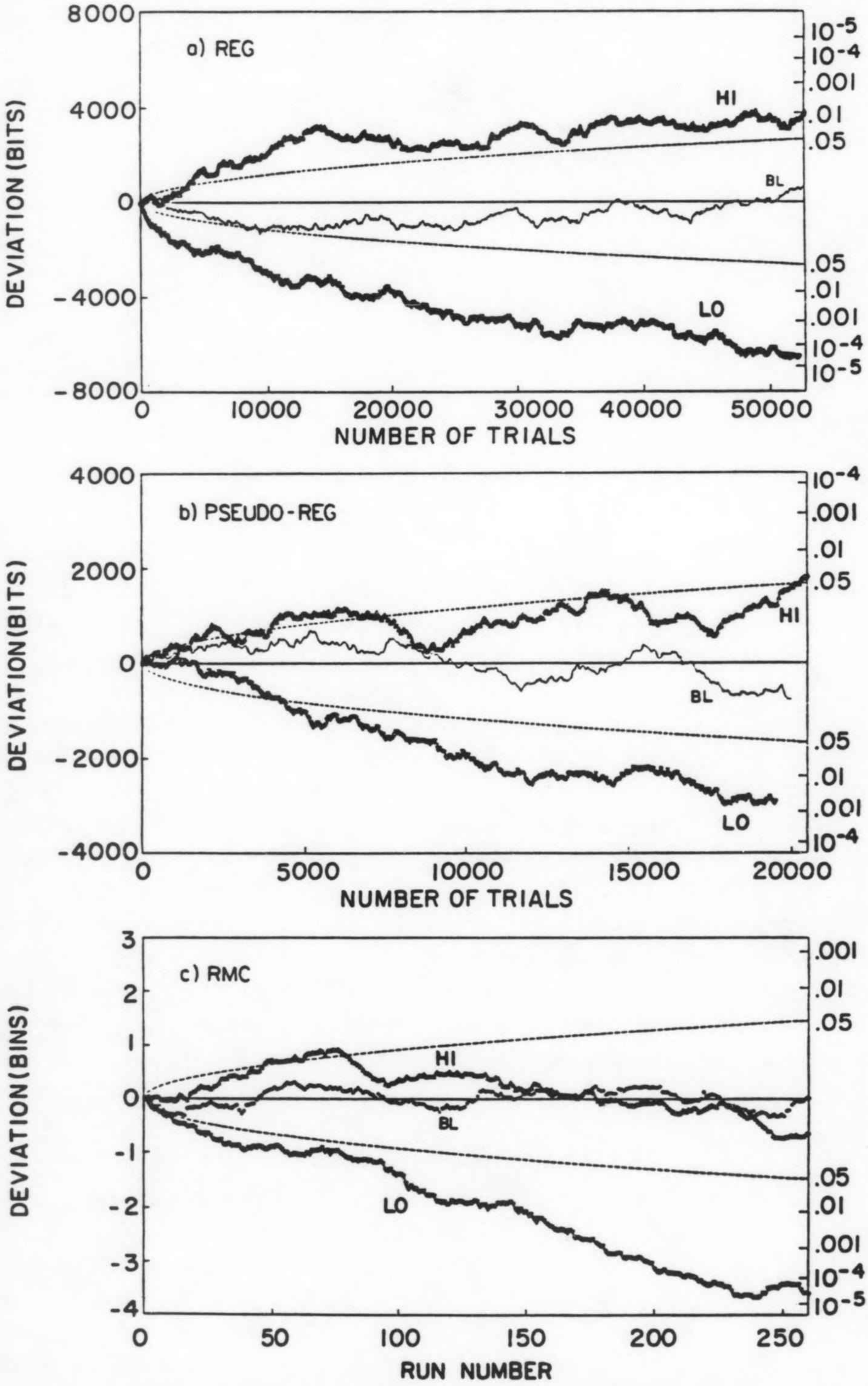

Fig. 4. Cumulative deviations, one operator on three experiments.

concept and its immediate application. . . . in associating the psychical and physical aspects of existence, we are concerned with a special relationship of complementarity which it is not possible thoroughly to understand by one-sided application either of physical or psychological laws. . . . only a renunciation in this respect will enable us to comprehend . . . that harmony which is experienced as free will and analyzed in terms of causality. (Bohr, 1961, pp. 20–24)

Heisenberg also offered a similar generalization of the complementarity principle:

We realize that the situation of complementarity is not confined to the atomic world alone; we meet it when we reflect about a decision and the motives for our decision or when we have the choice between enjoying music and analyzing its structure. (Heisenberg, 1958, p. 179)

and Pauli specifically addressed it to our context:

On the one hand, the idea of complementarity in modern physics has demonstrated to us, in a new kind of synthesis, that the contradiction in the applications of old contrasting conceptions (such as particle and wave) is only apparent; on the other hand the employability of old alchemical ideas in the psychology of Jung points to a deeper unity of psychical and physical occurrences. To us . . . the only acceptable point of view appears to be the one that recognizes *both* sides of reality—the quantitative and the qualitative, the physical and the psychical—as compatible with each other, and can embrace them simultaneously. . . . It would be most satisfactory of all if physics and psyche could be seen as complementary aspects of the same reality. (Pauli, 1955, pp. 208–210)

In short, our plea is for more formal acknowledgement of the pervasive metaphysical stream that continues to permeate and nourish much of our contemporary science and technology. This acknowledgement is not likely to be initiated by any of the well-established sectors of modern analytical research. These are too structured and hierarchical, too busy and comfortable, and admittedly too effective, to confront this dimension without considerably greater demonstration of its local relevance and provincial benefits. But within the traffic pattern of SSE, we not only have the opportunity and the disposition, but very possibly the necessity, of reengaging the analytical and the aesthetic aspects of scholarly science. Man/machine anomalies like those displayed in Figures 2–4, for example, are not likely to be rendered theoretically comprehensible without some disciplined inclusion of the role of consciousness as an active ingredient in the establishment of reality. One modest attempt at such a model, presented earlier to SSE, allows consciousness the same wave/particle duality it has ascribed to various physical systems, and then invokes the formalisms of quantum wave mechanics to represent interactions of consciousness with its environment (Jahn & Dunne, 1986, 1987).

Clearly, any attempt to generalize the analytical mechanics of science to encompass the metaphysical mechanics of consciousness is a monumental

task, fraught with all manner of seductive and dangerous sinkholes of naïveté. But it is a task that is ultimately unavoidable. As Carl Friedrich von Weizsäcker put it nearly 50 years ago:

> Two fundamental functions of consciousness underlie every statement of physics: cognition and volition. (von Weizsäcker, 1941, p. 489)

We can and, wherever possible, we should keep trying to accommodate our growing assortment of empirical anomalies within the concepts and formalisms of established science. But when this fails, and when all legitimate attempts to disqualify the anomalous data subside, there is no alternative but to expand the conceptual base. Like Sherlock Holmes, when confronted by an array of valid but irreconcilable evidence, we must boldly and cleverly redefine the question. In so doing, however primitively and incompletely, we shall not only enlighten some of our own enigmas and advance our parochial understanding, but we may well offer all of science a precious key to a more powerful future paradigm for many other areas of its endeavor.

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Trends in the Study of Out-of-Body Experiences: An Overview of Developments Since the Nineteenth Century

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Abstract—A review of conceptual and research trends in the literature on out-of-body experiences is presented for the period of mid-nineteenth century to 1987. The discussion emphasizes psychological, psychiatric, and parapsychological publications. The material shows recurrent topics, but there are also some differences, particularly regarding more detailed conceptual discussions and a higher frequency of research projects in recent times. Systematic research and testable theories have been presented mainly in the last two decades. This may be related to the revival of interest in cognitive variables and altered states of consciousness in psychology during the same time period.

Out-of-body experiences (OBEs), defined as "an experience in which a person seems to perceive the world from a location outside his physical body" (Blackmore, 1982a, p. 1), have received considerable attention in recent years. This is evident in a variety of books (e.g., Gabbard & Twemlow, 1984; Irwin, 1985), and articles in psychiatry (e.g., Tobacyk & Mitchell, 1987; Twemlow, Gabbard, & Jones, 1982), and parapsychology journals (e.g., Blackmore, 1984a; Stanford, 1987).

Although a number of publications present reviews of OBE research findings and concepts (e.g., Alvarado, 1986b; Blackmore, 1982a; Irwin, 1985), there is a need for a briefer and more systematic discussion of the development of research trends and concepts as opposed to more summaries of research findings. Accordingly, in the present paper I will focus on trends, as opposed to findings, in publications on OBEs published since the middle of the nineteenth century. I hope that this general, and admittedly brief, review will be helpful to convey to the reader a sense of the main ideas and literature of the field. For convenience I have organized the material into four chronological periods: (1) the nineteenth century; (2) 1900–1939; (3) 1940–1969; (4) 1970–1987. These division are to some extent arbitrary and should not be taken to represent literal epochs, only general trends.

In this paper I will emphasize some aspects to the exclusion of others. The following material will not be included: (1) the experiences and theoretical

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ideas of persons who have had multiple OBEs or who claim to be able to have them at will (e.g., Muldoon & Carrington, 1929); (2) discussions based on "psychic" sources of information (e.g., Crookall, 1960); (3) discussions of ideas on the existence and nature of subtle bodies unrelated to OBEs (e.g., Poortman, 1954/1978); (4) accounts of attempts to detect subtle bodies when it is not clear if the subject had the *experience* of being out of the body (e.g., Durville, 1909); (5) anthropological discussions of belief in OBEs (e.g., Sheils, 1978); (6) discussions of autoscopy—or seeing an apparition of oneself—with no OBE elements (e.g., Lhermitte, 1951); and (7) studies of depersonalization experiences when it is not clear that OBEs are involved (e.g., Noyes et al., 1977).

Nineteenth Century

Most of the early (pre-1880s) views on OBEs emphasized the idea that something—the spirit or the soul—exteriorized from the body and either stayed close to the physical body or visited distant locations or dimensions.[1] An anonymous (1853, 1854) writer expressed this viewpoint in the *American Phrenological Journal.* In the writer's view the OBE may have indicated "that while the soul is normally connected with the body, and is in a great degree dependent upon it, it still may exist as a *separate entity,* entirely independent of the physical organism" (Anonymous, 1854, p. 81). Similar ideas were presented by Owen (1860), who wrote about the projection of a "spiritual portion" (p. 347) of the body, and by many other writers (e.g., Brittan, 1864; Cahagnet, 1847–1848/1850; Crowe, 1848; Jung-Stilling, 1808/1851).

The concept of the "double" or subtle body was used by many writers to explain spontaneous psychic phenomena during this period. Some of the phenomena accounted by the presumed action of this agent were apparitions of living persons (e.g., Aksakof, 1895; D'Assier, 1887; Kardec, 1861; Stead, 1896). These apparitions sometimes have an OBE component.

In his discussion of theoretical problems to account for apparitions and particularly collectively perceived ones, Frederic W. H. Myers, a classical scholar and psychical researcher, proposed that persons having OBEs may be perceived as apparitions by some sort of nonphysical modification of space. The apparition, according to Myers, appeared to be "diffused from a 'radiant point,' or phantasmogenetic focus, corresponding with that region of space where the distant agent conceives himself to be exercising his supernormal perception" (Myers, 1886, p. 291).

Myers' ideas, to which he returned later (Myers, 1903), were a reaction to Edmund Gurney's concepts of apparitions. Gurney regarded the OBE as an hallucination of the pathological type and proposed and developed the concept of telepathically induced hallucinations to explain diverse types of spontaneous ESP experiences, particularly apparitions of the living and

reciprocal apparitions[2] (Gurney, Myers, & Podmore, 1886). Others, such as Podmore (1894) and Mrs. Sidgwick (1891) also supported telepathic hallucinatory explanations.[3]

This period presented little systematic research. Exceptions are Gurney's case collection of reciprocal apparition cases and attempts at presenting an analysis of case characteristics (Gurney, Myers, & Podmore, 1886), and the induction under hypnosis of so-called "travelling clairvoyance" by others. The latter were cases in which hypnotized subjects were instructed to visit a distant location and report events occurring there, or describe the location itself. This type of "experiment" may be considered as early attempts to test for ESP during OBEs (e.g., Backman, 1891; Barth, 1849; Elliotson, 1845; Haddock, 1851).

While some, like Cahagnet (1847–1848/1850), believed that the exteriorization of some aspect of the subjects explained travelling clairvoyance, others proposed different explanations. Richet (1887) held the opinion that the experience was created by the subject's memory and imagination processes. Podmore (1894) felt that instructions given to the subjects shaped the report into "travelling" imagery.

Overview

The nineteenth century writings on OBEs were characterized by theoretical concepts, particularly those postulating the exteriorization of some aspect of the person having the experience (the spirit, double, or subtle body). However, alternate explanations postulating the concept of telepathic hallucinations were also defended. Gurney's case collection and the attempts of others to induce travelling clairvoyance by hypnosis present the first efforts towards research on OBEs.

1900–1939

While numerous case reports (e.g., Anonymous, 1929; Häning, 1932), case collections, and discussions of specific groups of cases were published during this period (e.g., Bozzano, 1934/1937; Leaning, 1928; Mattisien, 1931; Muldoon, 1936), there was practically no empirical research on the phenomena. Possible exceptions are Cornillier's (1921) travelling clairvoyance studies, and Wallace's (1925) report of attempts to move objects during OBEs.

Also interesting was a paper by Hart and Hart (1933) presenting previously published cases of OBE apparitions and comparing them to cases of apparitions of deceased persons. They wrote, "some apparitions of living persons seem to have been self-conscious personalities, while others seem to have retained only vague memories, or no memories whatever, of their having appeared" (p. 247).

As will be seen in the following discussion this period is an extension and development of previous theoretical concepts proposed as explanations of OBEs.

The concept of projection of a subtle body received considerable attention. This interest was kindled by, among other factors, the attempts at the physical detection of subtle bodies (e.g., Durville, 1909; deRochas, 1908; see also Alvarado, 1980), and by numerous publications of autobiographical accounts of persons who claimed the ability to induce the experience. These writings emphasized phenomenological aspects of the experience such as travels to distant places and "dimensions" (e.g., Fox, 1939; Lancelin, n.d.; Muldoon & Carrington, 1929; Turvey, 1911).

Funk (1907) wrote that OBE apparitions indicated the "power of the human ego to manifest itself objectively at a distance" (p. 179). Hans Driesch (1932/1933) seemed to be open to the concept in one of his books. Other authors such as Bret (1939), Mattisien (1931), and Muldoon (1936) embraced the concept of the double to explain OBEs as well as other psychic phenomena.

The work of Italian psychical researcher Ernesto Bozzano is of particular interest in this regard. In a series of publications Bozzano (1911, 1934/1937, 1938) presented a classification of aspects of the phenomena of "bilocation," by which he meant a variety of manifestations for which the concept of an externalization of or action by a subtle body was offered as an explanation. The classification included OBEs, the phantom limb sensation of amputees, autoscopy, observations of OBE apparitions, and observations of other apparitions of the living, and apparitions and luminous phenomena observed at deathbeds. As Bozzano (1938) wrote:

> The phenomena of bilocation demonstrate that within the "somatic body" there exists an indwelling "etheric body," which in rare circumstances is able to release itself temporarily from the "somatic body". The inevitable inference follows that if the etheric body is able to separate temporarily preserving its consciousness intact, we must end by recognizing that when it separates definitively at the crisis of death, the individual spirit will continue to exist. (p. 101)[4]

Bozzano's concern with the implications of the OBE to the subject of survival of bodily death[5] can be found in many other publications in the psychical research literature (e.g., Hill, 1918; Mattisien, 1931; Myers, 1903).

Psychological explanations of OBEs were proposed by other researchers. Hyslop (1912) accepted the ESP component of the experience but attributed the OB sensation to the tendency of the subconscious mind to dramatize images of locality that give the impression that the subject was in a different physical location. Others considered the OBE a mere dream (Richet, 1922), and a hallucination or the product of the imagination (Osty, 1930).[6]

Bret (1939) accepted the idea of a subtle body to explain some OBEs, but conceded that some specific cases were dreams or fantasies. Watters (1935)

also accepted the projection of a subtle body (or, as he called it, the "intra-atomic quantity"), but argued that this could only occur at death. In his opinion accounts of OBEs were to be explained as imagination and hallucinations.

Psychophysiological explanations were proposed by a few writers. Schmëing (1938) explained flying dreams by external physical stimuli perceived during sleep by withdrawal of blood from the brain that lowered the body's pressure and produced a sensation of lightness. He believed that similar factors of a greater magnitude could induce an OBE. Also, psychological aspects such as the realization that the body was no longer functional helped to induce the experience.

In trying to make sense of an OBE he experienced, Charles Quartier (in Osty, 1930) speculated on the importance of internal bodily sensations when he wrote that the OBE was "the dramatization, in visual form, of cenesthesic sensations" (p. 191, my translation). Mairie (1933) agreed with this hypothesis.

Overview

As in the previous years, there was little by way of research, and conceptual discussions predominated. In depth discussions of specific cases, or groups of cases, was also an important development. Although concepts discussed in this period were similar to those of the nineteenth century, they were somewhat more detailed in their propositions, particularly regarding supposed psychological aspects of the phenomena. Another important development was the introduction of psychophysiological theorizing.

1940–1969

In these years the trend towards more systematic study increased. Case collections of OBEs and surveys of published cases were used to study phenomenological aspects of the OBE and to argue for the objective nature of the experience (e.g., Battersby, 1942; Crookall, 1961; Hart, 1954; Muldon & Carrington, 1951).

Particularly important and influential were the publications of American sociologist Hornell Hart and English geologist Robert Crookall. Hart focused on OBE cases with ostensibly veridical or ESP elements published mainly in the psychical research literature (Hart, 1954).[7] An important methodological development was the use of a scale to measure the level of evidentiality of the reports. In Hart's words: "The scale developed rules out at the start all cases which do not present evidence that the individual who had the psychic experience reported its details before receiving evidence of their veridicality" (p. 125). The cases were classified under the following categories: (1) cases induced by hypnosis; (2) willful projection by concen-

tration; (3) projection by methods more complex than concentration; (4) spontaneous apparitions of the living corresponding to OB sensations or visions of the experiencer; (5) other cases. Hart listed eight characteristics of OBEs in his collection, and suggested that hypnosis should be seriously considered as a means to induce OBEs,[8] since it seemed to be "the method most likely to produce full and verifiable" (p. 144) experiences. In later publications Hart (1957; Hart & Collaborators, 1956) returned to similar topics and compared the phenomenology of OB apparitions (conscious apparitions of the living) with apparitions of deceased persons. In one of these papers he published the first systematic percentage breakdown of OBE case characteristics (Hart & Collaborators, 1956).

Crookall's work, published in a series of papers (e.g., 1963, 1966, 1967a), and books (e.g., 1961, 1964, 1965) was an attempt to support subtle body concepts of OBEs by analyzing phenomenological aspects of OBE reports obtained mainly from the spiritualist, psychical research, and occult literatures.[9] Crookall alleged that he had found specific OBE characteristics that differentiated the experience from hallucinations, dreams, and other subjective phenomena.

The first survey on OBE incidence was conducted by Hart (1954) with a college student sample. His study was followed by surveys done by Banks (1962), and Green (1960, 1966, 1967, 1968). Green's work is particularly important because she tried to see if there was any relationship between OBEs, sex, and academic background (1966, 1967). She also studied OBE phenomenology with new cases, as opposed to reanalysing previously published ones (1968).

In 1942 Collins commented that the study of OBEs was "a field for experimental research, which so far has been strangely neglected" (Collins, 1942, p. 74). However, soon after some studies were reported, among them the observations of "bilocation" phenomena with Italian subject Pasqualina Pezzola (Cassoli, 1954), and attempts to study ESP during hypnotically induced OBEs (e.g., Bulford, n.d.; Roll, 1975). Charles T. Tart (1967, 1968, 1969b) conducted the most important and influential studies of the period. Subjects who claimed to have frequent spontaneous OBEs or to be able to induce the experience at will were tested for ESP and monitored on psychophysiological variables such as EEG, EKG, and REM. These studies represented a transition from older and simpler to newer and more sophisticated approaches. As Irwin (1985) stated, Tart's studies "often are regarded as seminal in restoring modern parapsychologists' attention to the phenomenon of the OBE" (p. 66).[10]

Conceptually, many writers speculated on OBEs implications for survival research. While Hart (1967) and Whiteman (1965) argued for the OBE's importance as an indication of survival, others like Ducasse (1961) and J. B. Rhine (1960) argued that the experience was inconclusive evidence of survival due to the fact that reports came from living persons underscoring the

idea that the phenomenon was dependent on the physical body for its manifestation.

Other conceptual issues included discussions of the projection of subtle bodies and purely psychological explanations. Crookall (1967b), Hart (1967), and other writers (e.g., De Boni, 1960; Dumas, 1947) speculated on the existence of a subtle body (see also Whiteman's [1967] concepts of non-physical dimensions). But others, like Louisa E. Rhine (1958) were not convinced. Rhine reacted to Hart's ideas by maintaining that the explanation of OBEs was "still in question and certainly open to more than one interpretation" (p. 65). Both Broad (1959) and Eastman (1962) showed skepticism to subtle body ideas. In Gardner Murphy's opinion OBEs were "not very far from the known terrain of general psychology, which we are beginning to understand more and more without recourse to the paranormal" (Murphy with Dale, 1961, p. 287).

