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Message to our Readers

We hope you will forgive us the delay in producing this, the first *European Journal of Parapsychology* from Edinburgh.

Advances in desktop publishing have enabled us to produce the journal 'in-house', and as you can see, we have taken the opportunity to give the *EJP* a facelift. Caroline Watt did the desktop publishing, and she is eager to get feedback from readers on the visual style and readability of the new-look *EJP*. Features such as the font style and size can easily be changed for the next journal if readers find these to be unsuitable.

Readers will also see that we have tried to help the journal live up to its 'European' title, by including with each paper a *résumé* in French. We would like to thank Michel-Ange Amorim for generously agreeing to do these translations at short notice and without payment. We would welcome comments from readers telling us whether they find such abstracts helpful. If so, we may expand the service for the next journal, to include some other major European languages. However, we will need other kind souls like Michel-Ange to volunteer to do the translations...!

Thanks are also due to the authors who helped us build up a sizeable journal with a wide variety of papers, and to our referees, who gave each paper a rigorous inspection. As you know, for the foreseeable future the *EJP* will be issued only once a year, but it is anticipated that each issue will contain a large number of quality papers. We therefore eagerly await your contributions for next year's journal, by 31st August 1992, please!

Although the present journal contains no letters from readers, we would be delighted if the articles herein stimulate readers to write in with comments, ideas, criticisms, etc., which we may include in the next *EJP*. Indeed, the first article in this issue is an editorial essay by Robert Morris, which itself is intended as a stimulus for discussion.

We hope that you enjoy this issue of *EJP*, and that you will wish to continue your subscription by filling out and returning the form enclosed with this journal. Please also photocopy the form and distribute it to others who you feel would be interested.

With best wishes from the editorial team: Dr. Deborah L. Delanoy, Dr. Julie Milton, Prof. Robert L. Morris, Dr. Sybo Schouten, and Ms. Caroline A. Watt.

Department of Psychology
University of Edinburgh
7 George Square
Edinburgh EH8 9JZ
Scotland, UK



Editorial Essay

Parapsychology in the 1990s: Addressing the Challenge

Robert L. Morris

Department of Psychology, University of Edinburgh

Abstract: Parapsychology has several characteristics that create problems for it and that must be addressed if we are to become more than at best an intriguing protoscience. Parapsychology has been linked to metaphysical and occult traditions in the past. Acceptance of psychic phenomena (psi) has been exploited by charlatans. Acceptance of psi can easily contribute to delusional systems. Parapsychology threatens the precision and tidiness of traditional scientific methodology. It forces us to re-examine concepts such as consciousness and volition that have been largely ignored within science. It challenges fixed ideas, both materialist and non-materialist, about how the world works. Ethical considerations arise when designing research programmes. Parapsychology involves the study of complex, open systems. It has difficulty in generating and testing theory-based hypotheses. For these and other reasons, parapsychology has often been labelled a pseudoscience by philosophers and sociologists of science. At the Koestler Chair of Parapsychology, we attempt to address these issues by setting our research within the wider context of society as a whole, by developing models for understanding how we can be deceived by ourselves and others into exaggerating the role that psi may play in our daily lives, and by pursuing the best forms of evidence for psychic functioning. We seek to enhance its availability under methodologically sound yet ecologically valid conditions, through several lines of research. In this way we try as best we can to confront the problems raised earlier, with some success but with much room for improvement. Projecting to the future, parapsychologists need to address these issues more systematically than in the past; our own programme is offered as an example of such an attempt, to prompt discussion.

As recent political, environmental and technological developments remind us, the world is rapidly, irreversibly changing. Our understanding of brain and mind is also undergoing considerable modification, expanding in many directions. But what about the parapsychological research community? Are we participating fully in this change? And if not, as many would suggest, what can we do about it? John Palmer's recent PA presidential message indicates that Parapsychological Association membership is declining slightly overall; it is declining in the US, but expanding in the rest of the world. We are acquiring more diversity, an important characteristic for the survival of any population in times of change. Within the European Commu-

nity there are organized parapsychology groups in almost every country. Popular interest and involvement is high, according to a variety of social indicators.