Among specific psychological concepts offered by other writers, Tyrrell (1942/1953) interpreted OBEs as hallucinatory constructs of the subconscious levels of personality of two or more persons working together, at a distance, through ESP means. (Tyrrell's ideas were an extension and further elaboration of the old telepathic concepts to explain reciprocal apparitions.)

Aspects such as body image (Hebb, 1960), vividness of visual imagery (Burt, 1968), and psychodynamic workings such as dramatization of the fear of death and reenactment of birth fantasies (Fodor, 1959), were also discussed. (See also the publications of Jaffé [1963], Menninger-Lerchenthal [1954], and Rawcliffe [1952]).

Equally interesting was the treatment of physiological variables by Burt (1968), Jung (Jung & Pauli, 1955), and Lippman (1953).

Overview

Attempts to conduct systematic research were more frequent in these years than in previous ones, as can be seen by the appearance of the first surveys, systematic case collections analyses, and psychophysiological experiments. Conceptual issues continued relatively unchanged, although there was a slight increase of psychological speculations.

1970–1987

The level of activity in these years was unprecedented. Obvious evidence are two symposia on the topic in parapsychology conventions (Morris et al., 1978; Palmer, et al., 1974), and the publication of the first specialized scientifically oriented monographs on the topic (Gabbard & Twemlow, 1984; Irwin, 1985).

The psychological approach has predominated but not to the complete exclusion of other ideas. Some writers have continued to present ideas based

on supposed projection of subtle bodies or on other aspects of personality (e.g., Becker, 1983; Giovetti, 1983; Tart, 1979; Vieira, 1986). This type of theorizing, however, is in minority in academic circles (for criticisms see Blackmore, 1982a; Irwin, 1985; and J. B. Rhine, 1974).[11] Some variations of these concepts have included ideas of a nonphysical or "higher space" dimension, as seen in the papers published by Greene (1983), Poynton (1975), and Whiteman (1975).

A great number of authors have offered psychological concepts to explain OBEs. Some proposed that the OBE is a defense mechanism designed to deal with the threat of death (Ehrenwald, 1974), or the loss of love (Reed, 1974). Others discussed the phenomenon in terms of archetypal images (Fisher, 1975), distortion of the body image (Horowitz, 1970), depersonalization (Whitlock, 1978), lucid dreams (LaBerge, 1985), or a variety of concepts basically postulating that the OBE is a hallucinatory creation arising under different psychological factors (e.g., Brent, 1979; Capel, 1978; Gabbard & Twemlow, 1984; Zusne & Jones, 1982).

Three writers have proposed the most important psychological ideas of the period. John Palmer (1978) conceptualized the OBE basically as a response to a body image change causing a threat to individual identity. Susan J. Blackmore (1984b) proposed that OBEs were a model of reality created by the organism using internal cognitive resources when the models dependent on sensory input were disrupted. Finally, Harvey J. Irwin (1985) emphasized attentional cognitive processes and attenuation of somatic sensory input, as well as a hypothetical synesthetic process accounting for a transformation of one sensory mode experience for another. These ideas were more detailed than the rest of the above mentioned speculations and presented testable predictions.

During these years there was also an unprecedented level of research following some of the above mentioned psychological concepts. In several surveys researchers explored possible relationships of OBEs to imagery and attentional capacities (e.g., Blackmore, 1982c, 1987; Irwin, 1980; Myers et al., 1983), to altered states of consciousness and related practices and experiences (e.g., Blackmore, 1982b; Kohr, 1980; Palmer, 1979), and to personality variables (e.g., Irwin, 1981, Jones, Gabbard, & Twemlow, 1984; Myers et al., 1983; Tobacyk & Mitchell, 1987). In other studies attempts were made to obtain increasingly detailed information on OBE phenomenological characteristics (e.g., Alvarado, 1984; Blackmore, 1984a; Giovetti, 1983; Poynton, 1975), including ESP claims (e.g., Alvarado, 1986a).

Information on demographic aspects (e.g., Kohr, 1980; Palmer, 1979), and physiological variables such as form of birth (Blackmore, 1983b), and proneness to migraine (Irwin, 1983), was also collected.[12]

Interest in experimentation also followed the predominantly psychological approach. Researchers explored ESP scores during claimed laboratory OBEs (e.g., Harary & Solfvin, 1977; Palmer & Vassar, 1974), as well as different psychological variables such as expectation, the effect of induction

procedures (e.g., Palmer & Lieberman, 1975; Palmer & Vassar, 1974), and other factors (e.g., Smith & Irwin, 1981; Nash, Lynn, & Stanley, 1984).

Experimental attempts to measure psychophysiological correlates of the experience (e.g., Morris et al., 1978; Osis & Mitchell, 1977), and to test in a physical way (e.g., by vibrations or the detection of electromagnetic activity) projection models of OBEs have also been published (Morris et al., 1978; Osis & McCormick, 1980).

Overview

While most of the elements found in previous years were present in the last period, the amount of serious and organized research and theory building was unprecedented. The psychological approach predominated in both conceptual and research developments. Additionally, the first psychological OBE theories with clearly testable predictions were presented.

Discussion

In this paper I have outlined briefly the trends of over a century of OBE research and theorization. Although several features have recurred throughout the periods discussed (such as the use of subtle bodies and psychological constructs as explanatory concepts), there have been differences in the frequency of systematic and empirical studies in different time periods. Particularly noticeable are the differences between the more recent years and previous ones. The modern period has a higher frequency of psychological concepts and empirical research, and consequently, has seen an increase in publications on OBEs in parapsychology and in the journals of other disciplines (e.g., Nash, Lynn, & Stanley, 1984; Twemlow, Gabbard, & Jones, 1982). This last development suggests that the OBE is no longer of interest only to parapsychologists but, as other human experiences and altered states of consciousness, it is beginning to pique the interest of a variety of disciplines that deal with anomalous psychological behaviors and reported experiences.

To some extent interest in OBEs in recent years may be seen as the consequence of conceptual changes during the 1960s and the 1970s in psychology (e.g., Holt, 1964; Tart, 1969a), and parapsychology (e.g., George & Krippner, 1984; Honorton, 1977) that brought increased attention and research on cognitive processes and altered states of consciousness. OBEs constituted a logical and appropriate subset of the general problem area of imagery and altered states of consciousness such as dreams, meditation, and drug-induced states.

Regardless of the reasons for the current shift of interests (and we could certainly speculate on the increasing influence of occultism in general, as well as on the impact of oriental philosophy) there may be benefits for modern researchers in realizing that OBE studies have a long and varied

history. A good grounding in this literature may be useful to the pragmatic goals of future researchers.

Endnotes

[1] These views are intimately related to ancient concepts of subtle bodies in religious and occult literatures (e.g., Mead, 1919; Poortman, 1954/1978) and to speculations on the "seat of the soul" (e.g., Bruyn, 1982).

[2] These are cases "in which a person who is undergoing an OBE, and finds himself at or 'projects' himself to a particular spot distant from his physical body, has been seen at that very spot by some person present there" (Gauld, 1982, p. 222).

[3] All the persons mentioned in this paragraph were members of the Society for Psychical Research (SPR), founded in London in 1882 to study anomalous psychological and physical phenomena (Gauld, 1968). The ideas mentioned here may be seen in the context of the work and concepts of some early SPR members regarding the study of "thought-transference" as a human ability and as the explanation of apparitions of the living and phenomena produced by mental mediums, as opposed to ideas involving the concepts of subtle bodies and spirit communication (Cerullo, 1982; Gauld, 1968). Myers was the only leading SPR member to challenge such concepts in relation to apparitions, particularly reciprocal ones.

[4] For a criticism of some of Bozzano's assumptions and conclusions see de Vesme (1934a, 1934b). Odeberg (1938) seems to agree with Bozzano's methods and concepts.

[5] The study of phenomena that may suggest the possibility of survival of death, such as communications received through mediums and apparitions of deceased persons, has traditionally been an area of parapsychological research (for reviews see Gauld, 1982, and Stevenson, 1977).

[6] For criticisms of Osty's views on the grounds that he ignored aspects of OBEs suggestive of the action of an objective subtle body, and for the general weakness of his arguments and assumptions see Bozzano (1934/1937) and Mattisien (1932).

[7] Soon after the First International Conference of Parapsychological Studies, held in Utrecht in 1953, at which Hart presented a few papers (e.g., Hart, 1955), Hart initiated an International Project on ESP Projection to work on OBEs and apparitions through correspondence with other researchers (Hart and Collaborators, 1956).

[8] See also C. D. Broad's (1948) remarks on the subject.

[9] Irwin (1985) published several important methodological criticisms of Crookall's work. Among them, he pointed out that Crookall's classificatory scheme was unsystematic and extremely subjective.

[10] Tart's attention to the psychophysiology of the OBE may be seen as a natural extension of previous developments in the study of the psychophysiology of diverse altered states of consciousness (e.g., Anand, Chhina, & Singh, 1961; Aserinsky & Kleitman, 1953).

[11] Also in the minority were defences of the importance of OBEs to the issue of survival of bodily death (e.g., Becker, 1983; Crookall, 1973), since most discussions on the topic during this period were skeptical on the issue (e.g., Blackmore, 1983a; Gauld, 1982; Moore, 1981). An important exception was Di Simone's (1984) attempts to compare the accounts of a subject's OBEs with the accounts of a supposed spiritual entity communicating through a medium relating encounters in a spiritual dimension.

[12] This period also presents much research on the so-called near-death experience (e.g., Greyson & Stevenson, 1980; Ring, 1980; Sabom, 1981; Twemlow & Gabbard, 1984–1985). A useful overview of the field appears in Greyson and Flynn's (1984) anthology.

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A Methodology for the Objective Study of Transpersonal Imagery*

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Abstract—Abundant methodologies already exist for the study of *preverbal imagery,* in which one's imagery acts upon one's own cellular, biochemical, and physiological activity. This paper reports a new methodology for the objective study of *transpersonal imagery,* in which one person's imagery may influence the physical reactions of *another* person. The method involves the instructed generation of specific imagery by one person and the concurrent measurement of psychophysiological changes in another person who is isolated in a distant room to eliminate all conventional sensorimotor communication. Thirteen experiments were conducted using this methodology. A significant relationship was found between the calming or activating imagery of one person and the electrodermal activity of another person who was isolated at a distance (overall $z = 4.08$, $p = .000023$, mean effect size = 0.29). Potential artifacts which might account for the results are considered and discounted. The findings demonstrate reliable and relatively robust anomalous interactions between living systems at a distance. The effects may be interpreted as instances of an anomalous "*causal*" influence by one person directly upon the physiological activity of another person. An alternative interpretation is one of an anomalous *informational* process, combined with unconscious physiological self-regulation on the part of the influenced person. Additional research is being conducted in an attempt to increase our understanding of the processes involved, as well as to learn the various physical, physiological and psychological factors that may increase or decrease the likelihood of occurrence of the effect.

Introduction

In her book *Imagery in Healing* (Achterberg, 1985), psychologist Jeanne Achterberg distinguished two types of imagery which may have positive impacts upon health. In *preverbal imagery,* the imagination acts upon one's own physical being to alter cellular, biochemical, and physiological activity. The study of such imagery has a long history, and there exist a variety of successful methodologies for its objective evaluation. The second type of imagery that Achterberg identified is *transpersonal imagery,* which "embod-

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ies the assumption that information can be transmitted from the consciousness of one person to the physical substrate of others" (p. 5). She suggested that the validation of transpersonal imagery must be sought in the more qualitative types of observational data gathered by anthropologists, theologians, and medical historians, and in intuitive philosophical speculation.

Indeed, the power of preverbal imagery in influencing one's own chemical, cellular, physiological and behavioral reactions has been well documented. We find extensive evidence for such psychosomatic influences in the areas of dreaming, hypnosis, relaxation, autogenic training, biofeedback, meditation, therapeutic imagery, mental rehearsal, and placebo effects. Some of the most exciting (and potentially useful) findings regarding the influence of imagery on somatic functioning are now being reported by researchers within the new interdisciplinary field of psychoneuroimmunology, in which it is being discovered that individuals, through use of relaxation, hypnosis, and imagery techniques, may be able to exert rapid and quite specific influences upon certain subpopulations of their white blood cells [see, for example, Hall (1984a, 1984b, 1987); Peavey (1982); Schneider, Smith, & Whitcher (1984)].

Less well known are the various observations which tend to support the reality and effectiveness of transpersonal imagery effects. There are, of course, abundant anecdotes and field observations that the sensations, thoughts, feelings and images of one person may, under certain conditions, directly affect the bodily reactions of another person, even when the two persons are separated by great distance, and when the influenced person is not aware that an influence attempt is being made. Observations of ostensible distant mental influence in the context of anthropology have been reviewed by Angoff and Barth (1974), Long (1977) and Van de Castle (1977). The late Eric Dingwall, in his four-volume work, *Abnormal Hypnotic Phenomena* (1968), surveyed many cases of putative distant mental influence which occurred in 19th-century practices of hypnosis (or "mesmerism," as it was then called). Two of the more interesting of these "higher phenomena of hypnosis" were (a) *community of sensation,* in which hypnotized subjects were reported to have responded appropriately to sensory stimuli presented to a distantly located hypnotist, and (b) *mental suggestion,* in which the hypnotist was alleged to have exerted an influence upon a distant subject's behavior (while the latter was in a hypnotic "trance") or even to have induced hypnosis itself at a distance. These phenomena, as well as the results of more modern hypnotic investigations, have been examined by Honorton (1974, 1977). Finally, possible distant mental influence effects occurring within the context of mental healing have been reviewed by Ehrenwald (1977) and by Solfvin (1984).

The possibility of distant somatic effects of imagery is also suggested by anecdotal reports of various investigators involved in clinical biofeedback applications who sometimes observed unusual correlations between the changes in electrophysiological activity of one client and those of another

client (in group biofeedback training sessions) or between the client's activity and that of the investigator himself or herself. If such coincident physiological patterns are reliable and replicable, they might be explained most parsimoniously by assuming that they result from either (a) gross or subtle external stimuli that influence both persons in the same manner, or (b) internal rhythms that happen to be in phase in the two persons and interact with the monitored activities in identical ways. A third possibility, however, is that at least some proportion of these physiological congruences may be attributable to transpersonal imagery effects. Such a possibility would be highly speculative were it not for several reports of experimental findings of similar interactions between, for example, the electroencephalic (Duane & Behrendt, 1965; Puthoff & Targ, 1976; Targ & Puthoff, 1974) or autonomic (Dean, 1966) activity of one person and that of another person, when those persons were remotely situated, shielded, and the possibility of conventional energetic and informational exchanges between them had been eliminated. Indeed, the entire body of research findings in the areas of psychical research and of parapsychology is relevant to and supportive of the notion that the mental activity of one person may influence the bodily activity of another person at a distance. Quite complete and useful reviews of the concepts, methods, findings, and theories of modern parapsychology may be found in Edge, Morris, Palmer and Rush (1986); Krippner (1977, 1978, 1982, 1984); Nash (1986); and Wolman (1977).

The Present Research Program: Purpose and Overview

In this paper, we describe an objective, quantitative methodology for the study of transpersonal imagery which allows the investigation of the latter within the framework of experimental psychology. In addition to the methodology itself, we shall present the promising results of 13 experiments that we already have conducted in order to test the usefulness of the procedure.

The method involves the instructed generation of specific imagery by one person, and the concurrent measurement of psychophysiological changes in *another* person. Throughout the experiment, the two persons occupy separate, isolated rooms, and all conventional sensorimotor communication between the two persons is eliminated in order to insure that any obtained effects are truly transpersonal. In a typical experiment, Person A is instructed to use specific mental imagery in order to induce a specific physiological change in Person B, who is isolated in a distant room. The expected psychophysiological effect is assessed by measuring the spontaneous electrodermal activity (skin resistance responses, SRR) of Person B during 20 30-second recording epochs. During 10 of these epochs, interspersed randomly throughout the sequence of 20 epochs, Person A generates imagery designed to produce a specific somatic effect (decreased sympathetic nervous system activity in some cases, increased sympathetic activation in other cases); the remaining 10 epochs serve as Control periods during which

Person A does not generate the relevant imagery. Person B is, of course, unaware of the sequence of the two types of epochs (the sequence is randomly determined) and is also "blind" to the exact starting time of the experiment, the number and timing of the various periods, etc. Electrodermal activity is objectively assessed by an electrodermal amplifier interfaced with an analog-to-digital converter and a microcomputer. The amount of electrodermal activity during the Imagery epochs is compared with that of the Control epochs using conventional parametric statistical techniques.

If the experimental protocol just described is not violated, and yet it is found that significantly greater somatic activity of an appropriate, imagery-relevant type is found to occur during the Imagery periods than during the Control periods, we can conclude with confidence that a transpersonal imagery effect (TIE) has occurred, and that the results cannot be attributed to (a) conventional communication channels or cues (since the two parties are isolated from contact with each other through the use of distant, isolated rooms), (b) common external signals, common internal rhythms, or rational inference of the imagery/nonimagery schedule and resultant appropriate self-regulation (since the imagery/nonimagery schedule is truly randomly determined and is unknown to Person B), or (c) "chance coincidence" (since the level of responding to be expected on the basis of chance alone may actually be determined and compared statistically with the obtained response levels).

Method

Subjects

The experiments involved the participation of unpaid male and female volunteer subjects, ranging in age from 16 to 65 years. Participants were selected from a pool of volunteers from the San Antonio community who had learned about the Foundation's experiments through local newspaper advertisements and articles, notices posted throughout the city, lectures given by Foundation staff at local colleges and universities, and comments from other participants, and whose interest in the experiments and time schedules permitted participation. Approximately equal numbers of males and females participated in the various studies. In most cases, participants were not selected on the basis of any special physical, physiological, or psychological characteristics, and could best be described as "self-selected" on the basis of their interest in the topics being researched. In only one experiment were "special" subjects recruited and selected. This was an experiment in which we were interested in whether persons having a greater "need" for a possible *calming* influence would evidence stronger results than persons without such a need. Therefore, for that experiment, we selected individuals who self-reported symptoms of greater than usual sympathetic autonomic activation—i.e., stress-related complaints, excessive emo-

tionality, excessive anxiety, tension headaches, high blood pressure, ulcers, or mental or physical hyperactivity. The subjects for this experiment were also screened in an initial electrodermal activity recording session to guarantee that they did in fact exhibit greater than average sympathetic autonomic activity.

The persons who served as "influencers" in these experiments (i.e., those who regulated their own images and intentions in order to influence the subjects at a distance) were selected from a similar pool of volunteers. In some experiments, the experimenters themselves served as influencers. In still other experiments, the influencers were individuals who were interested in unorthodox healing and who themselves practiced certain mental healing techniques, such as "therapeutic touch" (see Borelli & Heidt, 1982; Krieger, 1979; Kunz, 1985) or "Reiki healing" (see Schlitz & Braud, 1985). Many of the influencers were practitioners of various forms of meditation and self-exploration. In most cases, however, the influencers were simply interested persons from the local community who wished to give the experiments a try.

The authors served as the experimenters for the series of studies, assisted in some experiments by two other experimenters, J. C. and H. K. The first author had extensive research experience in the areas of experimental psychology, physiological psychology, and parapsychology. The second author had extensive experience in parapsychological and anthropological research. The third experimenter, J. C., had research experience in the area of nursing. The fourth experimenter, H. K., was a student at a local college who was participating in a research practicum at the Foundation.

In all, 337 persons participated in these experiments. Of these participants, 271 served as subjects, 62 as influencers, and 4 as experimenters.

Procedure

Physical Layout. During the experimental sessions, it was essential to guarantee that the influencer and the experimenter would not be able to communicate with the subject via conventional sensorimotor channels. This was accomplished by situating the experimenter and the influencer in one closed room, while the subject occupied a distant second room, which was also closed. Figure 1 illustrates the floor plan of the rooms used in Experiments 1 through 10. The rooms used in Experiments 11 through 13 are shown in Figure 2. The distance (20 meters or more) between the two rooms used in an experiment, and the presence of several intervening closed doors and corridors, isolated the participants from possible sensory interaction. Additionally, verbalization of any information regarding the imagery/nonimagery schedule (see below) by the influencer or the experimenter was not allowed during the experimental sessions. There were no active microphones in either room, through which participants could communicate. The headphones through which the participants in the two rooms received required auditory information were attached to independent electrical circuits

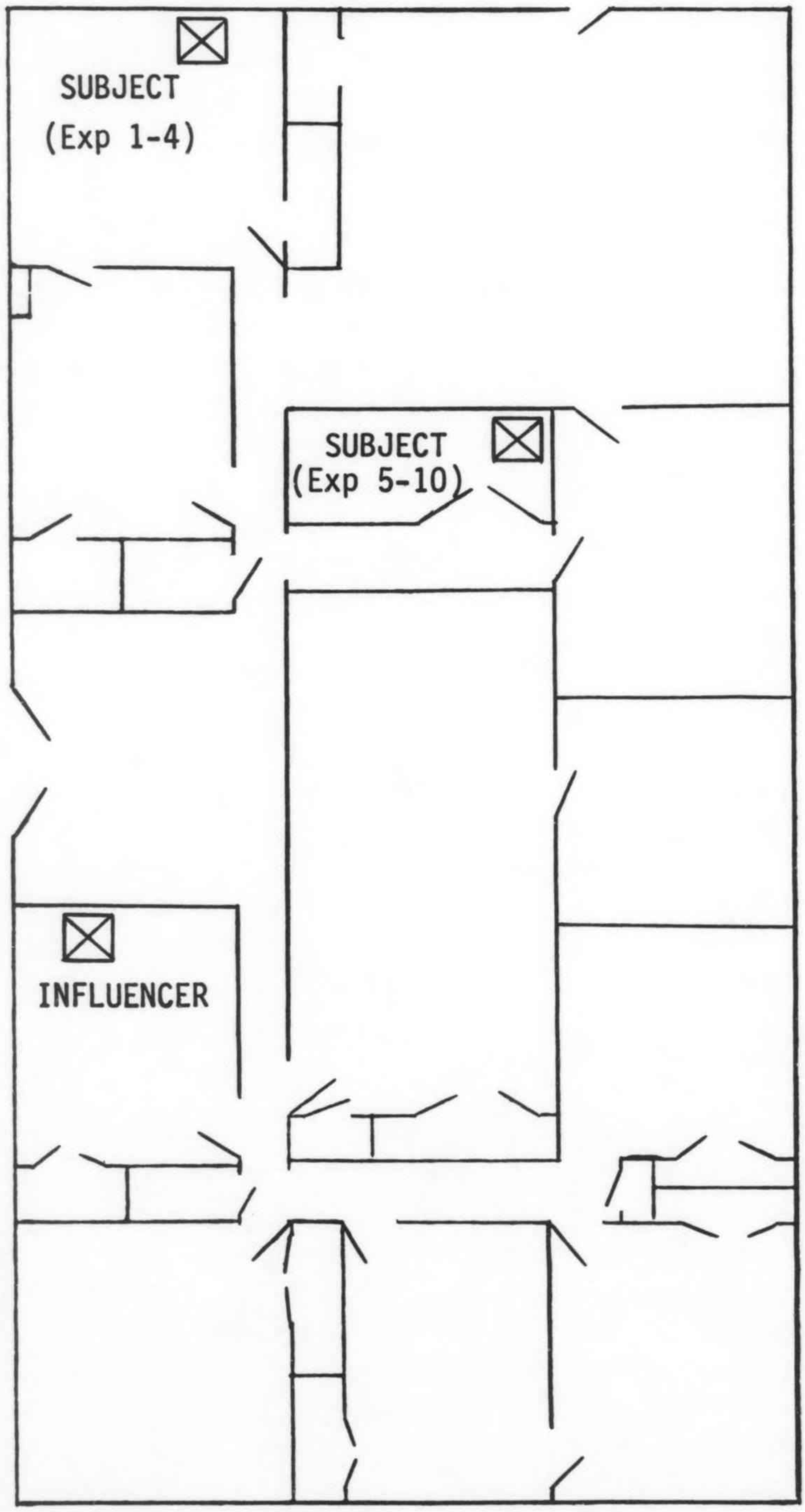

Fig. 1. Laboratory floor plan showing locations of subject and influencer for Experiments 1 through 10.

so that possible "crosstalk" between two sets of headphones was eliminated (i.e., it was impossible for one person's headphone to function as a microphone for the other person's headset).

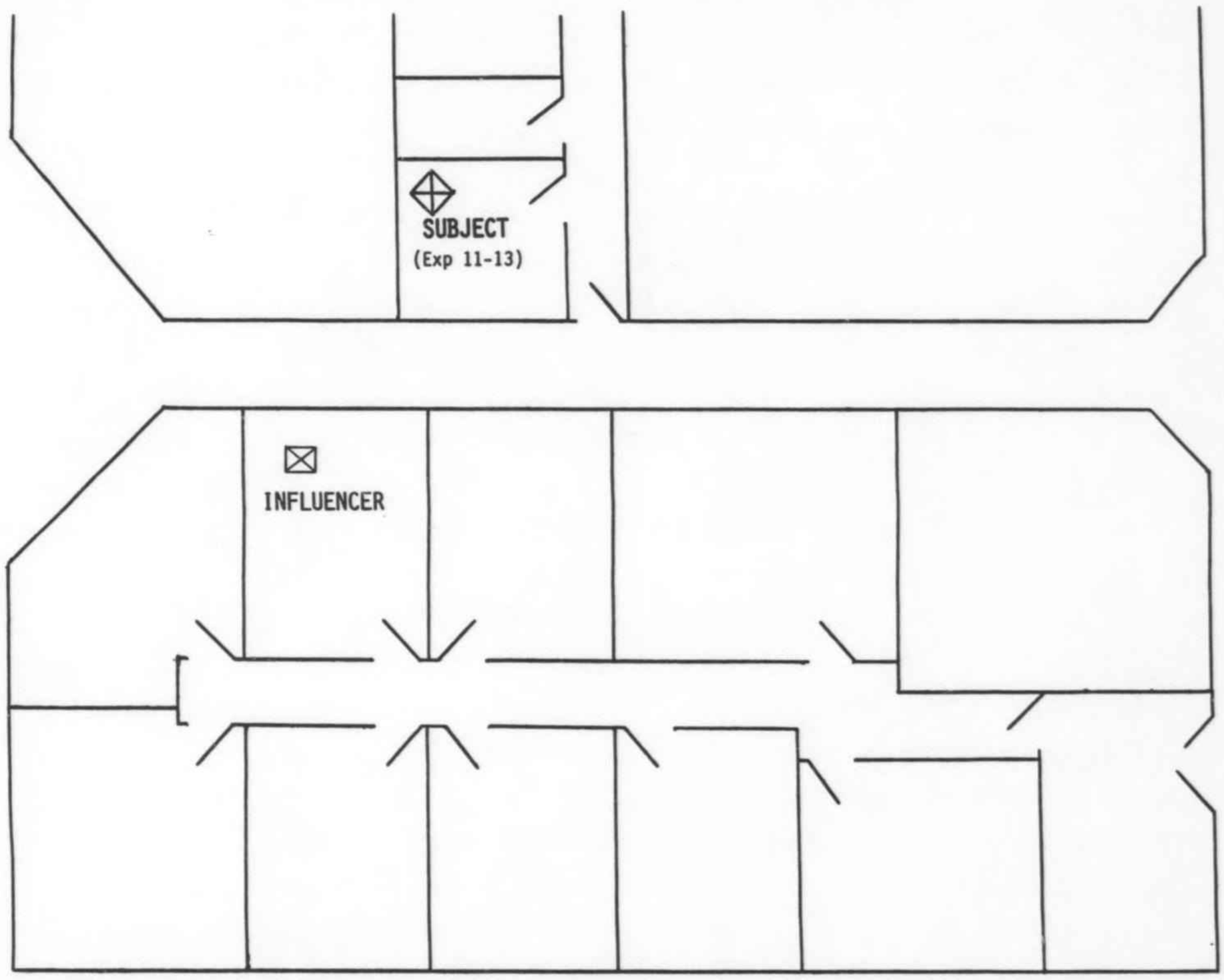

Fig. 2. Laboratory floor plan showing locations of subject and influencer for Experiments 11 through 13; subject and influencer rooms are in separate suites of the same building, separated by an outside corridor and several closed doors.

Subject's Instructions and Activities. Throughout an experimental session, the subject sat in a comfortable armchair in a dimly illuminated, closed room. In Experiments 1 and 3, the subject was exposed to visual and acoustic ganzfeld stimulation throughout the session (see Bertini, Lewis, & Witkin, 1964; Schacter, 1976); this was accomplished by having the subject view a uniform red light field through translucent, hemispherical acetate eye covers while listening to moderately loud white noise through headphones. In Experiments 2 and 4, ganzfeld stimulation was not employed; rather, the subject simply sat quietly in the dim room, with freedom to open or close the eyes as desired. In Experiments 5 through 13, the subject watched randomly changing patterns of colored lights on a 12-inch display screen 2 meters away, while listening to computer-generated random sounds through headphones. The subject was instructed to make no deliberate effort to relax or to become more active, but rather to remain in as ordinary a condition as possible and to be open to and accepting of a possible influence from the distant influencer whom he or she had already met. The subject remained unaware of the number, timing or scheduling of the various influence attempts, and was instructed not to try to guess consciously when influence attempts might be made. The subject was asked to allow his or her thought

processes to be as variable or random as possible and to simply observe the various thoughts, images, sensations, and feelings that came to mind without attempting to control, force, or cling to any of them.

Influencer's Instructions and Activities. The influencer sat in a comfortable chair in front of a polygraph in another closed room. The polygraph provided a graphic analog readout of the concurrent electrodermal activity of the distant subject. For half of each session for Experiment 6, this polygraph was turned off and no feedback was allowed. For all other sessions of all other experiments, polygraph feedback information about the momentary physiological activity of the subject was available to the influencer. The influencer had the option of attending to this polygraph feedback or ignoring it. In most cases, the influencer watched the polygraph tracing throughout a session. In some cases, the influencer closed his or her eyes and ignored the polygraph tracing during the actual 30-second imagery or nonimagery periods (see below), but looked at the tracings following those periods in order to learn of the success or failure of the influence attempts.

An experimental session contained 20 30-second recording periods or epochs. Each epoch was signaled to the experimenter and to the influencer by an auditory signal that could not be heard by the distant subject. Immediately before each signal, the experimenter exposed a card to the influencer. This card contained an instruction for the upcoming epoch. The word "influence" indicated that the next 30-second period was to be an imagery epoch during which the influencer would attempt to influence the distant subject; the word "control" indicated a nonimagery or noninfluence period. The influencer had been instructed beforehand that during each influence period, he or she was to attempt to influence the electrodermal activity of the distant subject through the use of self-generated imagery. In some experiments (Experiments 5, 6, 8, 10 and 11), the goal of the imagery influence attempts was the *calming* of the distant subject—the reduction of the subject's sympathetic autonomic nervous system activity and hence the reduction of the frequency and magnitude of spontaneous skin resistance responses. In other experiments (Experiments 2, 4, and 7), the goal of the imagery influence attempts was the *activation* of the distant subject—an increase in the subject's sympathetic autonomic nervous system activity and hence an increase in the frequency and magnitude of spontaneous skin resistance responses. In still other experiments (Experiments 1, 3, 9, 12 and 13), both calming and activation strategies were used within a single session; in those experiments, there were 10 calm-aim periods and 10 activate-aim periods.

During control periods, the influencer attempted not to think about the subject or about the experiment, and to think of other matters. During influence periods, the influencer used the following strategies (either alone or in combination) in an attempt to influence the somatic activity of the distant subject.

1. The influencer used imagery and self-regulation techniques in order to induce the intended condition (either relaxation or activation, as demanded by the experimental protocol) in *himself or herself,* and imagined (and intended for) a corresponding change in the distant subject.
2. The influencer imagined the *other person* in appropriate relaxing or activating settings.
3. The influencer imagined the desired outcomes of the polygraph pen tracings—i.e., imagined few and small pen deflections for calming periods and many and large pen deflections for activation periods.

There were rest periods, ranging in duration from 15 seconds to 2 minutes in the various experiments, between the 30-second recording epochs. During those periods, the influencer was able to rest and to prepare for the upcoming epoch.

Scheduling of Influence Attempts. In order to eliminate the possible influence of common internal rhythms and to remove the possibility that the influencer and the subject just happened to respond at whim in the same manner and at the same times, it was necessary to *formally assign* to the influencer specific times for engaging in imagery; such assignments had to be truly random and, of course, could not be known to the subject (lest the subject self-regulate his or her own physiology on the basis of such knowledge, in order to confirm the expectations of the experimenter). The subject's blindness with respect to the imagery/nonimagery sequence was maintained by keeping *all* participants (including the experimenter) blind regarding the sequence until preparatory interactions with the subject had been completed and the session was about to begin. Only then, when the subject and the influencer/experimenter team were stationed in their separate rooms, did the experimenter become aware of the proper epoch sequence for that session. In Experiments 1 and 3, the epochs were scheduled in a truly random manner by means of an electronic binary random event generator (see Schmidt, 1970). In Experiments 2 and 4, the epochs were randomly scheduled by means of a set of 20 cards (10 influence and 10 control cards) which were shuffled by the experimenter 20 times before each session. In the remainder of the experiments, the epochs were scheduled in an ABBA or BAAB sequence; the experimenter learned whether a particular session's sequence was to be ABBA or BAAB by consulting a sealed envelope immediately before the beginning of each session. The envelopes had been prepared beforehand by someone who had no further role in the experiments. The "preparer" had prepared each session's sequence envelope through the use of a table of random numbers, with the only restriction for its use being the occurrence of equal numbers of ABBA and BAAB sequences in an experiment. [We used an ABBA design in order to minimize possible progressive error in the experiments; such a design allows any progressive error (i.e., the contribution of any extraneous variable which

varies systematically with time) which may have autonomic concomitants to contribute equally to the A and B periods, thus avoiding a biasing contribution to any one condition alone.]

Monitoring of Electrophysiological Activity. The subject's sympathetic autonomic nervous system activity was assessed by monitoring his or her spontaneous skin resistance responses (SRR) on a continuous basis throughout the 20 minutes of an experimental session. In Experiments 1 and 3, SRR activity was recorded by means of silver/silver chloride electrodes (7.0 mm in diameter) with partially conductive electrode gel, attached by adhesive collars to the subject's right palm. Phasic electrodermal activity was recorded by means of a Stoelting Model SA 1473 GSR amplifier and a Stoelting Model 22656 Multigraphic Recorder. Sensitivity was adjusted so that an internal calibrating signal of 1.0 kilohm resulted in a 10.0 mm recording pen deflection. In Experiments 2 and 4, a Lafayette Model 76405 multiplex GSR amplifier was used, along with the Stoelting chart-mover/penwriter described above; chrome-plated stainless steel finger electrodes (each with a surface area of 585 mm^2) without electrode paste were attached to the first and third fingers of the left hand by means of Velcro bands. In Experiments 5, 6, and 7, the Lafayette amplifier was used along with a Harvard Apparatus chart mover and pen writers; the steel/pasteless finger electrodes were attached to the subject's right hand. In Experiments 8 through 13, the Lafayette amplifier and Harvard chart recorder were used, but with silver/silver chloride electrodes and partially conductive gel; electrodes were attached to the subject's right palm. For Experiments 1 through 4, electrodermal activity was evaluated by blind-scoring of pen tracings by someone who had no other role in the experiments.[1] For Experiments 5 through 13, scoring was automated through the addition of an analog-to-digital converter interfaced with a microcomputer. This equipment sampled the subject's SRR activity 10 times each second for the 30 seconds of a recording epoch and averaged these measures, providing what is virtually a measure of the area under the curve described by the fluctuation of electrodermal activity over time (i.e., the mathematically integrated activity). The computer provided a paper printout of the results at the end of the session. For all experiments, with the exception of Experiment 13, a 5-minute adaptation/habituation period for the subject preceded the actual experimental session. For Experiment 8, other physiological measures were recorded in addition to electrodermal activity (*viz.*, pulse rate, hand temperature, breathing rate, and electromyographic activity of the frontalis muscle group); those measures, however, will not be described in this paper.

Assessment of Physiological Responses. Each session of each experiment yielded 10 assessments of electrodermal (SRR) activity recorded during an influencer's attempts to influence that activity in a specific direction using specific imagery, and 10 assessments of activity recorded in the absence of such attempts. (The sole exception to this occurred in Experiment 13, in

which there was a total of only 12 recording epochs for each session, rather than the usual 20.) Our evaluation of whether the influencer's imagery influenced the subject's somatic activity was carried out on a session-by-session basis, and involved a determination of the proportion of somatic activity in the prescribed direction which occurred during the influence periods, relative to its occurrence during control periods. For each session, we calculated the total activity for that session by summing the SRR scores for all 20 30-second recording epochs (or for all 12 epochs, in the case of Experiment 13). Next, we calculated the activity that occurred during the 10 30-second influence or imagery epochs of a session by summing those 10 scores; separately, we calculated the activity occurring during the 10 30-second control (i.e., noninfluence or nonimagery) epochs of the session by summing those 10 scores. Dividing the influence and control sums, respectively, by the total activity yielded two activity proportions. In the absence of a transpersonal imagery effect (TIE), each of these two proportions would be expected to approximate 0.50; i.e., on the basis of chance alone, half of a subject's total electrodermal activity would be expected to occur during the influence periods and half during the control periods. A significant departure of these proportions from 0.50, in the appropriate predicted direction, would constitute evidence for the presence of a transpersonal imagery effect.

Results

We have completed 13 experiments using the methodology described above. Experiments 1, 2, 3, 4, and 11 were "demonstration studies" conducted to test the effectiveness of the method with different samples of subjects and influencers. In the remaining eight experiments, we sought to determine how the transpersonal imagery effect might be influenced by various psychological factors. Since our purpose in this paper is to describe the method itself, we shall not present the rationales, details, or specific outcomes of the individual experiments, but will limit our remarks to the common features of the studies and to their overall results.

In each experiment, the primary method of analysis involved a comparison of the proportion of electrodermal activity which occurred during the imagery influence epochs of a session with the proportion expected on the basis of chance alone, i.e., 0.50. Chi-square goodness of fit tests indicated that the distribution of obtained session scores did not differ significantly from a normal distribution; therefore, parametric statistical tests were used for their evaluation. Single-mean *t* tests were used to compare the obtained session scores with an expected mean of 0.50.

Summary statistics for the 13 experiments are presented in Table 1.

For experiments (such as Experiments 5 and 13) in which significant differences obtained between different subconditions and/or in cases in which *a priori* decisions had been made to evaluate certain groups separately, scores are presented for each subcondition; otherwise, scores of sub-

TABLE 1
Quantitative summary of transpersonal imagery experiments

Experiment	Influencer(s)	Number of Sessions	Hit Sessions	Mean % Influence	*t*	*p*	*z*	*d*	Type of Study
1	Experimenter	10	9	9%	3.07	.0065	2.73	.97	Demonstration
2	M. M.	10	8	9%	2.04	.035	1.81	.64	Demonstration
3	10 unselected volunteers	10	8	8%	2.96	.0077	2.42	.94	Demonstration
4	10 unselected volunteers	10	5	−3%	−0.76	.736	−0.63	−.24	Demonstration
5	Experimenters	16	12	10%	2.40	.014	2.20	.60	Need (greater)[4]
5	Experimenters	16	6	0%	−0.09	.537	−0.09	−.02	Need (lesser)[4]
6	24 unselected volunteers	24	17	7%	1.77	.043	1.72	.36	Feedback (within)[5]
7	Experimenters	32	16	3%	1.15	.13	1.13	.20	Blocking[5]
8	Experimenters	30	15	2%	0.45	.33	0.44	.08	Specificity[5]
9	Experimenters	30	18	1%	0.44	.33	0.43	.08	Direction[6]
10	Experimenters	16	7	3%	1.31	.10	1.28	.33	Magnitude (within)[6]
11	3 healing practitioners	15	9	1%	0.62	.28	0.58	.16	Demonstration (Reiki method)[7]
12	5 selected volunteers	40	19	1%	0.21	.41	0.23	.03	IDS pilot (within)[8]
13	8 selected volunteers	32	20	7%	2.14	.02	2.08	.38	IDS confirmation single seed (within)[8]
13	8 selected volunteers	32	14	−2%	−0.53	.70	−0.52	−.09	IDS confirmation multiple seeds (within)[8]

conditions are combined and presented for the experiment as a whole. The number of sessions contributing to each experiment varied from 10 to 40. The single-mean *t* tests produced independently significant evidence for the transpersonal imagery effect (i.e., an associated *p* of 0.05 or less) in 6 of the possible 15 cases, yielding an experimental success rate of 40%. The experimental success rate expected on the basis of chance alone is, of course, 5%.