How can parapsychology participate more fully in the current climate of global change, in the inevitable knowledge explosion that accompanies us as we move toward the 21st century? In the remainder of this essay I will try to identify some of the factors that make our progress difficult, and then describe the general strategies we have been adopting with some success here in Edinburgh, in hopes of encouraging discussion of how we might all proceed together to improve our research and its impact upon society.

Some Hindrances to Parapsychology's Progress

Parapsychology is linked to problematic metaphysical origins

It is helpful at the start to draw a distinction between parapsychology and psychical research. Psychical research was an emergent movement in the late 1800s, initiated by intellectuals who were driven by concern that the materialist, reductionist, clockwork model of the universe was acquiring a dominance it did not deserve. They felt that mind and consciousness were being left out, ignored by science or at best consigned to the role of trivial epiphenomena, incidental byproducts of brain physiology. The psychical researchers organized themselves to develop research strategies for the examination of mental and spiritual life, to query the nature of mind and its relationships to the physical body. Specifically, they focused on areas of human behaviour and experience that were most troublesome for the reductionist view: evidence for survival of bodily death, psychic ability in the living, hypnosis, volition, various psychopathological states such as hysteria and multiple personality, and so on. Psychical research tended to operate from a 'top-down' perspective, starting with major metaphysical issues and grappling with the nature of evidence bearing on those issues. It had its origins in various traditions, including both spiritualism and diverse occult systems.

Parapsychology, on the other hand, developed in the early 20th century, emerging from portions of the body of scientific research that the psychical research societies had offered. The focus was on evidence for psychic ability in living individuals, particularly on the experimental evidence. Parapsychology was organized more from a 'bottom-up' approach, drawing from a sizeable body of quantitative and qualitative data suggesting that individuals can in fact interact with their environment more fully and perhaps more directly than merely via the standard psychomotor means so well understood by the main-

stream scientific community. The parapsychologist's remit was to examine the nature of these data, to assess whether there truly are genuine anomalous abilities, and to develop programmes of process-oriented research to build an understanding of the basis for these abilities. Parapsychologists were well aware that genuine psychic ability could be regarded as evidence for a non-physical, non-reductionistic aspect of self. Some worked hard for this interpretation, noting that the results seemed to indicate that psychic ability was not constrained by biophysically relevant variables such as distance, time, sensory shielding or informational complexity. Others preferred a parapsychical interpretation, and explored extensions of modern physical theory that seemed to allow for non-local, transtemporal influence. And still others were neutral, open to a variety of interpretations, preferring to build a more solid data base, a more concrete empirical foundation, before attempting to construct extensive theoretical edifices.

In some sense, psychical researchers tended to be theorists looking for data; parapsychologists tended to be data collectors looking for theory. The 2 groups are not a tidy dichotomy, and they have much overlap of interest and endeavour. One of the problems parapsychology faces, however, is that it is generally identified with the spiritualist aspects of psychical research, in which it had its roots, and with occultism, with which it is indirectly associated; the public tend to regard parapsychology as an attempt to use the tools of science to prove the existence of a non-physical soul, or to prove that we all have special occult powers. Parapsychologists are seen as people who have already made up their minds, who are now attempting to use the tools of science to persuade others that parapsychologists' view of the world is correct. Thus we acquire enemies that we haven't earned and don't deserve. Some are from a neo-reductionist, rationalist, secular humanist tradition, perhaps exemplified by many of the formal sceptical groups that now also have organizations in most major countries.

Others come from more orthodox religious traditions, readily linking parapsychology with its heterodox metaphysical precursors. Both views are fed by the present-day linkages that the practitioners of various current, spiritistic, occult, New Age traditions often make with parapsychology. The findings of physical research and parapsychology are frequently cited in support of various beliefs and practices of these traditions, and often incorporated in a host of bogus claims for scientific validation.