Results for the 13 experiments are presented in another form in Figure 3. For this presentation, we calculated *z* scores and effect size scores for the overall results of each experiment. The *z* scores were calculated according to the Stouffer method [see Rosenthal (1984)] which involves converting the studies' obtained *p* values into *z* scores, summing these *z* scores, and dividing by the square root of the number of studies being combined; the result is itself a *z* score that can be evaluated by means of an associated *p* value. For Figure 3, this method was used to provide an overall or combined *z* score for each of the 13 experiments, for ease of graphical portrayal. The effect sizes shown in Figure 3 are "Cohen d" measures which are recommended by those interested in meta-analyses of scientific experiments [see Cohen (1969); Glass, McGaw, & Smith (1981); Rosenthal (1984)]; the effect sizes

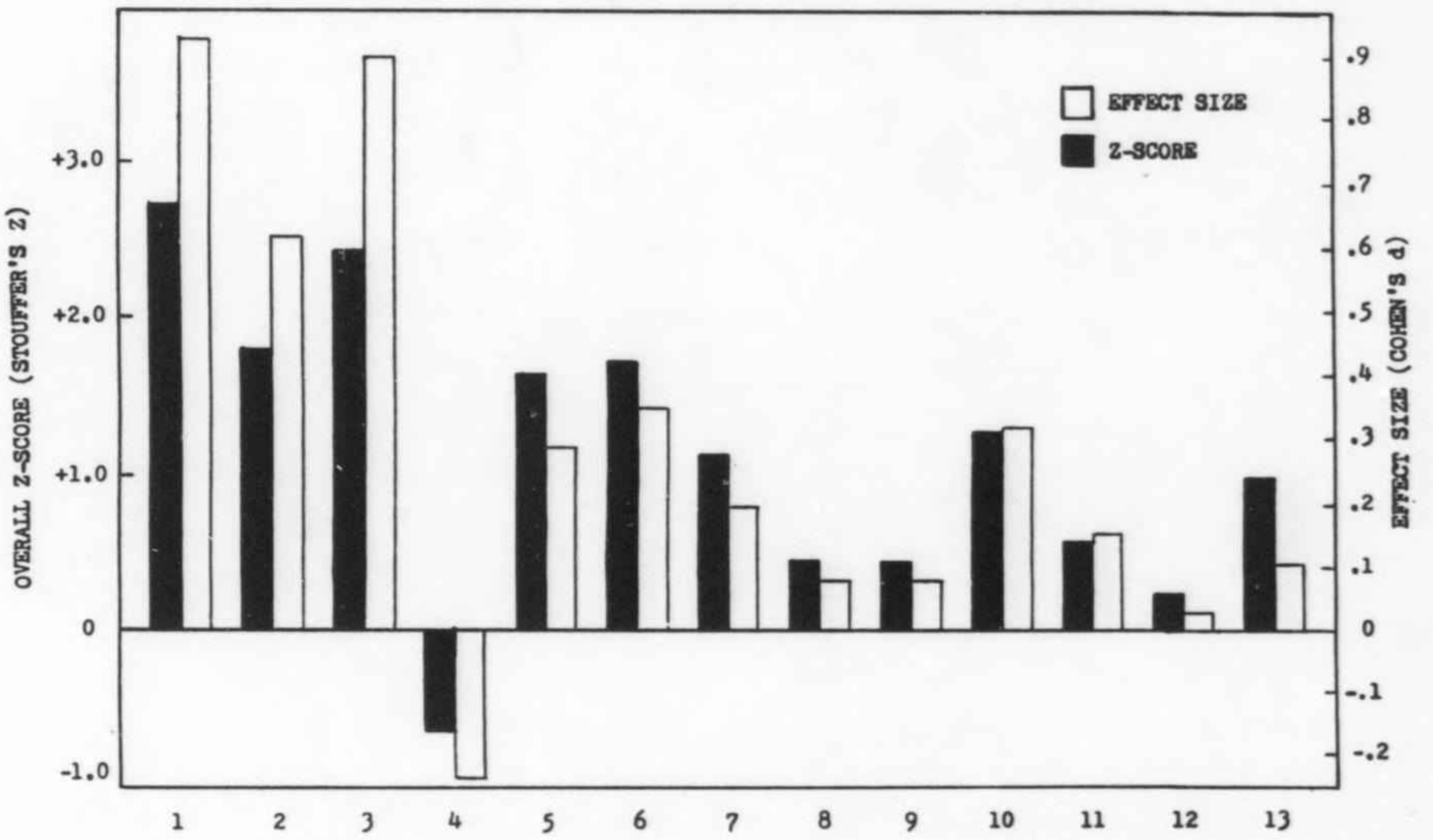

Fig. 3. Overall *z* scores and effect sizes (Cohen's d measures) for the 13 successive transpersonal imagery experiments.

were calculated according to the formula $d = t\sqrt{1/n}$. These effect sizes varied from −0.24 to 0.97, with a mean $d = 0.29$, and compare favorably with effect sizes typically found in traditional behavioral research.

A global analysis of the 13 experiments is presented in Table 2. There were 15 assessments of the transpersonal imagery effect. Contributing to those assessments were 323 sessions conducted with 271 different subjects, 62 influencers, and 4 experimenters. Six of the 15 assessments (40%) were independently significant statistically ($p < .05$); this is to be compared with the 5% experimental success rate expected by chance. Fifty-seven percent of the *sessions* were successful (i.e., these were sessions in which the influence imagery epochs accounted for more than 50% of the subject's electrodermal activity during activation attempts and less than 50% of the total activity during calming attempts); this is to be compared with the 50% session success rate to be expected on the basis of chance. The overall mean magnitude of the TIE for all experiments differed from chance expectation by 3.73%; when only the six independently significant experiments are considered, the obtained mean TIE had a magnitude of 8.33%. The two most important entries of Table 2 are the combined *z* score (for the experimental series as a whole, calculated according to the Stouffer method) and the mean effect size (Cohen's d, for the entire series). The overall *z* is 4.08 and has an associated $p = .000023$; the average effect size for all 13 experiments is 0.29.

Inspection of Table 1, Figure 3, and Table 2 indicates that the effect occurring in these 13 experiments is a relatively consistent, replicable, and robust one. It should also be pointed out that, in terms of its magnitude, the effect is not a negligible one. Under certain conditions, the transpersonal imagery effect can compare favorably with an imagery effect upon one's

TABLE 2
Summary statistics for transpersonal imagery experiments

Total Experiments	Psi Assessments	Number of Sessions	Number of Subjects	Number of Influencers
13	15	323	271	62

Successful Studies	Successful Sessions	Mean Percent Influence		Combined z	Overall p	Mean Effect Size (d)
		All	Successful			
6/15 (40%) (MCE = 5%)	183/321 (57%) (MCE = 50%)	3.73%	8.33%	4.08	.000023	.29

own physiological activity. Although it is not reviewed in this paper, an autonomic self-control experiment was conducted immediately following Experiment 5. In the self-control study, volunteer subjects attempted to calm themselves using relaxing imagery during 10 30-second periods, and their SRR activity during those periods was compared with activity levels during 10 interspersed nonimagery control periods. The strength of the self-control imagery effect in that study (an 18.67% deviation) did not differ significantly from the strongest transpersonal imagery effect of Experiment 5 (a 10% deviation).

Discussion

The results of this series of 13 experiments indicate that the present methodology is effective for the objective assessment of transpersonal imagery effects. It was demonstrated that the psychophysiological activity of one person varied, to a significant degree, with the imagery content of another person. The experimental design guaranteed that the effect could not be attributed to conventional sensorimotor cues, common external stimuli, common internal rhythms, or chance coincidence. A number of additional potential artifacts can be mentioned here and can be effectively dismissed.

1. *The findings are the result of recording errors and motivated misreadings of polygraph records.* This explanation is rejected on the basis of blind-scoring of polygraph records (see Figure 4 for sample record) and, later, by the use of completely automated assessment techniques and computer-scoring of response activity.

2. *The subjects knew beforehand when influence attempts were to be made and "cooperated" by changing their own autonomic activity when appropriate.* This explanation may be rejected because the subjects were not told when or how many influence attempts would be made, nor was the experimenter aware of the influence/control epoch schedule until all preliminary

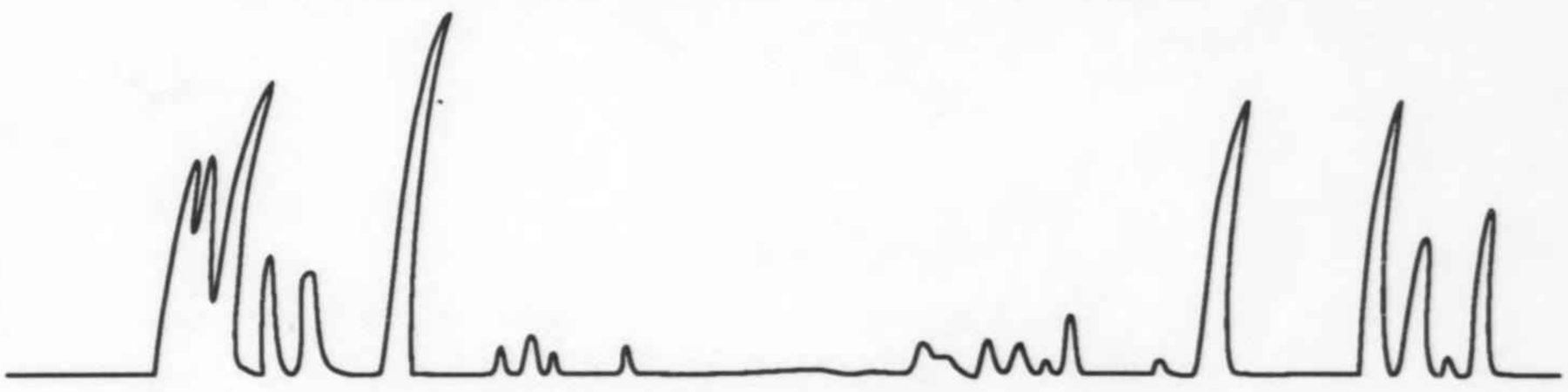

Fig. 4. Sample of polygraph tracing of electrodermal activity.

interactions with the subject had been completed. Subjects did not know of the existence of, or have access to, the envelopes containing schedule information.

3. Subjects could have become aware during the experimental sessions themselves of when influence epochs were in progress and could have altered their own physiological reactions during those periods. This possibility was eliminated by isolating the subject from any such cues from the influencer. Subject and influencer were in separate, closed rooms at least 20 meters apart. No auditory cues could have impinged upon the subject to indicate when recording epochs were in progress or whether such epochs were influence or control periods. Neither the influencer nor the experimenter made any vocalizations that could have informed the subject about whether influence or control periods were in progress. The epoch-indicating tones heard by the influencer and experimenter, and the random tones heard by the subject, were provided by independent audio systems which eliminated the possibility of electrical crosstalk and also the possibility of headphones functioning as microphones and inadvertently cueing the subject.

4. Differences in autonomic activity between influence and control periods are due to systematic error—i.e., some progressive change in electrodermal activity over time. This objection may be rejected. Progressive (time-based) errors could have been contributed by (a) changes in equipment sensitivity as the equipment warmed up, (b) changes in electrodermal activity due to adaptation or habituation to the experimental environment, or (c) changes in electrodermal activity due to polarization of the recording electrodes. Equipment was allowed to warm up for 15 to 20 minutes prior to the beginning of a session and therefore had become thermally stable before the experiment began. The use of electrodes with large surface areas, and the use of a constant-current electrodermal recording device reduced the possibility of polarization problems. The use of silver/silver chloride electrodes and partially conductive paste in other experiments further minimized a polarization problem. A special analysis of the data from Experiment 5 is relevant to the habituation question. Statistical evaluation of total electrodermal activity for the first halves *versus* the second halves of the sessions indicated no evidence of an habituation effect. This absence of habituation could be

attributed to the use of an adaptation period before the actual recording session began, and to the use of constantly changing auditory and visual stimulation of the subject (i.e., the use of the random tones and colored lights display). Thus, there was no progressive change in electrodermal activity due to any of the three possible processes mentioned above. However, even if a progressive change had occurred, the use of the ABBA counterbalanced design and the use of truly random influence/control sequencing in other experiments would have prevented this error from contributing differentially to influence *versus* control epochs.

5. *The findings are due to arbitrary selection of data.* This explanation may be rejected since total numbers of subjects and trials were prespecified, and the analyses reported include all recorded data.

6. *The results are due to fraud on the part of the subjects.* This explanation may be rejected. The subjects were unselected volunteers; it may be assumed that such subjects had no motive for trickery. However, even if a subject were motivated to cheat, such an opportunity was not present. Cheating would have required knowledge of a session's influence/control epoch sequence and of the precise starting time for the session, or the assistance of an accomplice. Both of these requirements were eliminated.

7. *The results are due to fraud on the part of the experimenters.* No experiment, however sophisticated, can ever be absolutely safe from experimenter fraud. Even if an experiment were controlled by an outside panel of disinterested persons, a hostile critic could still argue that collusion was involved. The imagined extent of such a conspiracy would be limited only by the imagination and degree of paranoia of the critic. We can only state that we used multiple-experimenter designs so that one experimenter's portion of the experiment served as a kind of control for another experimenter's portion. Only the successful replication of these findings by investigators in other laboratories would reduce experimenter fraud to a non-issue. We hope that this report will stimulate such replication attempts.

We conclude that our results cannot be attributed to any of the various potential artifacts or confounds mentioned above, and therefore are not spurious. Rather, the results reflect an anomalous psychophysical interaction between two individuals separated from one another in space.

A Range of Reactions

In addition to responding physiologically in a manner consistent with the imagery of the distant influencer, subjects often reported subjective responses which corresponded to the influencers' images. Sometimes these reports were of relatively vague feelings of relaxation or activation. However, there were also reports of extremely *specific* thoughts, feelings, and sensations which strikingly matched the imagery employed by the influencer. For example, a subject reported spontaneously that during the session he had a very vivid impression of the influencer coming into his

room, walking behind his chair, and vigorously shaking the chair; the impression was so strong that he found it difficult to believe that the event had not happened in reality. This session was one in which the influencer had employed just such an image in order to activate the subject from afar.

Subjects sometimes spontaneously reported mentation which corresponded closely to that of the influencer or the experimenter, even when that mentation was incidental and not employed consciously as part of an influence strategy. For example, at the beginning of one session, the experimenter remarked to an influencer that the electrodermal tracings of the subject were very precise and regimented and that they reminded him of the German techno-pop instrumental musical group, Kraftwerk. When the experimenter went to the subject's room at the end of the session, the subject's first comment was that early in the session, for some unknown reason, thoughts of the group Kraftwerk had come into her mind. The subject could not have overheard the experimenter's earlier comment to the influencer. Such correspondences were not rare.

There appeared to be a continuum of reactions possible for the subject. At one extreme, there would be no resemblance whatsoever between the imagery of the influencer and the imagery and physiological reactions of the subject. Next on the continuum would be cases in which autonomic reactions occurred which were appropriate to the influencer's imagery, but the subject was completely unaware of those reactions. Next were appropriate physiological reactions of which the subject was only vaguely aware, and next would be reactions accompanied by very definite subjective experiences. Closer to the "resemblance" end of this continuum would be cases of reactions accompanied by images in the subject which were virtually identical to those of the influencer. Even closer to the resemblance end would be cases of appropriate electrodermal activity, quite similar imagery, *plus* behavioral and/or gross physical changes consistent with the influencer's imagery. An example of the latter occurred in a subject who experienced a dramatic reddening of the face and neck during a session. Other subjects experienced muscle tremors, tingling of body parts, awareness of a pounding heart and rushing blood, a felt need to take deep breaths, decreased awareness of body parts, etc. Although our overall statistical findings leave no doubt that the subjects' recorded autonomic reactions were in fact related to the imagery of the influencers, such certainty is not possible in the case of these subjective or physical reactions, since no time-correlated records of those latter reactions were kept. Some of the physical symptoms observed may simply have been bodily conditions that were present all along, but which were brought to the subjects' awareness more forcefully during the experimental sessions due to the demand characteristics of the study. However, some reactions may have been directly influenced or even brought about by the influencers' imagery. We intend to pursue this issue more analytically in future studies in which the temporal distribution of such reactions will be monitored by having subjects verbalize their reactions as they are occurring, or indicate unusual or noteworthy feelings by pressing a

button that will mark an event channel of the polygraph. This will allow a determination of whether particular experiences or reactions of subjects are "time-locked" to specific images used by influencers during the sessions. A similar monitoring of the details of the influencers' imagery would permit a determination of the most and least effective forms of imagery, and could teach us a great deal about the varieties and manifestations of transpersonal imagery.

Some Preliminary Findings

We indicated earlier that we did not intend to describe specific details of the various 13 experiments in this paper. However, it does seem appropriate to mention some of our preliminary findings and tentative conclusions at this point.

1. The transpersonal imagery effect (TIE) is a relatively reliable and robust phenomenon; this conclusion is based upon overall statistical results.
2. The magnitude of the effect is not trivial, and under certain conditions it compares favorably with the magnitude of a self-regulation effect.
3. The ability to manifest the effect is apparently widely distributed in the population. Sensitivity to the effects appears to be normally distributed among the 271 volunteer subjects tested in these experiments. Altogether, 62 different influencers were able to produce the effect, with varying degrees of success. Many persons were able to produce the effect, including unselected volunteers attempting it for the first time. More practiced individuals seem able to produce the effect more consistently. There are indications of improvements with practice for some influencers.
4. The TIE can occur at a distance, typically 20 meters; greater distances have not yet been explored.
5. Subjects with a greater need to be influenced (i.e., those for whom the influence is more beneficial) seem more susceptible to the effect.
6. Immediate, trial-by-trial analog sensory feedback is not essential to the occurrence of the effect; intention/visualization of the desired outcome is effective.
7. The TIE can occur without the subject's knowledge that such an influence is being attempted.
8. It may be possible for the subject to block or prevent an unwanted influence upon his or her own physiological activity; psychological shielding strategies in which one visualizes protective surrounding shields, screens, or barriers may be effective.[2]
9. Generally, our volunteer participants have not evidenced concern over the idea of influencing or being influenced by another person.
10. The TIE may generalize to other physiological measures (such as heart rate), but the effect may also be intentionally focused or restricted to one of the number of physiological measures.[3]

11. The TIE does not always occur. The reasons for the absence of a significant effect in some experiments of a series which is otherwise successful are not clear. We suspect that the likelihood of a successful TIE may depend upon the presence of certain psychological conditions, in both influencer and subject (and perhaps even in the experimenter), which are not always present. Possible success-enhancing factors may include belief, confidence, positive expectation, and appropriate motivation. Possible success-hindering factors may include boredom, absence of spontaneity, poor mood of influencer or subject, poor interactions or poor rapport between influencer and subject, and excessive egocentric effort (excessive pressure or striving to succeed) on the part of participants. We suspect that the effect occurs most readily in subjects whose nervous systems are relatively "labile" (i.e., characterized by free variability) and are momentarily free from external and internal constraints. Perhaps fullness of intention and intensity or vividness of visualization in the influencer facilitate the effect.

Additional research is needed to determine the validity of these conclusions, and to explore more thoroughly the various physiological and psychological factors which are favorable or antagonistic to the occurrence of the TIE.

Implications and Applications

The methodology employed in these experiments reveals that, under certain conditions, mental imagery does indeed have a transpersonal aspect. The results suggest a fundamental inter-connectedness among people through which the transpersonal imagery effect may be mediated. The findings provide an additional illustration of the power of the imagination. The method extends research possibilities for the further laboratory study of imagery, transpersonal functioning, psychic functioning, emotional contagion, and other related processes.

If the effects of transpersonal imagery prove to be sufficiently strong and robust, it is not inconceivable that the phenomenon could be practically applied. Possible applications include the use of transpersonal imagery as an adjunct in medical and psychological healing practices; as an aid in therapy, counseling, and training for biofeedback, hypnosis, and meditation; and as an additional educational tool. Each of the processes just mentioned could conceivably be facilitated in one person (the learner or client) if appropriate and powerful images are held concurrently in the mind of another person (the teacher or therapist).

We hope this presentation of our methodology and preliminary findings will prompt other researchers and practitioners to conduct further experimental, theoretical, and applied investigations of the important but relatively ignored phenomenon of transpersonal imagery.

Endnotes

[1] The scorer measured, to the nearest millimeter, the amplitudes of all skin resistance responses greater than 2 mm. The amplitudes of all reactions during a 30-second epoch were summed, yielding a total SRR activity score for that period. This was done for each of the 20 30-second trial epochs. The trial sequence was then decoded and the SRR activity was summed for the 10 control and for the 10 influence periods. The scorer, of course, had been blind during the measurement phase.

[2] This tentative conclusion derives from certain segments of Experiment 7; the reader should consult Braud, Schlitz, Collins and Klitch (1985) for details.

[3] This tentative conclusion derives from certain segments of Experiment 8; see Braud, Schlitz, Collins and Klitch (1985) for further details.

[4] In Experiment 5, we studied the influence of the strength of "need" or motivation; for further details see Braud and Schlitz (1983).

[5] For additional information about Experiments 6, 7, and 8, see Braud, Schlitz, Collins and Klitch (1985).

[6] In Experiment 9, we sought to determine whether increments or decrements in SRR activity might be easier to produce via distant mental influence; in Experiment 10, we sought to determine whether the magnitude of a distant mental influence effect could be self-modulated by the influencer. Detailed results of these experiments will be published at a later date.

[7] The influencers for Experiment 11 were practitioners of a Reiki healing method; see Schlitz and Braud (1985) for details.

[8] Experiments 12 and 13 were conducted to examine the possible role of "intuitive data sorting" (IDS) in these experiments; details may be found in Braud and Schlitz (1988).

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Experiments Investigating the Influence of Intention on Random and Pseudorandom Events*

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Abstract—Eight of 27 experiments using a random event generator provided statistical evidence supporting a claimed correlation between intention and the distribution of random events. Twelve control tests produced results conforming closely to chance expectation.

Introduction

Over the last three decades, some 68 researchers have reported more than 800 experiments investigating the possibility that people may have an ability to influence simple random systems solely through the application of mental intention (Radin & Nelson, 1987; Radin & Nelson, in press). Overall, the results of these experiments provide evidence for the existence of a correlation between intention and the statistical behavior of electronic random event generators (REG). The claimed effects are relatively weak in absolute magnitude, and are evidenced by small shifts of various distribution parameters from chance expectation (usually the mean or the variance). In such experiments, participants attempt to influence sequences of random or pseudorandom events produced by electronic REGs by assigned or operationally defined mental intention. REGs are based upon truly random events such as radioactive decay or electronic noise, or use pseudorandom algorithms seeded with truly random numbers.

One of the first investigations of the possible influence of intention on radioactive decay rates was reported by Beloff and Evans (1961). They asked people to alternatively increase or decrease the count rate of a Geiger

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* This paper is a revised version of a technical report presented by the first author at a conference in 1981 (Radin, 1982). Prompted by Schmidt's (1987) reference to that report in this Journal, we decided to reanalyze and publish the data reported in the Radin (1982) paper, including several, previously unpublished experiments performed with the same random event generator.

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counter subjected to a source of alpha particles. The study was not statistically successful. Participants in several other studies in the 1960's had varying degrees of success in attempting to influence alpha, beta, and gamma particles (e.g., Chauvin & Genthon, 1965).

In the late 1960's, Helmut Schmidt developed a random event generator (REG) based upon the random waiting times between successive emission of beta particles from strontium 90 (e.g., Schmidt, 1970, 1971). Schmidt's REG generated random events by stopping a fast (one megahertz) binary counter when a Geiger tube detected a beta particle. Since radioactive sources theoretically emit particles at random time intervals, the probability that a binary switch would stop at a "1" would be the same as a "0."

The present paper reports a series of experiments conducted using an REG designed and constructed by Schmidt, and kindly loaned to the first author. In the studies described below, the random element in the REG was based upon the waiting times between successive emissions of gamma particles from radioactive ore (pitchblende). Volunteers were asked to concentrate on audio and/or visual feedback in a task that, if successful, would affect the statistical properties of the random events.

Method

Random Event Generator

The test machine is illustrated in Figures 1 and 2. The functions of the machine are controlled by a microprocessor (INTEL 8035) and several external switches and controls. Data reported in the studies below were automatically collected by the microprocessor and stored in a memory chip (INTEL 2716 PROM).[1] The automatic data recording method enabled the experimenter to double-check and allow independent verification of the data.

To prevent a participant from erasing the results of a poor run by turning the machine's power off during such a run, the microprocessor was programmed to increment the data storage counter at the beginning of each run, and record this number in the PROM. Thus, if the power was turned off, the data chip would record a zero score in that memory slot. Later, when the experimenter read the run scores off the data chip, the empty memory slots would be immediately apparent.[2]

The face of the machine shows 16 lamps arranged in a circle (Figure 1). When the microprocessor is reset, and the "PUSH" button is pressed, a program on a PROM chip starts running in the "direct" mode or "seed" mode, depending on how an external switch has been set.

Direct Mode. In the "direct" mode of generating random events, lamps starting at the top of the circle are sequentially illuminated in the clockwise (CW) direction at a rate of 4 Hz such that one light is lit at any one time. With each jump, a "hit" counter records the total number of CW steps.

When a Geiger tube detects a gamma particle, it sends a signal to the

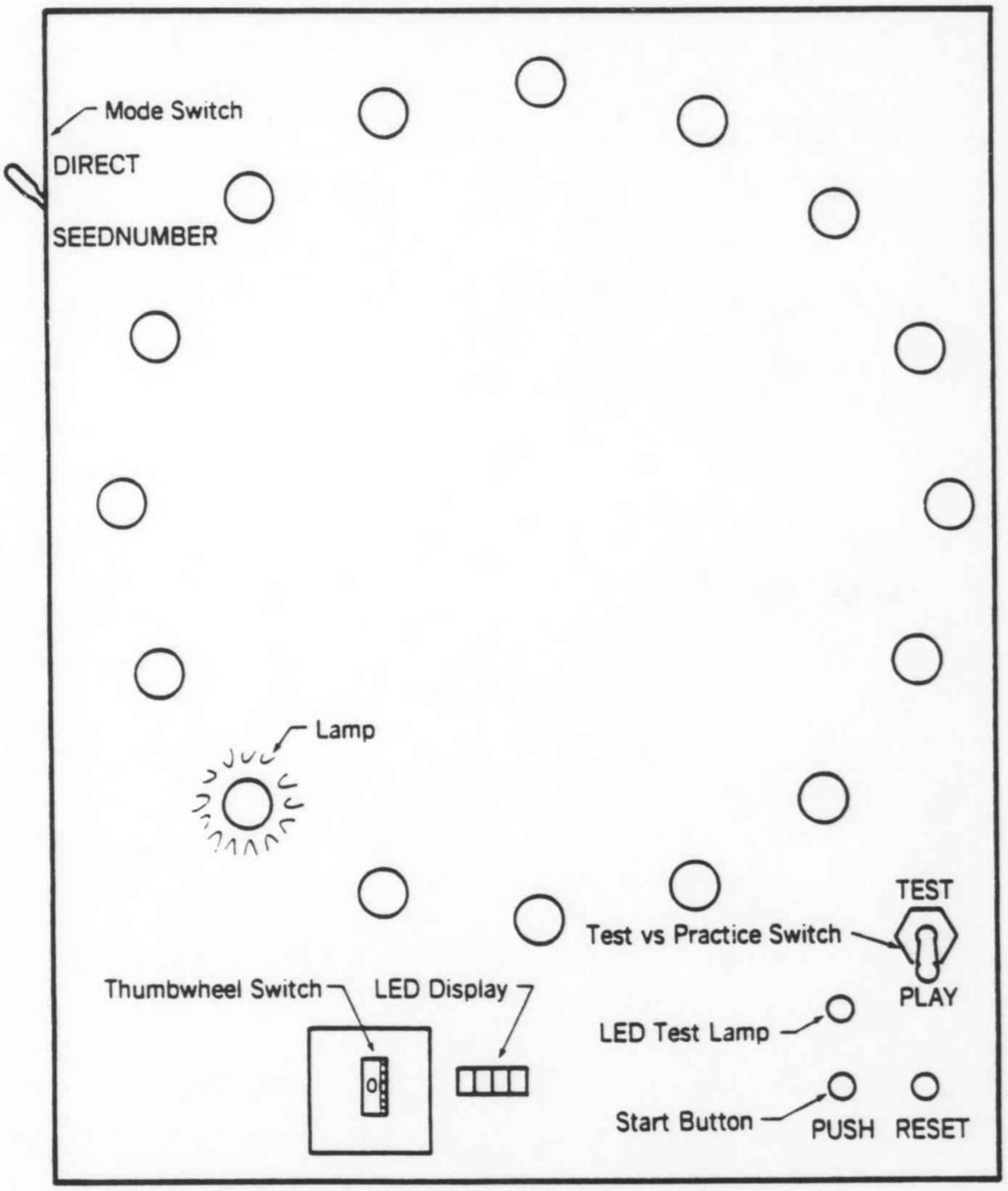

Fig. 1. Front view of random event generator.

microprocessor, which stops the CW pattern of the lights and begins a counter-clockwise (CCW) sequence. A "miss" counter records the number of CCW steps. When another gamma particle is detected, the lights start moving CW again, and so on.

This alternating CW-CCW illumination sequence is repeated until a pre-specified number (e.g., 16, 32, 64) of CW-CCW pairs have been completed. A thumbwheel switch on the face of the machine is used to specify the number of pairs. At the end of one run of say, 16 pairs, an LED display shows the total number of CW and CCW steps and the data is stored in the memory chip.

Seed Mode. In this mode, a 19-bit seed is generated by stopping a one megahertz counter when the Geiger tube detects a gamma particle. The

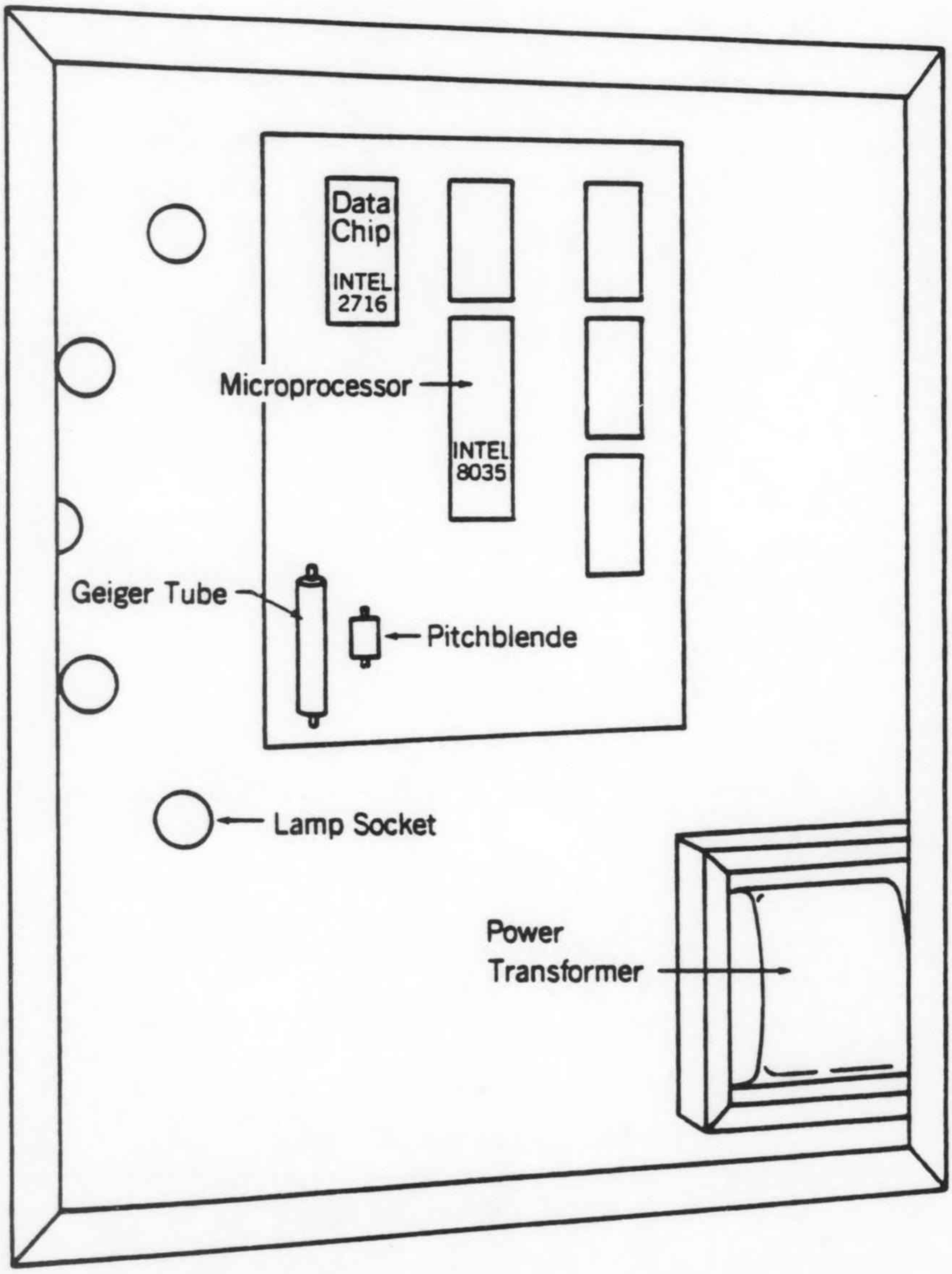

Fig. 2. Inside view of random event generator, viewed from the back.

microprocessor uses this 19-bit seed to initiate a sequence of 19-bit pseudorandom numbers such that the sequence contains every possible 19-bit number. This particular algorithm generates over a half million different numbers before repeating.[3] The four least significant bits of each pseudorandom number are used to determine one number in the range 1 to 16. During test runs, numbers are generated at a typical rate of 8 per second. With each generated random number (RN), a light jumps CW until RN = 3 has been obtained. Then the lights move CCW until a RN = 12 has been generated. This process continues until 16 CW-CCW pairs have been completed.

As in the direct method, the experimental participant tries to make the lights move CW more than CCW, thus extending the time periods of CW illumination and shortening the periods of CCW illumination.

Note that both methods have a built-in control feature—reversing the lamp illumination direction on successive "hits" (i.e., gamma particle in the direct mode and a specified pseudorandom number in the seed mode). For this control to be defeated, a long-waiting-time followed by short-waiting-time periodicity must be systematically present throughout all runs. Any such periodicities should become immediately apparent upon running control tests in which the REG is set up to run without human intervention. Studies 1, 3, 4, 5, 6, and 8 (described below) employed such control tests.

Feedback

In addition to the flashing lights, audio feedback was provided by a tone generator. Whenever the lights started moving CW, the tone generator produced a gong-like tone. This tone persisted until the lights started moving CCW, then no tone was generated. Audio feedback was provided through headphones connected to the test machine.

In all of the experiments reported here, rather than show CW-CCW motions, an optional feedback mode was employed, which was to show CW motion as usual, but no motion instead of CCW motion. This feedback was chosen because in pilot tests it was found to be less distracting than the constant motion provided in the CW-CCW feedback.

The subject's task, then, was to try to maintain the CW "hit" state for as long as possible and the stopped "miss" state for as short a time as possible. The visual task was to keep the lights moving, and the audio task, to keep the sound going.

Comparison of Direct and Seed Modes

In one respect, the seed mode is superior to the direct mode in generating random events because it relies upon a mathematical algorithm rather than a naturally occurring random event. Compared to a Geiger tube, for example, an algorithm is relatively insensitive to extraneous radiation and electrical disturbances. However, the deterministic nature of pseudorandom number generation makes interpretation of successful experiments somewhat more complex. Because an algorithm completely determines the results of a run once the starting seed has been selected, it would seem that any mental effort applied during a seed mode run would have to be focused "backwards" in time in order to influence the selection of a favorable seed. Although the existence of such a backwards effect defies common sense, mathematical models have been proposed that support such a focusing concept and predict the effect to operate backwards in time as efficiently as in present time (Schmidt, 1975, 1976, 1978).

Another way of interpreting positive results observed in the seed mode is

to postulate that precognition is the mediating factor. Precognition[4] would be used to select favorable, future moments in which to start a seed mode run. Under this interpretation, if a person could "see" the result of a future run before actually starting it, he or she would simply wait for a propitious time that would result in selection of a particular seed, which would in turn produce the desired result (e.g., more hits than misses). An analysis of existing RNG experiments provides some support for the precognition interpretation (May, Radin, Hubbard, Humphrey, & Utts, 1986).

A third interpretation is that positive effects are due to subtle strategies developed by the subjects, consciously or unconsciously, that somehow take advantage of inherent periodicities in pseudorandom number sequences. Such strategies, even if they were possible to develop and apply systematically, could not be used with the present REG because the selection of the seed number is based upon a truly random event, i.e., radioactive decay. And as we have mentioned above, once the seed is selected, the rest of the random generation process is completely determined. Thus, no normal strategy could be employed to influence results produced in the seed mode.

Hypothesis and Statistical Methods

The hypothesis for each experiment was simple: If intention can affect the distribution of random events, then the total number of CW steps, defined as hits in many of these experiments, would be greater than the total number of CCW steps. This hypothesis was tested with the formula[5]

$$Z = \frac{(H - M)\sqrt{2N}}{\sqrt{(H + M - 2N)(H + M)}},$$

where H = total number of CW counts, M = total number of CCW counts, N = total number of samples in the CW direction,[6] and Z is a standard normal deviate. Because a directional hypothesis was postulated, probabilities are reported one-tailed.

Results

Table 1 summarizes results of all experiments conducted by the first author using the same REG. They range from Study 1, conducted in November, 1980, to Study 12, conducted in December, 1983. Short descriptions of each test follow:

Study 1

The first experiment consisted of the first author (DR) as subject, running the REG for a preset total of 60 direct mode runs of 16 CW-CCW pairs per run, in three daily sessions of 20 runs each. This test was performed in the evenings, in a secure location, in a relaxed setting, and with no distractions. After each run the number of hits and misses was manually recorded.[7]

TABLE 1
Results of experimental and control tests

	Subject	Mode	Condition	Hits	Misses	Runs[a]	Z
Study 1	DR[b]	direct	E[c]	14896	13644	60	1.990†
	DR	direct	C	13135	13729	60	−1.005
Study 2	CY	direct	E	3820	3385	15	1.369
	JK	direct	E	3671	3769	15	−0.298
	MD	direct	E	3994	3398	15	1.827*
	RW	direct	E	3865	3538	15	1.001
	TD	direct	E	3471	3691	15	−0.697
	WL	direct	E	3466	3519	15	−0.172
	AK	direct	E	3623	3236	15	1.282
	HF	direct	E	3474	3446	15	0.092
	RK	direct	E	3550	3545	15	0.016
	SR	direct	E	3835	3546	15	0.887
Study 3	AK	direct	E	3413	3840	15	−1.335
	AK	direct	C	4057	3527	15	1.582
	CY	direct	E	3974	3230	15	2.342†
	CY	direct	C	3743	3652	15	0.279
	MD	direct	E	3563	3756	15	−0.598
	MD	direct	C	3559	4045	15	−1.447
	RW	direct	E	3093	3494	15	−1.385
	RW	direct	C	3685	3950	15	−0.786
	SR	direct	E	3688	3168	15	1.723*
	SR	direct	C	3727	4046	15	−0.928
Study 4	DR	direct	E	11963	10936	50	1.860*
	DR	direct	C	11926	11540	50	0.682
Study 5	DR	seed	E	12236	12780	50	−0.899
	DR	seed	C	12600	12176	50	0.708
Study 6	DR	seed	E	13251	12086	50	1.900*
	DR	seed	C	12223	12696	50	−0.785
Study 7	DR	seed	E	13361	12235	50	1.817*
	DR	seed	C	12611	12985	50	−0.604
Study 8	DR	seed	E	11844	13062	50	−2.022†
	DR	seed	C	12169	12737	50	−0.943
Study 9	DR	seed	E	26835	25018	100	2.046†
	DR	seed	C	25822	26031	100	−0.235
Study 10	RS	direct	E	2445	2197	10	0.990
	RS	seed	E	2319	2251	10	0.276
Study 11	BJ	seed	E	33520	33676	128	−0.153
	BJ	direct	E	30557	29539	128	1.123
Study 12	BN	direct	E	13326	13159	64	0.297

* Significant at $p < .05$, one-tailed.
† Significant at $p < .05$, two-tailed.
[a] There were 16 clockwise (and counter-clockwise) samples per run.
[b] DR is the first author. Excepting BN, all other subjects were unselected volunteers claiming no special abilities.
[c] E = experimental, C = control condition.

After completing the 60 test runs, a series of 60 control runs was performed in which the lights were covered and the headphones unplugged. The machine was located in the same place and under the same conditions as in the experimental condition. During control runs DR engaged in other tasks, pausing every so often to begin the next run. When each control run had completed, the number of hits and misses was manually recorded.

Study 2

In this experiment, DR recruited 10 volunteers from AT&T Bell Laboratories, Columbus, Ohio. Each participant performed 15 runs in the direct mode for a total of 150 runs pooled across subjects. The experimental runs took place in the Human Factors Laboratory at the Columbus Labs. Each subject was instructed in the use of the machine, and each was allowed to perform as many practice runs as he or she wished before beginning the formal data collection. Because Study 1 showed no systematic (first-order) bias in the control condition, no separate control condition was included in this study.

Study 3

This experiment consisted of rerunning the top five scorers from Study 2, where top scorers were defined as those persons obtaining the top five greatest excesses of hits over misses. This process was intended to select the more "talented" subjects from Study 2, even though it was recognized that those subjects could have obtained their higher scores by chance. Each individual performed 15 direct mode runs in the Human Factors Laboratory, for a total of 75 pooled runs. Immediately after each subject's 15-run session, DR ran a 15-run control session. During the control runs the face of the test machine was covered and the headphones unplugged, as in the first control study, so no feedback could be heard or seen.

Study 4

This test was an attempt to replicate the results of the first experiment. The experiment was performed by DR in the direct mode, in the same relaxed, undisturbed setting as in Study 1. This time, however, 5 sessions of 10 runs each were performed over 5 successive days, for a total of 50 runs in experimental and control conditions.

Study 5

In this study, DR ran the REG in the seed mode. The setting was quiet and undisturbed, and one or two sessions of 10 runs each were performed per day until 50 test runs were completed. Immediately after completing the 50 test runs, 50 control runs were performed. The test procedure was to reset the microprocessor, wait for a seed to be generated (this random waiting

time was usually between 1 and 5 seconds), then try to influence the REG to produce more clockwise motion than no motion. The control procedure was identical except that no directional intention was applied, the face of the test machine was covered, and the headphones unplugged.

Study 6

In this study, DR tested a possible time-displaced effect. Conditions of the test were the same as in Study 5, except that several seconds *after* the REG generated the seed for a run, a task was randomly selected by stopping a digital stopwatch and examining the hundreds-place digit. If the digit was even, the task was to try for as much clockwise motion as possible. If the digit was odd, the task was to try for as much "no motion" as possible. That is, what counted as success on each run was randomly selected between clockwise motion and no motion (recall that a feedback option allowed CCW motion to be reset into no motion).

It is important to realize that at the moment the seed was generated, DR did not know what would constitute success (i.e., CW motion or no motion). Thus, the momentary generation of the seed could not be "influenced" as might be the case in other studies reporting time-displaced effects (e.g., Schmidt, 1975, 1976). The distribution of randomly assigned tasks in the experimental and control conditions showed 27/50 even tasks (i.e., try for CW motion tasks) in the experimental condition and 23/50 even tasks in the control condition.