Parapsychology is linked with concepts that have been exploited and misused in the past

A central tenet of parapsychology is that we humans (and perhaps animals as well) appear to have access to certain mental abilities above and beyond those presently acknowledged by orthodox bodies of scientific knowledge. Unfortunately, special mental powers are surprisingly easy to fake and have been incorporated into exploitative practices both by individual frauds, who seek financial, personal or political gains, and by fraudulent groups such as religious cults, whose leaders demonstrate special powers to validate the cult's philosophy and practices. Sometimes such frauds can be harmless or even beneficial, as in the case of the psychic or medium who serves as a relatively inexpensive therapist or bereavement counsellor. But such benefits are available through other means without the need for deception, and in the hands of unethical practitioners considerable psychological and socioeconomic damage can be inflicted upon innocent clients or cult group members. For those who attempt to provide legitimate counselling for the victims of psychic fraud, it is extremely inconvenient to have parapsychology around as a provider of scientific support for psychic claims.

The sophistication of psychic fraud has additional negative effects upon the scientific community, including those members who may be open to serious parapsychology and who are considering becoming

involved. First, it raises the possibility that any specific strong evidence for psychic ability produced by one or a small number of apparently talented individuals may turn out to be fraudulent, and there have been well-publicised examples of this in the past. Thus, researchers who are aware of these examples and who are dealing with an ostensibly talented performer who starts doing well under controlled conditions, may find that their initial hopes for effective research with a reliable source of psychic functioning plummet as suspicion of fraud sets in. That's no way to do business; it has no intellectual or emotional appeal to the researcher, and may lead many to minimise their involvement. Second, scientists and academicians in general hate to be fooled or held up to ridicule. They have made careers out of being knowledgeable and clever, and have no desire to risk being caught out. There is much more intellectual and emotional safety in being a sceptic, especially a sceptical commentator who does no actual research. If a particular anomalous event does not readily admit to an orthodox explanation, then those wishing to be regarded as good scientists have long since learned that it is safest to label the event a likely fraud, albeit a clever one. Such assertions may be a safety net for the researcher's (or commentator's) reputation, but, unless a viable fraudulent scenario is offered, these attitudes do little to advance our understanding.

Parapsychology can easily be linked with delusional systems

The possibility that we ourselves may have special mental powers, or may be influenced by the special powers of others, can lead us to develop problematic belief systems about how we interact with the world around us. Some are short-term and drug-induced: under the influence of a hallucinogen we become persuaded we can fly and so we attempt to do something dangerous such as jump out of a high window. Other beliefs may be the product of brain dysfunction: we hear voices and, unable to find a conventional explanation, decide that

a powerful psychic or supernatural entity is attempting to communicate with us; or our thoughts periodically get scrambled and we decide that someone must be deliberately interfering with our mental activities. Still other beliefs may just involve isolated anomalous experiences that cannot readily be explained without recourse to some paranormal interpretation. If we notice a strong correspondence between some specific thought, such as an impression or an intention, and some unusual external event, we may look for an association between the two. If other such coincidences arise, even undramatic ones, they may prompt us to search for patterns, for meaning in the resemblance. This may lead to a search for new coincidences, not difficult for an intelligent and creative person to find, with consequent testing of various hypotheses. After a while, a bizarre theory may emerge, that accounts for all the 'facts'. I may decide that I have special but poorly controlled mental powers. Or, it may seem that some other entity has singled me out for special attention. If no such 'paranormal' interpretation suits me, I may settle instead for a conspiracy theory, for example, that for some reason a group of people have conspired through complex but ordinary means to produce all these coincidences in my life. Any list of the presenting symptoms of schizotypic personality or schizophrenia contains several that are related to distortions in the perceived relationship between one's own thoughts and the outside world, that can be construed as increases or decreases in mental competence of some sort. Once again, many counsellors and mental health specialists find the existence of parapsychology very inconvenient; to the extent that we succeed in verifying the existence of psychic ability, even to a limited extent, we appear to give credence to distorted beliefs such as those described above. If someone is confused about their own mental activities, and knows of parapsychology's positive findings, they can readily form beliefs based on some of the more exaggerated or speculative interpretations of psychic functioning. The task of the mental health specialist who

accepts our evidence as valid can be made still more complicated by the need to tease out the legitimate from the bogus psychic events that their clients may present.