Study 7

This study was a replication of the previous study, except that the control condition was performed by generating 50 new tasks. In the experimental condition, 22/50 tasks were even (i.e., try for CW motion), and in the control condition, 24/50 were even.

Study 8

A few REG experiments have provided evidence that subjects may be able to influence REG statistics even when they are unaware of the task (e.g., Stanford, Zenhausern, Taylor, & Dwyer, 1975). This study investigated whether the effect observed in the previous two studies could be achieved when the task was unknown. The experimental procedure was similar to that in Studies 6 and 7, except that instead of looking at the random task before the feedback, DR looked at it *after* the feedback. In this way, tasks remained hidden during the test runs.

Study 9

Because Studies 6 and 7 were successful (see Table 1), but Study 8 was not (at least, not in the directional sense), an experiment was planned as a

replication of Studies 6 and 7. Preset at 100 runs, the experimental procedure was the same as in Studies 6 and 7, but the following method was used as a control: The sum of all CW counts was taken as the control score for hits, and the sum of all CCW counts was taken as the control for misses. Recall that the definition of "hits" and "misses" in this study, and in Studies 6 and 7, depended on the assigned directional *task* for each run. These tasks (aim for CW or CCW counts) were generated *after* the seed numbers were selected, but before the feedback was presented. Thus, experimental hits and misses consisted of combinations of CW and CCW counts, depending on the task for each successive run.

It is important to note that the directional tasks generated from one run to the next (by the method described in Study 6, above) were recorded manually. While it is unlikely that systematic recording biases could have influenced the data given that the task was generated *before* the results of a run were known, manual recordings are less certain than automatic recordings, and thus results of Studies 6–9 should be considered as tentative only.

Study 10

Subject RS, a surgeon highly skeptical of psi phenomena, contributed 10 runs in both direct and seed modes, in the first author's presence.

Study 11

Subject BJ, a homemaker, claimed no special abilities but was interested in participating in the experiment. She was allowed to keep the REG for one month at home, and was instructed to perform 128 runs in both the seed and direct modes. Upon reading out the scores on the PROM chip, no instances of data selection or turning off the REG's power during a run were detected.

Study 12

Subject BN, a homemaker, claimed a variety of psychic abilities. She was allowed to keep the REG for one month, at her home, and was instructed to perform 64 runs in the direct mode. No attempts at data selection were detected upon reading out the scores on the PROM chip.

Discussion

Figure 3 shows the same information as Table 1, but in the form of cumulative deviation curves for experimental and control studies. Figure 4 shows these curves separated by direct or seed mode of operation.

Figure 3 reveals that the experimental condition produced a significant deviation from chance (terminal $Z = 2.941$, $p = .002$) and that the control condition remained within the chance expectation envelope (terminal Z

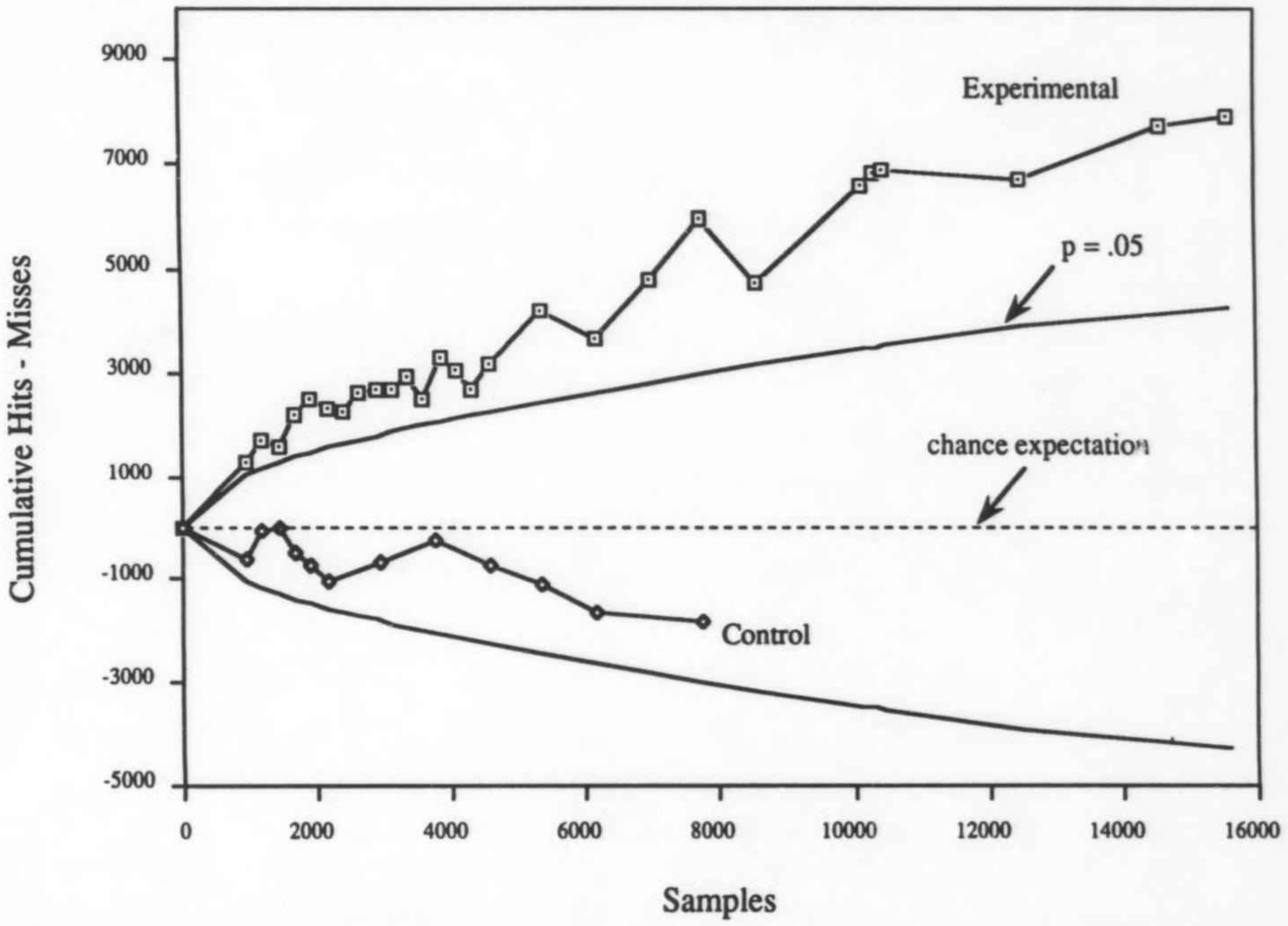

Fig. 3. Cumulative deviation of hits–misses from chance expectation (as defined by the assigned directional task) for all experimental and control tests. The parabola shows the $p = .05$ level.

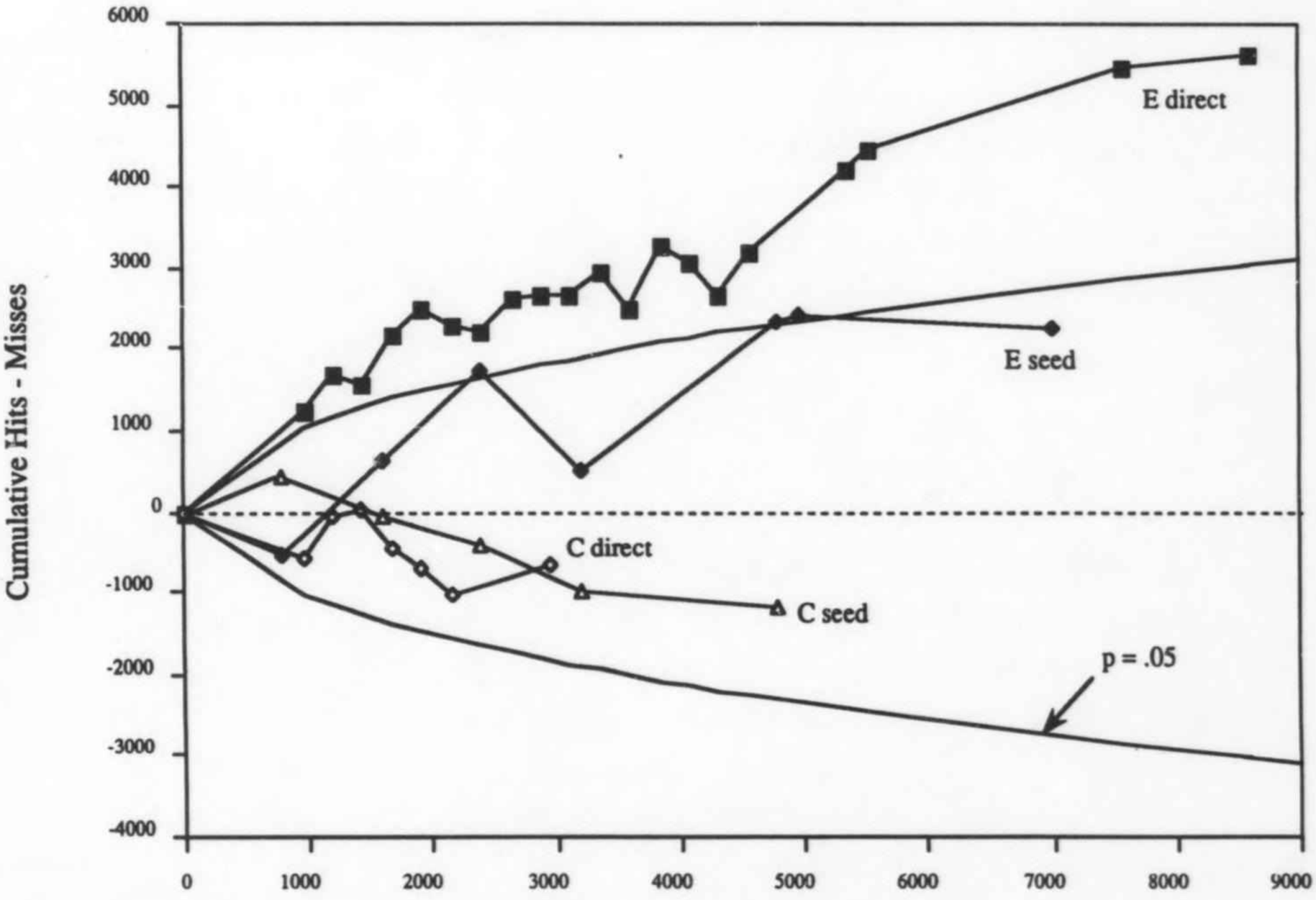

Fig. 4. Cumulative deviation of hits–misses from chance expectation, separated according to experimental condition and mode. "E" indicates the experimental condition and "C" the control condition. The parabola shows the $p = .05$ level.

$= -1.005$, $p = .843$). Figure 4 indicates that the primary significance was obtained in the direct mode (terminal $Z = 2.754$), but the seed mode also contributed to the positive trend (terminal $Z = 1.121$).

In Table 1 we see that 8 of 27 experiments and 0 of 12 control tests were significant at $p < .05$, one-tailed. This corresponds to $p = 3.67 \times 10^{-5}$, one-tailed, and $p = .540$, respectively.[8] Because the first author, DR, was subject in 7 of the 12 Studies, it is instructive to examine the results when other individuals participated as subjects. From Table 1 it can be seen that 3 of 20 experimental tests not involving DR as subject produced results with $p \leq .05$ ($p = .076$), 5 of 20 tests resulted in $p \leq .10$ ($p = .043$), 7 of 20 resulted in $p \leq .15$ ($p = .022$), and 9 of 20 resulted in $p \leq .20$ ($p = .0099$). This suggests that the overall level of significance is not only due to DR's contributions.

Conclusion

Eight of 27 experiments using a random event generator constructed by Helmut Schmidt, and independently tested by the first author, confirmed a claimed correlation between intention and the statistical distribution of random events. Twelve control tests were non-significant.

In isolation, the anomaly observed in this experiment would be interesting, but not particularly persuasive. This is because there are, at present, no compelling theoretical reasons to predict the existence of such an effect. However, in spite of prevailing theory, three independent reviews of experiments using REGs have agreed that the aggregate evidence for this effect is exceptionally persuasive (Honorton, 1978; May, Humphrey, & Hubbard, 1980; Radin & Nelson, 1987; Radin & Nelson, in press). The anomaly has resisted repeated efforts to "explain-away" the evidence as being due solely to methodological artifact, statistical problems, or experimenter or subject fraud.

Numerous theorists have proposed mathematical, physical, and psychological models to explain how such effects might be possible (e.g., Bastin, 1977; Costa de Beauregard, 1979; Jahn & Dunne, 1986; Schmidt, 1975; Walker, 1974). These models attempt to provide world views which encompass concepts such as acausality and time-displacement. Some of these efforts have been inspired by interpretations of quantum mechanics which suggest that objects in the world may not be completely independent of consciousness or observation (e.g., d'Espagnat, 1979; Hall, Kim, McElroy, & Shimony, 1977; Mermin, 1985; Shimony, 1963; Squires, 1987, in press; Trefil, 1987; Wigner, 1963). Some theorists argue that rather than being paradoxical or contrary to theoretical expectation, some form of mental influence on physical objects should in fact be expected. We close with the intriguing thoughts of the physicist, Costa de Beauregard (1979, p. 186):

> My thesis is that [these phenomena] are postulated by the very symmetries of the mathematical formalism [of quantum theory] and should be predicted for reasons

completely akin to those that led Einstein to enunciate the principle of special relativity, de Broglie to produce the concept of matter waves, and Dirac to (almost) predict the positron.

Endnotes

[1] With one exception, as noted below.

[2] No cases of attempted data selection were detected in these experiments.

[3] The algorithm is $r(n + 1) = [B \times r(n)][\text{mod } p]$, where $p = 2^{19} - 1$ and $B = 243^6$. See Hardy and Wright (1945) or Radin (1985) for further details on generation of pseudorandom number sequences.

[4] Precognition is defined here as non-inferential prediction or perception of future events.

[5] See the Appendix for a derivation of this formula.

[6] There were 16 CW and 16 CCW samples per run, thus N = total number of runs$*$16.

[7] A hardware problem disabled the fully automatic recording mode in this study, thus the recorded numbers of hits and misses in Study 1 are not as dependable as those in succeeding experiments. All other studies were recorded both manually and automatically on the PROM. No discrepancies were detected when manual and automatic recordings were compared.

[8] Exact binomial probabilities.

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Appendix

We assume there is a fixed probability that the random process will cause the REG to reverse the lamps' direction while any specific lamp is "moving" in the clockwise (CW) or counter-clockwise (CCW) direction. If we call the CW probability P_H and the CCW probability P_M, then the null hypothesis of interest is $P_H = P_M$.

Let X_i and Y_i be the number of lamps lit in the CW and CCW directions, respectively, for sample i, where $i = 1, \ldots, N$. Then $P(X_i = 1) = P_H$, $P(X_i = 2) = (1 - P_H)P_H$, $P(X_i = 3) = (1 - P_H)^2P_H$, and in general, $P(X_i = x) = (1$

$- P_H)^{x-1}P_H$. This is known as a geometric distribution with parameter P_H. Similarly, Y_i follows a geometric distribution with parameter P_M.

Let $H = \sum_{i=1}^{N} X_i$, and $M = \sum_{i=1}^{N} Y_i$, where N is the total number of samples in the experiment. (In the current experiment, one "run," initiated with a single button press, produced 16 CW and CCW samples.) Since H and M are each sums of independent geometric random variables, they have *negative binomial distributions* with parameters (N, P_H) and (N, P_M), respectively. Thus, $E(H) = N/P_H$ and $\text{Var}(H) = N(1 - P_H)/P_H^2$. Similar results hold for M.

Instead of testing the null hypothesis, $H_0{:}P_H = P_M$, directly, we test the equivalent but intuitively more appealing hypothesis, $H_0{:}E(H) = E(M)$. Relying on the Central Limit Theorem, we use the standardized version of $H - M$ as our test statistic.

When H_0 is true, $E(H - M) = 0$ and $\text{Var}(H - M) = \text{Var}(H) + \text{Var}(M) = 2N[(1 - p)/p^2]$, where $p = P_H = P_M$. To estimate $\text{Var}(H - M)$, substitute $\hat{p} = 2N/(H + M)$ for p. Thus, the test statistic, which should be compared to the standard normal table, is:

$$Z = \frac{(H - M)}{\sqrt{\dfrac{2N(H + M - 2N)(H + M)^2}{(H + M)(2N)^2}}} = \frac{(H - M)\sqrt{2N}}{\sqrt{(H + M - 2N)(H + M)}}.$$

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A Case of the Possession Type in India With Evidence of Paranormal Knowledge

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Abstract—A young married woman, Sumitra, in a village of northern India, apparently died and then revived. After a period of confusion she stated that she was one Shiva who had been murdered in another village. She gave enough details to permit verification of her statements, which corresponded to facts in the life of another young married woman called Shiva. Shiva had lived in a place about 100 km away, and she had died violently there—either by suicide or murder—about two months before Sumitra's apparent death and revival. Subsequently, Sumitra recognized 23 persons (in person or in photographs) known to Shiva. She also showed in several respects new behavior that accorded with Shiva's personality and attainments. For example, Shiva's family were Brahmins (high caste), whereas Sumitra's were Thakurs (second caste); after the change in her personality Sumitra showed Brahmin habits that were strange in her family. Extensive interviews with 53 informants satisfied the investigators that the families concerned had been, as they claimed, completely unknown to each other before the case developed and that Sumitra had had no normal knowledge of the people and events in Shiva's life. The authors conclude that the subject demonstrated knowledge of another person's life obtained paranormally.

K. S. Rawat accompanied us on one of the field trips of this investigation and participated in some of the interpreting. Satwant Pasricha wishes to thank the National Institute of Mental Health and Neurosciences for support. Emily Williams Cook read a draft of this paper critically and helped us to clarify and amplify our report of some details of the case. Thanks are also due to Susan Adams for assistance in the preparation of this paper.

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The word *possession* designates a wide variety of conditions that psychiatrists, psychologists, and anthropologists describe. It indicates that a person has undergone such a marked change of behavior that other persons seem no longer to be in contact with the ordinary personality of the affected person.

Possession states occur widely in India (Carstairs & Kapur, 1976; Teja, Khanna, & Subrahmanyam, 1970).[1] Most psychiatrists, psychologists, and anthropologists have emphasized the similarities between cases of the possession type and diagnostic entities recognized in the West, such as multiple personality and hysteria. Accordingly, they tend to use phrases such as "possession syndrome" and "hysterical possession." They also, in varying degrees, offer motivational explanations of the condition that depict it as beneficial to the affected person in improving his status and perhaps resolving internal and external conflicts. However, Claus (1979) cautioned against psychological and sociological interpretations for *all* cases until we have more information. Along the same line, Lewis (1971, pp. 178–179) wrote: "Nothing after all is easier than leaping to conclusions and projecting our own psychological assumptions and interpretations onto exotic evidence which may correspond only in superficial detail with apparently similar data in our own culture."

The question arises of whether some ostensibly possessed persons show knowledge about the life of a deceased person that they could not have obtained normally. We think that in a small number of cases the subjects do show such knowledge. Cases of this kind are rare, and yet sufficiently well known in India so that the Hindi word *parakayapravesh* ("entering into another body") exists for designating them. One of us (I.S.) has studied several cases of the type with evidence of paranormal knowledge[2] and has published reports of two of them (Stevenson, 1966/1974a, 1983a). Their occurrence and that of occasional other cases of ostensible possession with evidence of paranormal processes, such as the case of Uttara Huddar (in which the subject spoke a language she had not learned and assumed the personality of a deceased woman completely unknown to her family) (Stevenson & Pasricha, 1979; Stevenson, 1984), have encouraged us to search for new cases of the possession type with evidence of paranormal processes.

Such evidence is not easily obtained. The ostensibly possessing personalities (when not gods or godlings) are usually persons known to the subject or about whom the subject may easily have learned normally. In cases of this type it is difficult to obtain satisfactory evidence of the subject's having knowledge paranormally acquired. We believe we have satisfied this criterion in the case we now report. It involves two completely unrelated and unacquainted persons. Their families lived in widely separated towns and villages, and the informants' testimony warrants believing that they had had no contact with each other before the case developed.

Case Report

Summary of the Case and its Investigation

When this case developed, the subject, Sumitra Singh, was a young married woman of about 17. She was living with her husband and their one child in her husband's family home (according to the custom in India) in the village of Sharifpura, in the Farrukhabad District of the State of Uttar Pradesh, India. Early in 1985 she began to develop episodes of loss of consciousness with eye-roll movements and clenching of the teeth. Sometimes she would speak during these trance-like states, and one day in July she predicted that she would die three days later. When the predicted day (July 19) came, she seemed to die. At least members of her family and other villagers considered her dead, because she was pulseless and apneic, and her face was drained of blood like that of a dead person. They had begun grieving and also preparing for Sumitra's funeral, when she unexpectedly revived.

Following a brief period of confusion Sumitra began to behave like a different person. She did not recognize the people around her and said that her name was Shiva and that she had been murdered by her in-laws at a place called Dibiyapur. She rejected Sumitra's husband and child and asked to be taken to Shiva's two children. She stated many details that were subsequently found to correspond with the life of another young married woman, Shiva Diwedi, who had died violently—whether from murder or suicide is still unclear—at Dibiyapur on the night of May 18–19, 1985, that is, two months before Sumitra's apparent death and revival. Shiva's parental family believed that her in-laws had murdered her and then attempted to simulate suicide by laying her body on railway tracks nearby. Her father, Ram Siya Tripathi, filed a complaint, and this instituted a judicial inquiry. Reports of Shiva's death and of the legal proceedings appeared in newspapers published in Etawah, the district town where Ram Siya Tripathi lived.

Sumitra's in-laws said that they knew nothing of a Shiva who had died at a place called Dibiyapur. At first they thought that Sumitra had gone mad and later that she had become possessed by a discarnate spirit; but they made no attempt to verify what she was saying. It was about a month before Ram Siya Tripathi learned about Sumitra's statements. This occurred, almost accidentally, when he heard a rumor, while he was visiting Dibiyapur, that his deceased daughter had taken possession of a girl in a distant village. Nearly two more months elapsed before he was able to verify this information by having someone from a village called Murra, which is close to Sharifpura, visit Sumitra and her family.

The information gathered corresponded to facts in the life of Ram Siya's deceased daughter, Shiva, and so on October 20, 1985 Ram Siya went

himself (accompanied by a relative) to Sharifpura, where Sumitra recognized him and said she was his daughter. Sumitra also recognized in Sharifpura and Etawah (where she visited Ram Siya during the following days) at least 13 members of Shiva's family and circle of friends.

In addition to Sumitra's statements about the life of Shiva and her recognitions of persons Shiva had known, she showed a marked change in behavior. Sumitra's family belonged to the Thakur caste and they were villagers with almost no education; Sumitra herself had had no formal education, although she could read and write a little. The Tripathis, on the other hand, were Brahmins and middle-class urbanites. Ram Siya was a lecturer in a college, and Shiva had been educated up to the level of earning a B.A. degree. After her revival, Sumitra's behavior changed from that of a simple village girl to that of a moderately well-educated woman of higher caste and more urban manners, who could now read and write Hindi fluently.

The case came to our attention soon after the first exchanges of visits between the families concerned. The *Indian Express* published a report of the case on October 26, 1985. One of I.S.'s correspondents in India noticed this and sent a copy of the report to him. At about the same time a correspondent in northern India sent to S.P. a copy of a report of the case in a Hindi newspaper, *Dainik Jagran,* that had appeared on October 23, 1985.

Methods of Investigation

Our principal method of investigation was interviews with informants, particularly firsthand witnesses of the apparent death of Sumitra and the change in her personality that followed her revival; but we spent almost as much time interviewing the members of Shiva's family.

We were able to begin our interviews within three weeks of learning about the case. In November 1985, S.P. conducted a series of interviews with some of the principal informants for it. These included Sumitra and her mother-in-law, Shiva's parents, and one of Shiva's maternal uncles.

In February and March 1986, we worked together on the case for seven days. We interviewed again all but one of the persons S.P. had interviewed earlier. In addition, we interviewed numerous other informants for the case, especially in Sumitra's village of Sharifpura. We met her father in his village, Angad ka Nagla. We had to seek out other informants in four other towns and villages of the Farrukhabad District and the neighboring districts of Etawah, Mainpuri, and Hardoi.

In November 1986, February 1987, and October 1987 two of us (I.S. and S.P.) spent another 10 days on fieldwork for the case. During these three periods we interviewed (in Dibiyapur) Shiva's husband and father-in-law, whom we had not met earlier. We also interviewed informants who had connections through marriage or trade with more than one of the communities involved in the case; we intended these interviews to help us to assess the

likelihood that information about Shiva's life and death could have reached Sumitra's family along normal lines of communication. We also, during this later phase of the investigation, had new interviews with some previous informants, including Sumitra and her husband and Shiva's parents.

By the end of our investigation in October 1987 we had interviewed 24 members of Sumitra's and Shiva's families, and we had interviewed all the more important witnesses among these persons two or more times. In addition, we had interviewed another 29 persons who were able to furnish background information, especially that mentioned above concerning communications between the communities involved in the case.

During the interviews S.P. made notes, mostly in Hindi, and also acted as principal interpreter for I.S. and N.McC-R., both of whom made notes in English, as nearly verbatim as possible. During a few special interviews we made tape recordings only, or in addition to making notes.

In our interviews we gave particular attention to the following aspects of the case: the preceding illness, apparent death, and revival of Sumitra; the possibilities for normal communication of information about Shiva's life and death to Sumitra and her family; and the circumstances under which Sumitra, after her revival, identified Shiva's family in person and in photographs.

In addition to our interviews we obtained copies of newspaper reports (published in Etawah) of Shiva's death and of Ram Siya Tripathi's allegations that her in-laws had murdered her. We also obtained copies of the photographs of Shiva's family in which Sumitra had correctly identified persons normally unknown to her before the change in her personality.

One of us (I.S.) has published elsewhere further details of the methods followed (Stevenson, 1966/1974a, 1975).

Relevant Facts of Geography and Possibilities for Normal Communication Between the Families Concerned

This case occurred in the Farrukhabad and Etawah Districts of the State of Uttar Pradesh. Etawah is a small city of about 100,000 inhabitants located approximately 500 km southeast of Delhi. It is a district town on the main line of the railway that joins Delhi to Kanpur (about 250 km east of Etawah) and, ultimately, to Calcutta. Shiva's parents lived in Etawah from the time she was three years old until the case developed.

After her marriage Shiva moved to the home of her in-laws at Dibiyapur, which is about 55 km east of Etawah. Almost adjoining Dibiyapur is the railway station of Phaphoond, which is on the main railway line linking Etawah and Kanpur.

Sharifpura, the village where Sumitra was living when the case developed, is just outside the Etawah District in the Farrukhabad District, about 65 km north of Etawah and about 100 km (in a direct line) from Dibiyapur. Some

of Sumitra's in-laws lived in the town of Sikandarpur, which is about 30 km farther north and west from Sharifpura, in the Farrukhabad District.

Angad ka Nagla, where Sumitra's father lived, is about 15 km east of Sharifpura and perhaps slightly south.

Informants for Sumitra's side of the case said that they had no previous acquaintance with Shiva's family, and members of Shiva's family similarly said they were completely ignorant of Sumitra's family before the case developed. Apart from the long (for India) geographical distances between the families, they were further separated by significant differences of caste, education, and economic position.

Strong support for the informants' denial of prior acquaintance (or knowledge about each other) comes from the slow and roundabout manner in which Shiva's family learned about the personality change in Sumitra. Sumitra's father and her in-laws made no attempt to verify her statements about Shiva. Word about them first reached the neighboring village of Murra, which is 2 km from Sharifpura. From there it traveled to Dibiyapur apparently conveyed by women of Murra who had married and were living there. Ram Siya Tripathi, on a visit to Dibiyapur, heard a rumor that his dead daughter had taken possession of a girl in a distant village called Sharifpura. However, he had never been to Sharifpura and did not even know where it was located. After another two weeks he learned about a man called Ram Prakash Dube, a native of Murra who was living in Etawah, but whom he had not previously known. He asked Ram Prakash Dube to inquire in Murra about the truth of the account he had heard in Dibiyapur. The monsoon rains led to further delays. When Ram Prakash Dube next visited Murra, he looked into the story and confirmed its main outlines to Ram Siya Tripathi, who, as we have mentioned, then went to Sharifpura and had his first meeting with Sumitra on October 20, 1985. This was exactly three months after Sumitra's apparent death and revival. We believe that if the families concerned had been previously acquainted or had had any lines of communication through mutual acquaintances, they would have exchanged information about Sumitra's change of personality much sooner than they did.

If Ram Siya Tripathi had not complained to the police about the unseemly haste with which his daughter's body had been cremated (as we shall describe below), few persons outside those immediately concerned would have heard about her death. However, when the police began to investigate the matter, the newspapers of Etawah took notice and published accounts of Shiva's death and of the judicial inquiry. Some of the newspapers carrying these reports reached Sikandarpur, where persons who might have come in contact with the family of Sumitra's mother-in-law read them. At least one newspaper with a report also reached a reader of Sharifpura. The brother of the headman of Sharifpura said that he had read about Shiva's death in a newspaper before the change in Sumitra; but he gave the matter little attention at the time. The headman himself, a schoolteacher, said that he learned

about Shiva's death only after Sumitra's changeover. In addition, we learned of two traders who went regularly between Sharifpura and Etawah on business. After the case developed one of them became acquainted with Ram Siya Tripathi, but had not known him earlier.

Members of Sumitra's family said that they had heard nothing about Shiva's death before Sumitra's death, revival, and personality change. However, in view of the circulation of some newspapers in their area, and of some trading between Sharifpura and Etawah, it is best to assume that they might have learned of Shiva's death and perhaps also learned about some of the allegations of suicide and homicide that figured in the newspaper reports. (There was no radio station in the area. Some television had been introduced at Etawah only [Sharifpura had no electricity], but it only relayed programs from Delhi and broadcast no local news.) The newspaper reports included some of the names of Shiva's parental family and in-laws. The important question remaining is, therefore, whether Sumitra, after the change in her personality, demonstrated knowledge and behavior corresponding to Shiva's life that went beyond the information available in the newspapers reporting the death of Shiva and the related judicial inquiry.

The Life, Last Illness, Apparent Death, and Revival of Sumitra

Sumitra was born in (probably) 1968[3] in Ahmedabad, Gujarat, where her father, Chotte Singh, had gone for employment in one of the many textile mills there. He was a native of Angad ka Nagla, a village of the Etawah District. He and other members of his family belonged to the Thakur caste.

Sumitra's early life was unusual in the degree to which she lived separated from one or both of her parents. The separations reflected her father's efforts to break out of the life of a village cultivator and become a steady wage-earner.

Because of her parents' movements Sumitra lived for about eight years with an older cousin, Phool Mala, in the village of Birpur, in the neighboring district of Mainpuri. Sumitra never attended school, but Phool Mala taught her the elements of reading and writing. Phool Mala herself had gone to a primary school only for a year or two, and she had mainly learned to read and write at home. She taught Sumitra as much as she knew. She said that Sumitra could read the *Ramayana* and was able to write a letter. Sumitra's father told us (incorrectly) that she could not write at all, and her husband said that she could write "a very little like a child in kindergarten." He thought that she was (when he knew her) unable to write a letter, but we learned that she had occasionally done this.

Sumitra's mother, Ganga Devi, died in 1979, when Sumitra was about 11.

In childhood Sumitra enjoyed good physical health. When she was about 13 (in 1981), she was married (in the Indian style of arranged marriages) to Jagdish Singh and moved to her husband's village of Sharifpura, which is about 15 km from Angad ka Nagla, where her father was then living. Sumi-

tra's marriage to a considerable extent repeated the pattern of separations that she had experienced in childhood, because her husband, like her father, went to a city (in his case, Delhi) trying to obtain regular employment, and he was often away from Sharifpura for months at a time. After three years of marriage, Sumitra gave birth to a baby boy in December, 1984. A month or two later, early in 1985, she began to suffer from periods of loss of consciousness or trance in which her eyes would roll upwards and she would clench her teeth. She seemed not to have fallen suddenly in any of these episodes. The spells lasted varying times—from a few minutes to a whole day. Sometimes Sumitra would say afterward that Santoshi Ma[4] had possessed her. On two occasions she was seemingly possessed briefly by discarnate personalities. One of these communicating personalities said that she had been a woman of Sharifpura who had drowned herself in a well; the other (a male) said that he had been a man of another state in India. She gave some particulars about this latter life that have not been verified and are probably unverifiable.

During these episodes of apparent possession, Sumitra's condition sufficiently troubled her family so that they consulted local healers. Of these the most prominent was a man called Vishwa Nath. He was a distant relative of the family, a cultivator regularly, and without experience in other cases like Sumitra's. Nevertheless, he seemed to have a pacifying influence on her. He himself at times went into trances when he would be possessed, as he would say afterward, by the Hindu god Hanuman.

Vishwa Nath's intervention did not arrest Sumitra's episodes of trance; and, as we described earlier, she predicted her death, and three days later (on July 19, 1985) she lost consciousness and seemed to die. We questioned several eyewitnesses of this event. Her respiration and pulse stopped and her face became drained of blood like that of a dead person. A considerable group of persons surrounding Sumitra were convinced that she had died, and some began to cry. It was proposed to put her body on the ground (a Hindu ritual performed for persons who are thought about to die or who have died). Sumitra seemed to be dead for a period estimated by her father-in-law and brother-in-law as about five minutes. Some other informants thought that she had been dead for much longer than five minutes before she revived, but we think they may have estimated the onset of death from a time when her breathing became shallow and barely perceptible. No doctor was in or near the village, so Sumitra's heart was not auscultated, and we have reported the villagers' judgment that Sumitra was dead without asserting ourselves that she was.

When Sumitra revived she did not recognize her surroundings including the people of her husband's family. She said little or nothing for a day after her revival. Then she began to say that she was Shiva and to describe details of the life and death of Shiva. We shall summarize her statements about Shiva's life in a later section of this report, after we describe what we could learn about Shiva's death.

Upon hearing Sumitra's statements about Shiva's life and death, her in-laws thought first that she had gone mad and then that she had become possessed by a wandering discarnate personality who could be exorcised away or might leave spontaneously as had the ones previously manifesting in Sumitra.

In the autumn of 1986 Sumitra became confused for a few hours and seemed to resume her ordinary personality. Then the Shiva personality resumed control and was still dominant at the time of our last interviews in October 1987. By this time the Shiva personality had been dominant—with the single brief exception just noted—for more than two years.

The Life and Death of Shiva

Shiva Tripathi was born in Sevpur in the Etawah District on October 24, 1962. Her parents were members of the Brahmin caste and her father, Ram Siya Tripathi, was a lecturer in a college. After 1965 the family lived in Etawah, and Shiva grew up there along with five brothers and sisters. She attended school and then college from which she graduated with a B.A. in Home Economics.

At the age of 18 and a half she was married to a man called Chhedi Lal, who lived in the village or small town of Dibiyapur.

Shiva gave birth to two children who became known by the nicknames of Tinku and Rinku. (Tinku was about 18 months old and Rinku less than 6 months old when Shiva died.) Shiva was living (according to the custom of joint households in India) in her in-laws' house. Friction developed between Shiva and her in-laws. It is possible that Shiva's superior education and more urban manners irritated her in-laws. They grumbled when Shiva returned to Etawah in order to write final examinations for her college degree. Shiva complained that her mother-in-law had told her to go and hang herself. At one stage her father-in-law wrote to Shiva's father and suggested that he take her back, but nothing came of this.

A more serious quarrel developed in the second half of May 1985. Shiva was invited to attend the wedding of a member of her family, and her in-laws, after at first agreeing to her absence for this function, changed their minds and forbade her to leave the house. On the evening of May 18, 1985 Shiva's maternal uncle by marriage, Brijesh Pathak, who lived in a village (Kainjari) about a kilometer from Dibiyapur, called on the family and learned from Shiva about the quarrel she had had with her in-laws. Shiva was crying and told him that her mother-in-law and one of her sisters-in-law had beaten her. She did not seem depressed, and she did not talk of suicide. Her uncle tried to calm the family members and advised them to ask Shiva's father to come and arrange a more durable peace.

The next morning Brijesh Pathak and his brothers learned that Shiva had died in an "accident." Her dead body had been found on the railway tracks, and her in-laws said that she had thrown herself in front of a train. We

interviewed five persons who saw Shiva's body on the morning of May 19 before it was cremated. When discovered, it lay between two rails of a track at the railway station of Phaphoond (which adjoins Dibiyapur). The body was intact and therefore had not been run over by the wheels of a train; several trains had passed the station during the night.

Brijesh Pathak, remembering the quarrel between Shiva and her in-laws of the night before, asked them to delay cremation of the body until he could go to Etawah and bring Shiva's father (which would take only four hours, because the railway stations of Dibiyapur and Etawah are both on the main railway line). However, Shiva's in-laws ignored his pleas, obtained permission from local authorities to cremate her body,[5] and lit the fire at about 11:00 a.m. To make it burn more quickly they had poured fuel oil on the wood.

Shiva's in-laws said that they had noticed her unexpected absence from the house and had gone in search of her. Her body had been found on the railway tracks at the railway station, and they concluded that she had thrown herself in front of a train. They presented this account of Shiva's death during judicial inquiries and also in our interviews with them.

Although it is not uncommon for young married women in India who are harassed by their in-laws to commit suicide, several circumstances in the death of Shiva pointed away from suicide and raised a suspicion of murder. First, there was the history of the quarrel on the evening of May 18, when Shiva had told her uncle that her in-laws had been beating her. Second, rumors began to circulate in Dibiyapur about persons having seen Shiva's in-laws carrying her during the night to the railway station at Phaphoond (just a few hundred meters from their home). They were said to have explained that they were taking her to the hospital. Although there were people around the railway station, it was night, and at one point the lights in the station failed; it would then have been dark, so that a dead body might have been placed on the tracks unobserved. (Firsthand confirmation of this report would have been of critical importance, but we were unable to learn even the name of a firsthand informant.) Third, Brijesh Pathak, who saw his niece's body lying on the platform of the railway station before it was taken away by her in-laws, remarked that only the head was injured; he thought it suspicious that a body knocked down by a train was not more extensively damaged.[6] Fourth, although it is customary to have an inquest and autopsy after any accidental death, the *panchnama* was signed with suspicious haste, the expressed wishes of Shiva's uncle to delay the cremation until her father could arrive were ignored, and the cremation proceeded with hurriedly.

Ram Siya Tripathi arrived at Dibiyapur around 2:00 in the afternoon of May 19. By that time the cremation fire had reduced his daughter's body to ashes. After considering all that he could learn about the circumstances of her death, he complained to the police, and they began a belated inquiry. Later, he filed a formal charge of murder against Shiva's in-laws. Shiva's husband and father-in-law were arrested and then released for lack of evidence. Her mother-in-law and sister-in-law absconded and remained in

hiding for some months. In 1986 they returned to their home, were arrested, and then released pending an expected trial. In October 1987 the judicial inquiry was continuing with the usual delays of such proceedings.

On the facts available to us, we must suspend judgment about how Shiva died. That she died violently and after a quarrel with her in-laws on the night of May 18–19, 1985 is established. Her in-laws may have killed her and put her body on the railway tracks to simulate a suicide; or she may have thrown herself in front of a train during a trough of depression following the quarrel with her in-laws.

Sumitra's Statements About the Life and Death of Shiva

Sumitra's statements made after her revival may be divided into three groups. The first group consists of names of persons and places that the newspaper accounts of Shiva's death and her father's lawsuit had published. We think it extremely unlikely that anyone communicated even the fact of Shiva's death, to say nothing of its details, to Sumitra or her family. However, as we have mentioned, some newspapers were circulated in the general area of Sharifpura, and so we must assume that Sumitra's family might have learned about Shiva's death normally. This means we cannot count as paranormally derived any of the names Sumitra stated that had appeared in newspaper accounts.

A second group of Sumitra's statements remains unverified. We refer to her account of Shiva's final quarrel with her in-laws and of how her sister-in-law had hit her on the head with a brick, after which her body was laid on the tracks at the railway station to simulate suicide. Nothing refutes these statements, but they remain unverified and may be wrong.

A third group of statements, those concerned with nicknames and private affairs not published in the newspapers, includes statements that we think contain information Sumitra could not have obtained normally. We learned of 19 items that we felt justified in placing in this, the most important group. These showed that Sumitra had knowledge of: a particular yellow sari that Shiva had owned, a watch that had belonged to Shiva and the box (in the Tripathi home) in which it was kept, the respective order of birth of Shiva's maternal uncles (although one who was younger actually looked older than one of the older uncles), one of Shiva's nicknames familiarly used in the home (Shiv Shanker), the names of two educational institutions where Shiva had studied (Sarvodya College and Sorawal Intermediate College), the pet names of Shiva's two children (Rinku and Tinku), the names of two friends of Shiva who happened to have the same name, and the names of Shiva's two brothers, two of her sisters, two of her maternal uncles, a maternal aunt (by marriage), and a nephew.

Sumitra's Recognitions of Shiva's Family Members and Friends

Observers of recognitions in cases suggestive of reincarnation—of which the present case may be considered a variant—frequently vitiate them by

asking leading questions or by cueing the subject with glances directed toward the person to be recognized (Stevenson, 1975, pp. 39–40). Nevertheless, there remain several circumstances in which recognitions may occur that deserve credit as showing paranormal knowledge on the part of the subject. These are: recognitions that the subject makes spontaneously without anyone's having asked him or her to identify another person; recognitions that occur when the subject is confronted with a person and asked a question like: "Do you know who this person is?" or "Tell me who I am"; and recognitions in which the subject immediately afterward adds a statement about some intimate detail, perhaps a nickname, not known outside a small circle of family and friends. We learned of 12 members of Shiva's family and circle of friends whom Sumitra recognized under conditions that we believe excluded cueing. We shall describe the circumstances of seven of Sumitra's recognitions, including one in which cueing might have played a part and six in which we think it did not.