In general, these first 3 problem areas all relate to the fact that parapsychological findings can readily be seen to be related to beliefs and practices that have been largely discredited by the advance of science, and that are well known to be linked to various forms of fraud, deception, exploitation, human error, erroneous belief, and mental dysfunction. Parapsychologists can easily be regarded as at worst exploitative, colluding charlatans; and at best as earnest but misguided researchers whose sincere endeavours have muddied the waters for years and are quite troublesome for the efforts of real scientists and mental health professionals. This isn't a pretty picture for us, but we must acknowledge these concerns as having some validity, and as deserving of our attention. We cannot afford to ignore them or dismiss them as unfair and uninformed. The next 3 closely related areas deal less with the social implications of our work and more with its awkward implications for the scientific enterprise itself.

Parapsychology threatens the tidiness of our scientific methodology

If scientists in various disciplines take seriously the possibility of an indefinite set of additional means by which organisms are capable of interacting with their environments, then they would see that much of their experimental methodology would appear to need modification and improvement. Sciences that study organisms generally need to control the range of influences between organism and environment that transpire during the course of an investigation. Otherwise, interpretation of their results is rendered uncertain, due to the possibility of unmonitored, unwanted environmental influences. Scientists like to study relatively simple, closed systems, that are sheltered from all but the influences of interest to the researcher. The

problem is quite evident in psychology, where most of the research can be construed as the study of interaction between organism and environment. How do you study sensation and perception in subjects who have ESP? What about studying problem-solving or interpersonal communication? How do you examine psychomotor skills in subjects who may have psychokinetic abilities? How do you keep subjects blind to experimental conditions, to the investigator's hypotheses, to the researcher's hopes and expectations for their performance?

In analogous fashion, how do we take into account the possibility of psychically mediated experimenter effects? Such effects have already been proposed within the parapsychological literature, and involve either direct psi influence upon experimental conditions and outcomes, by the experimenters or any other interested parties, or more indirect influences, such as psychically mediated interactions between researchers and subjects.

One indication that the possibility of such influences has occurred to members of the scientific community is that the apparent absence of such influences is occasionally offered as evidence against the existence of psi. If psychic abilities truly exist, they argue, why don't non-parapsychologists see more evidence of them in their own experiments? Shouldn't experimenters much more often get experimental results in accordance with experimenters' own hypotheses? Shouldn't people succeed at ordinary experimental tasks in psychology, for example, much more so than researchers have noticed? Some scientists feel that the apparent absence of detectable psi in their (non-psi) experiments counts as evidence against psi's existence. At the same time, they are likely to have some emotional resistance to the idea of developing psychic functioning in the public as a whole, because that would make the business of conducting controlled scientific experiments extremely problematic, in ways already partly acknowledged by parapsychologists struggling to interpret all

the variance in results within and between experiments.

Parapsychology forces us to look at some theoretical concepts that science has found problematic in the past

By its very nature, parapsychology focuses on the nature of consciousness and experience, on imagery, a variety of altered states, and volitional mentation. The last includes both free-choice behaviour in the apparent absence of biasing information, as done in restricted-choice ESP tests, and conation or striving as is done in PK tests. Some of the theoretical possibilities put forth to explain consciousness are suggestive of the notion of vitalism, currently held as unnecessary by biologists. Within psychology and neurobiology, consciousness and intentionality are controversial topics, yet they are vital to understanding the phenomena studied by parapsychology. We cannot afford to ignore them if we are to explore our subject-matter fully. Conventional scientists may thus feel additionally ill at ease with our subject matter.