1. Ram Siya Tripathi, Shiva's father. When he first went to Sharifpura, he introduced himself outside the house and someone told Sumitra, who was then inside, that "her father" (that is, Shiva's) had come to the house. We therefore attach no significance to Sumitra's telling Ram Siya Tripathi what his name was. However, she called him "Papa" (as Shiva had) and wept. Also, when Ram Siya Tripathi asked her, Sumitra immediately stated two pet names by which Shiva was sometimes called in her family: Aruna and Shiv Shanker. The first of these names, Aruna, had been published in a newspaper report of Shiva's death, but the second had not.
2. Baleshwar Prasad Chaturvedi, Shiva's maternal uncle by marriage. Sumitra recognized him at the time he accompanied Ram Siya Tripathi to Sharifpura. Asked who he was, Sumitra at first said he was Arvind's father. (Arvind was one of Shiva's maternal uncles.) Asked to try again, Sumitra then said that Baleshwar Prasad Chaturvedi was the father of Arvind's wife. This was correct.
3. Ram Rani, Shiva's mother. This recognition occurred at the time of Sumitra's first visit to Etawah. Ram Siya tried to mislead Sumitra by telling her that her (Shiva's) mother was standing in a group of other women at the Tripathi house. In fact, Ram Rani had gone inside the house and was not in this group of women. Sumitra insisted that her (that is, Shiva's) mother was not among the group of other women; she then went into the house and searched for Shiva's mother whom she found and embraced tearfully. (Attempts were also made in another instance to mislead Sumitra deliberately, but failed.) In connection with this recognition we should note that Ram Siya Tripathi had already shown Sumitra a photograph of Ram Rani (see below).
4. Ram Naresh, another of Shiva's maternal uncles. This recognition occurred at the time of Sumitra's first visit to Etawah. Ram Naresh pre-

sented himself to Sumitra and said: "Who am I?" Sumitra said: "You are my mother's brother." He said: "Which one?" She replied: "Ram Naresh of Kanpur." He had formerly lived in Kanpur, and had moved to Etawah after Shiva's death.

5. Ram Prakash Dixit, another of Shiva's maternal uncles. He went to Sharifpura (at the end of October, less than 10 days after Ram Siya Tripathi had first met Sumitra). He had grown a beard, which Shiva had never seen. When Sumitra first saw him, he was sitting in front of her and remained silent. She recognized him as Shiva's mother's brother, but was at first unable to give his name. He then spoke a few words, and she immediately recognized his voice and stated his name.
6. Manish, Shiva's nephew (the son of her sister Uma). This recognition occurred in Etawah on November 22, 1985. Sumitra was at the Tripathi house on an upstairs terrace. One of Shiva's brothers, noticing Uma and Manish approaching, drew Sumitra's attention to them. Sumitra looked down and said "Manish has come." Sumitra went down from the terrace, hugged Uma. and called her "sister." However, this cannot count as a flawless recognition, because Ram Siya Tripathi had already shown Sumitra a photograph of Uma (see below).
7. Krishna Devi Dube, a friend of Shiva's youth. This recognition occurred at Sikandarpur when Sumitra visited her mother-in-law's family, in February 1986. More than eight years earlier, Krishna Devi and Shiva had known each other when Shiva used to visit one of her maternal uncles (Brijesh Pathak) in the village of Kainjari, Krishna Devi's native place. When Krishna Devi married, she moved to Sikandarpur and had not met Shiva for more than eight years prior to Shiva's death. When Sumitra saw Krishna Devi, she said: "Jiji! How come you are here? I died and have come into a Thakur's family and am helpless." Shiva would have known normally that Krishna Devi had married and moved to Sikandarpur; nevertheless, the Shiva personality of Sumitra —if we may use that expression here—seemed surprised to meet Krishna Devi in Sikandarpur. Shiva, when alive, had called Krishna Devi "Jiji." This word means "sister," and although Shiva and Krishna Devi were not real sisters, close female friends in India may use this form of address with each other.

In addition to the above mentioned and other recognitions of living persons, Sumitra was able to recognize 15 members of Shiva's family in photographs. When Ram Siya Tripathi first met Sumitra in Sharifpura, he showed her eight photographs in an album that he had brought. One was of his wife and children that was taken in 1967, that is, about 18 years earlier. Sumitra correctly identified all six persons in the photograph: Ram Siya Tripathi, his wife, his mother, his daughter Uma, his son Raman, and his daughter Shiva. Of the last, Sumitra said: "This is me."

Shown another photograph, this one of five of the Tripathi children,

Sumitra correctly identified and named all the persons in the photograph. (Some of these persons figured in the previous photograph and in others shown to Sumitra.)

Another photograph showed three adult women, two of them holding infants. Sumitra identified Shiva's mother in it and said the child on her lap was Shiva's brother Raman. She then said that one of the other women was a maternal aunt. She said the third woman was possibly another aunt, but she was unsure of this, and she could not recognize the child on this woman's lap.

Upon seeing a photograph of Shiva's young son Tinku, Sumitra began to cry, said the photograph was of Tinku, and asked where Tinku and Rinku then were.

When a photograph of Shiva's sister-in-law Rama Kanti was shown to Sumitra, she said: "This is Rama Kanti who hit me with a brick." (Ram Siya Tripathi said Sumitra's recognition of this photograph dispelled his remaining doubts about whether his daughter Shiva was possessing her.)

In showing the photographs to Sumitra, Ram Siya Tripathi's attitude appears to have been one of keen interest mixed with skepticism. He said that as he showed Sumitra the photographs in the album, he asked her to identify the persons in them and gave her no cues. She gave the name of each person and usually the relationship of the person to Shiva. The villager, Lal Man Dube, who escorted Ram Siya Tripathi and his relative Baleshwar Prasad Chaturvedi to the house of Sumitra's in-laws in Sharifpura, witnessed Sumitra's recognitions of the photographs as Ram Siya Tripathi showed them to her. He confirmed that her recognizing statements about them were entirely spontaneous and not cued by any remarks the visitors made. We have described only a portion of the photographs she recognized, and we have examined the photographs ourselves. Excluding repetitions—of the same person appearing in more than one of the photographs—Sumitra was asked to identify 17 persons in the photographs. She unhesitatingly identified 12 of them, identified another three after some hesitation, and failed to recognize two persons.

Ram Siya Tripathi showed the album of photographs to Sumitra when he first met her on October 20, 1985. Although he did not give her cues before she recognized each photograph, he did tell her after she had finished that she had recognized all the people in them correctly. We think it likely also that he communicated to her, if only nonverbally, that she was correct after her statements about each photograph, if not about each person in a photograph. Under these circumstances, Sumitra had some advantage in recognizing persons she met later in Etawah whose faces she had already seen in the photographs. (We have referred to two of these persons above, Shiva's mother and sister.) However, Sumitra was credited with recognizing and identifying (usually by name) eight members of the family or their circle of friends whose photographs she had not seen.

Some of the newspaper accounts of the death of Shiva and of the subsequent judicial inquiry included photographs of Shiva, but these were taken in 1979. The photographs of her as a child (which Sumitra recognized) and the photographs of other members of the Tripathi family had not been published.

In sum, Sumitra recognized 23 members of Shiva's family and acquaintances either in person or in photographs, some of them in both ways.

Sumitra's Failure to Recognize People and Places Familiar to Her

After her revival from apparent death Sumitra could not recognize the people around her in Sharifpura, such as her husband and her in-laws; they all seemed complete strangers to her. Similarly, when her father came from Angad ka Nagla to see her, she did not recognize him. Ordinarily, she would have greeted him and called him "Father," but instead she said of him: "I do not know him." She was persuaded to go to Angad ka Nagla, and somewhat reluctantly agreed to do so. She said that she had no connections there and no interest in the place. In the village she recognized no one and seemed not to recognize any of the places with which Sumitra would have been familiar.

Similarly, when Phool Mala, Sumitra's older cousin who had in effect raised her from the age of five, went to see her in Sharifpura, she did not recognize her; nor did she recognize Phool Mala's husband, Risal Singh.

Sumitra, after her revival, showed no interest in her husband and child. She refused her husband's amatory advances for some time and did not acknowledge that her child was hers. Instead, she asked about Shiva's two children. Over a period of some days and weeks, she gradually came to accept her husband and son and to respond appropriately to them. Of her child she said (while still insisting that she was Shiva, not Sumitra): "If I look after this child [meaning Sumitra's son] God will take care of them [meaning Shiva's children]. If I neglect this child, would God not punish me?"

Under this heading we may mention also Sumitra's disorientation for place. For example, when her mother-in-law took her out to the fields for natural functions—the usual site for these in Indian villages—she seemed nonplussed and asked what they were doing in the fields. When her mother-in-law explained, she said: "We have a latrine inside the house [meaning in Etawah and Dibiyapur]." This was correct for both the house of Shiva's parents and that of her in-laws.

Sumitra's Changed Behavior After Reviving

We have already described Sumitra's initial failure (after her revival) to recognize the people around her in Sharifpura, and how, after neglecting her husband and child, she gradually resumed more or less normal relations with them. However, she said that her son was Jagdish Singh's child from his first marriage. Her attitude toward members of the family of her in-laws was

that they were good people and, as she was thrust among them, she would be as gracious about the necessary adjustment as she could be. Nevertheless, there remained important differences in her behavior compared with that before her apparent death and revival. We shall now describe some of these changed behaviors.

Sumitra's Different Identification of Herself and Modes of Addressing Other Persons. Sumitra obstinately insisted that she was Shiva and sometimes would not respond or carry out a request unless she was called Shiva. She addressed her husband, Jagdish Singh, as "Thakur Sahib," showing respect, but distance. Previously she had called him (with the indirection Indian wives commonly use) "Guddi's brother." (Guddi was Jagdish's sister.) Formerly Sumitra had called her father-in-law by a Hindi word "chacha," meaning an uncle; now she called him "Father." Formerly she had called her mother-in-law by a word, "Amma," for mother; now she addressed her by another, more respectful word for mother, "Mataji."

In the month following Sumitra's apparent death and revival, a particularly poignant episode occurred. At that season (August) it is customary in northern India for women to show their loyalty to their brothers by tying a short length of string around a brother's wrist. This is known as the *rakhi* ceremony, and Hindus attach great importance to it. When Sumitra's brother came to Sharifpura and asked her to return with him to Angad ka Nagla for the rakhi ceremony, she refused saying she knew no one at Angad ka Nagla. The brother began weeping and begged her to tie the rakhi string on him. She still refused and began to weep herself saying that she had no brothers near her to whom she could tie the rakhi string. (We have our account of this episode only from Sumitra and have not learned about it from Sumitra's family.)

Different Style of Dress. Sumitra changed her style of dress. She wore her sari in a different manner and put on sandals, which Sumitra, like most village women, had rarely worn. Her new habits of dressing accorded with Shiva's style.

Caste Snobbery. After her revival Sumitra showed, for a time, a definite hauteur toward her in-laws, the Singhs. Thinking of herself as Shiva, a Brahmin, she regarded them, Thakurs, as inferior.

On the occasion of Sumitra's first journey to Etawah with Shiva's father, Ram Siya Tripathi, and her husband, Jagdish Singh, the group stopped at the home of Baleshwar Prasad Chaturvedi in the village of Umrain. After they had eaten a meal, Sumitra told Jagdish Singh: "Please wash the plates and utensils you used. You are a Thakur and they are Brahmins. It does not matter for me [meaning about her plates and utensils], because I am one of them."

Sumitra's Increased Literacy. Sumitra could read a little, and she was able to write letters and occasionally did so. However, the testimony of infor-

mants was concordant that she had never attended school and had attained only a very limited knowledge of reading and writing. After her revival she showed a marked improvement in her ability to read and write. We observed her in both of these activities and found her able to read and write Hindi with great facility. However, we wish to emphasize that the significant change in Sumitra's literacy was not in her basic ability to read and write but in her fluency in these activities and in her interest in writing. Whereas formerly she only wrote a letter occasionally, after the change in her personality she wrote letters and postcards often to Shiva's family in Etawah.

Other Unusual Behavior. Ram Siya Tripathi said that he noticed in Sumitra some behavior that he regarded as characteristic of Shiva, such as a certain boldness and a tendency to joke. Her husband said that Sumitra, before her apparent death and revival, would usually get up at about 6:00 a.m.; after her death and revival she got up much earlier, at about 4:00 a.m. Shiva had been an early riser; her father said that she used to get up at 5:00 to 5:30 a.m. and even earlier in the summers. One of her uncles commented to us that Shiva used to go to bed earlier and get up earlier than other members of the family. However, we recognize that traits such as we have mentioned in this section are difficult to appraise, and we think they are less important than the others we have mentioned.

Discussion

We propose to discuss what we believe are the four principal interpretations of the case: fraud; cryptomnesia (source amnesia) with secondary personality; secondary personality having paranormal knowledge; and possession of Sumitra's body by the deceased Shiva.

Fraud

We think we can exclude a hoax perpetrated by Sumitra alone. A barely literate village woman in India could not have obtained detailed accurate information about another woman who lived 100 km away without assistance. If there was a hoaxing team, who composed it? Sumitra's husband, as a man, could move around more easily than Sumitra, but he was not in a position to go to places like Dibiyapur and Etawah in order to search out unpublished details about the life of Shiva. It has been suggested that the exorcist Vishwa Nath, who had access to Sumitra (although probably never alone) before and after her apparent death and recovery, might have obtained information about Shiva and coached Sumitra with the details of which he had knowledge. However, this suggestion also, in our view, fails to take account of the information Sumitra had of the private life of the Tripathi family, and it fails to explain her ability to recognize 23 strange persons in person or in photographs.

Shiva's in-laws at Dibiyapur had all the information included in Sumitra's statements about Shiva's life and death, but they were already incriminated in Shiva's death and had an interest, therefore, in Sumitra's silence. Sumitra's "Shiva personality" was saying publicly that Shiva's in-laws had murdered her, and the in-laws could not be suspected of promoting this view of Shiva's death.

Shiva's parental family also had the information included in Sumitra's statements. Ram Siya Tripathi might have collaborated in a hoax. In talking with us, he himself mentioned this possibility. When we asked him whether he thought Sumitra might have learned normally about Shiva he said: "No. If she had done that how could she recognize me and members of my family?" Then he spontaneously added: "People say I have made this case up, but why would I do that? I am gaining nothing, and my [legal] case [against Shiva's in-laws] will not be improved. Sumitra cannot be a witness. I am not getting my daughter back." We think he is correct on these points.

Sumitra and her in-laws might have gained a little status from the change in her behavior and from the social elevation of a "new" family member from the Thakur caste to the Brahmin one. In addition, her husband, who had been away a lot prior to the change, was staying at home more than he had before and presumably giving her more attention. However, all these possible gains seem to us minuscule.

Cryptomnesia

The interpretation of cryptomnesia with secondary personality suggests that Sumitra somehow obtained information about Shiva's life normally without being aware that she had done so and also without her family being aware of this. She would then have used the information in the construction of a secondary personality (Stevenson, 1983b). As mentioned, information published in the newspapers about Shiva's death may have reached persons in Sharifpura, even though we found no evidence that it had. The next steps are, however, difficult to imagine. How could the published information—and much else besides—have been passed on to Sumitra without others being aware that this had happened? Moreover, how could information in a verbal form have enabled Sumitra to recognize many members of Shiva's family in person and in old photographs? Such recognitions depend on tacit knowing that cannot be conveyed in words (Polanyi, 1966).[7]

Apart from the newspapers there were the traders who went between Sharifpura and Etawah and the women of the neighboring village of Murra who were married in Dibiyapur or nearby. The latter group particularly drew our attention. If one or two of them had brought the news of Sumitra's death, revival, and changed personality to Dibiyapur, could they not also have been conduits for information from Dibiyapur about Shiva's death and other details of her life? We spent considerable time both at Dibiyapur and Murra in probing for ways in which this might have happened, and we

cannot see any way in which it could. Although Murra and Sharifpura are only 2 km apart by road (and closer across the fields), there is little intercourse between the villages except on special occasions. Because they are so close, they are considered twin villages and each is inside the other's boundaries for acceptable exogamy; therefore, there are no marriages between the two villages. At the same time, the villages are far enough apart so that the inhabitants of one do not meet those of the other when they are at work in their fields. One informant of Sharifpura, who lived less than 60 m from Sumitra's house (and was distantly related to her), assured us that it would have been "impossible" for women of Murra to have told or taught Sumitra what she knew about Shiva's family. Again, even supposing that the women of Murra had brought back and somehow conveyed to Sharifpura some news of Shiva's death, such knowledge as they may have had would not have accounted for all of Sumitra's knowledge of Shiva's private life and for her ability to recognize so many persons known to Shiva, but whom Sumitra had never seen before.

Secondary Personality With Paranormal Knowledge

Most secondary personalities do not demonstrate paranormal knowledge;[8] but a few exceptions have occurred, and the case of Doris Fischer (Prince, 1915–1916, 1926) belongs in this small group. There is no evidence that Sumitra had any powers of extrasensory perception before her apparent death and revival. However, if she somehow acquired such powers she might conceivably have obtained information paranormally about the life and death of Shiva and then developed a secondary personality with that information. The sudden enhancement of Sumitra's ability to read and write makes the case parallel to that of Pearl Curran; she was a person of extremely modest education whose secondary personality, "Patience Worth," wrote a series of remarkable historical novels that seemed far beyond the normal capacities of Mrs. Curran (Litvag, 1972; Prince, 1927). However, "Patience Worth" showed little evidence of telepathy and certainly nothing like what Sumitra demonstrated, if we attribute her knowledge of details of Shiva's life to that process.

Possession

The evidence that Sumitra's case provides of paranormally acquired information invites comparison between it and the cases of Lurancy Vennum (Hodgson, 1901; Stevens, 1887) and Maria Talarico (Giovetti, 1985; Scambio, 1939). However, these were cases in which the change of personality was so marked that the term *possession* seemed applicable to them. And this is the fourth interpretation we wish considered for the present case.

If the other interpretations we have mentioned should be set aside as inadequately accounting for all the facts of the case, we are led to consider that a drastic change of personality occurred. When personality becomes

altered unrecognizably, taking on the attributes and the knowledge a deceased personality was known to have, it may be best to speak of the change as a type of possession or reincarnation. Although we do not dogmatically assert that this is the correct interpretation of this case, we believe much of the evidence makes it the most plausible one.

Endnotes

[1] Possession states occur in many other countries, one might almost say in all of them. However, we think it unnecessary to review the unmanageably extensive literature on this subject, all the more so since adequate reviews with further references have been published elsewhere (Bourguignon, 1976; Lewis, 1971; Pattison, Kahan, & Hurd, 1986). We should like to record our agreement with Lewis (1971, pp. 29–30), who warned against the futility of trying to find a single interpretation that will fit all cases of ostensible possession. The word *possession* labels many conditions of various types and different origins and processes.

[2] Although we shall refer to knowledge ostensibly obtained without the known sensory organs as *paranormal knowledge,* we do not mean thereby to beg the question of how such knowledge is communicated. However, the important question in any specific case, including the one of this paper, is *whether* such knowledge has been obtained, not *how* it was obtained.

[3] The absence of adequate written records in the villages makes some of the dates and ages that we give in this report approximate only. However, we know precisely the dates of Shiva's death and of Sumitra's later apparent death and revival.

[4] Santoshi Ma is a Hindu goddess regarded as the special protector of pious and faithful women. She was little known until this century and is not even mentioned in some standard works of reference for Hinduism, such as Walker (1968/1983). An immensely successful moving picture film about her in the 1970s both expressed and promoted the cult of Santoshi Ma in regions of northern India. Villagers of Uttar Pradesh would regard apparent communications from Santoshi Ma and seeming possession by her as unusual, but not pathological.

[5] The legal formalities for cremation of a body in a village of India require that five notables of the village sign a document (called a *panchnama,* which means "five names") authorizing cremation. Although some of the persons forming such committees are honest and incorruptible, many of them, at least in small villages, are likely to be friendly with the family of a deceased person and will do the family's bidding with and without bribery. Not surprisingly, observers give no credence to a *panchnama* if other evidence points to suspicious circumstances.

[6] However, if a train strikes a living person the head alone may be damaged either by impact with the train itself or if the body is lifted, thrown away from the train, and falls so that the head strikes the ground first. A more suspicious circumstance to us in this case is the location of the dead body within the rails of a single line. We find it difficult to believe that a person intending to commit suicide could have thrown herself so neatly in front of a train as to remain, after the train had passed, within the two rails of a track. We should have expected the train either to have thrown the body away from the line or to have run over it and severed it in two or more parts.

[7] Readers interested in a fuller discussion of this point—here asserted axiomatically—can find it in Stevenson (1974b, 1984).

[8] It would be more accurate to say that secondary personalities are not observed or not reported often to demonstrate paranormal knowledge. This may be partly due to the failure of observers to examine open-mindedly the evidence that some cases, such as the present one in our view, provide.

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CONTENTS

Page		
	Invited Essays	
1	Arguments Over Anomalies: II. Polemics	*Henry H. Bauer*
15	Anomalies: Analysis and Aesthetics	*Robert G. Jahn*
	Research Articles	
27	Trends in the study of Out-of-Body Experiences: An Overview of Developments Since the Nineteenth Century	*Carlos S. Alvarado*
43	A Methodology for the Objective Study of Transpersonal Imagery	*William Braud* *Marilyn Schlitz*
65	Experiments Investigating the Influence of Intention on Random and Pseudorandom Events	*Dean Radin* *Jessica Utts*
81	A Case of the Possession Type in India With Evidence of Paranormal Knowledge	*Ian Stevenson* *Satwant Pasricha* *Nicholas McClean-Rice*

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