Because these topics have been difficult to conceptualise and research experimentally in the past, little is known about them. It is only relatively recently that experiential topics such as imagery have become actively researched. Volition has largely disappeared, replaced by motivation and, in a handful of studies, simple intentional acts. The qualitative aspects of conative mentation that are involved in PK studies are essentially unexplored within cognitive or experimental psychology. Thus parapsychologists have received little help from orthodox science in these areas so far, although the situation is improving.

Parapsychology threatens fixed beliefs about how the world works

By suggesting that we may interact with our environments through some unspecified new means, parapsychology threatens to reintroduce considerable uncertainty for those who have come to espouse very specific world-views. For those holding a

reductionistic, materialistic, secular humanist interpretation of the world, we appear to raise the possibility of some sort of direct, non-physical influence, perhaps even a spiritual one, of the sort advocated by various religions and held in disdain by traditional science. Some critics such as James Alcock have clearly sought in their writings to identify parapsychologists as researchers in search of the soul.

On the other hand, many theologians are also troubled by parapsychology and its implications. Some regard us as secularising sacred experiences, as raising the possibility that religious experiences, including supplicatory prayer, visions, ecstasy, and so on, will all be explainable as a combination of known psychobiology plus some additional mental force that is not necessarily spiritual in nature. They can cite various researchers who prefer to regard psychic phenomena as part of the natural world and who thus deny the existence of supernatural causes. Other religious leaders may regard us as probing in supernatural areas, but probing totally inappropriately, and even fear that we may awaken potentially negative or evil forces.

In short, because we are building a case for new means of communication or influence, without yet any kind of consensus about the nature of this influence, we are introducing fresh uncertainty into the worlds of those who have regarded themselves as certain of their core metaphysical assumptions. As a result, such people hope we will fail or, if their faith is quite firm, they know we will fail and regard us as temporarily dangerously misleading. Of course, some of them assume that we will succeed in verifying their own view and are thus more friendly, regarding us as buttressing their own arguments. This latter group are often disappointed to learn that we ourselves still debate the meaning of our results.

The next set of issues shifts the focus to the problematic nature of the research techniques in parapsychology.

Parapsychology's most obvious potential research projects often raise ethical issues

Much of parapsychological research as currently done seems bland and not to the point. If we take as our starting point the patterns that seem to run throughout the most striking spontaneous cases, we should be doing much more of our ESP research with participants in altered states of consciousness, including some fairly extreme ones; our target material should be highly arousing, emphasizing strong emotions and realistic emotional environments for agents; and our entire experimental milieu should be consistent with the first 2 points. Our PK research should follow analogous patterns, involving circumstances of strong need for our agents, and target material dramatically relevant to those needs. Many of our studies should involve strongly emotionally charged situations, with outcomes that are truly important for our participants. But such circumstances may raise strong ethical concerns for the physical and psychological well-being of our participants, researchers as well as subjects. Procedures for altering states may have side effects, both during the study and outwith the experimental situation.

Research on training techniques to help people enter psi-conducive states more readily and under more of their own volitional control, and to respond to milder target situations, would be a step in the right direction. However, it inevitably involves inducing considerable change in participants because they will be given new mental tools to explore, about which we know relatively little and for which sizeable individual differences undoubtedly exist. Also, participants will now be inclined to regard themselves as increasingly likely to possess genuine psychic skills, of the sort that, as discussed earlier, may readily become part of distorted, exaggerated views of reality. As they lead their daily lives outside the experimental setting, they will be tempted to apply psychic skills and abilities, to discuss the progress of their abilities with those people who are important to them, perhaps to interact with oth-

ers who make similar claims to have psychic ability, and so on. Both our participants and those around them are thus likely to be undergoing change, with the degree of change directly proportional to their success in our studies.

Many metaphysical and spiritual development traditions offer specific warnings about the use and misuse of psychic ability, and caution that participants who are not sufficiently spiritually advanced should not embark upon the path of psychic development. People who are involved in spiritual development are often even warned against becoming too emotionally involved with the psychic powers that will come their way in the course of their development, and are encouraged to ignore them. Yet on the other hand, in recent years an extensive psychic development industry has emerged, which appears to be offering psychic techniques to large numbers of laypeople who have made relatively little complaint. Whether these techniques are strong enough to produce bona fide psychic development remains to be seen; they may primarily be encouraging people to expect increased psychic functioning, and leading participants to misinterpret and read extra meaning into the relationships between their own mentation (mental activity) and environmental events. Whichever it is, the bottom line is that if we do training research with participants, we must confront a host of ethical issues affecting both the participants themselves and those with whom they come in contact.

Yet another sensible strategy is to do research with participants who have already had sufficient time to become psychologically comfortable with the notion of having psychic skills themselves because they have already completed psychic training programmes or have for some time appeared to manifest these abilities naturally. Many researchers have adopted this strategy, occasionally with success, but in some respects one set of ethical problems has been solved at the expense of allowing others to come forth. Training people to develop psychic skills and claiming to have voluntary control of such skills oneself can

be big business and can easily exploit people, as noted earlier. The researcher needs to guard against being fooled by individuals or groups who may have developed sophisticated deceptive techniques to guarantee a steady stream of satisfied customers. Anyone (be they genuine or fake) working with a respected researcher may choose to exploit that connection in later public promotional material or in private personal recruitment of new clients. One bogus psychic performer advertised that he had been officially tested at the parapsychology laboratory at Duke University; what he neglected to mention was that he had been tested only once and had scored at chance.

Researchers thus have 2 ethical issues to iron out when working with claimants who may be motivated to cheat or exploit connections with respected researchers. First, researchers need to ensure that any procedures in a study are sufficiently fraudproof that a claimant who attempts fraud will not succeed and will, ideally, be detected. This is necessary both to prevent or at least minimise fraud. Second, it is important to protect both the researcher's reputation and the reputation of any successful claimant. As researchers, we owe claimants the security of using procedures with them that will allow us to take successful results seriously and gladly, and that will protect claimants against unfair charges of fraud by opponents of legitimate parapsychology. Given the sophistication of fraud techniques and the difficulty of recreating the exact conditions of a successful test after it's over, the researcher is confronted with no easy task.

Unfortunately, there is probably no real way to guarantee absolutely that everyone who participates in one's research programme will not misrepresent the nature of their participation and what can be concluded from the results. Attempts to build in anonymity and privacy safeguards are helpful, and serve to safeguard those participants who wish to avoid the attentions of the media. However, well-known psychic claimants may make a case that the public deserves to know the results of any serious investigations with them. Negotiat-

ing when to talk about results, what to say about them, and to whom, has been and will continue to be a difficult issue when working with psychic claimants.

Parapsychology involves the study of complex, open systems

Parapsychology in some sense is the study of what really happens when somebody appears to have observed or experienced a psychic event. At least one and generally more than one observer or experiencer is involved. A whole host of alternative interpretations of the observed events must be considered and objectively evaluated. Often the researcher or other evaluators themselves are physically present during the crucial events and thus become part of the picture, needing to be described and taken into account. If they are not able to be physically present, then they must engage in some sort of reconstruction of the past event, be it a spontaneous case or a controlled experimental study. The former will be harder than the latter to reconstruct, because the former occurred under circumstances in which the details were not yet known to be important. Because there is such an abundance of alternatives to consider, many details are known to be important and quite a few others may conceivably be relevant as well. If we are taking seriously the idea of new means of exchange between organisms and environment, then it is hard to draw the line as to where the experimental (or spontaneous case) environment ends; what is a part of it, needing to be described, varied, held constant, and so on; and what can be safely ignored. For these reasons, and for reasons to be developed more fully later, it seems appropriate to regard parapsychology settings, be they the natural settings of spontaneous cases or the controlled environments of experiments, as complex, open systems. A system is a set of interactive parts; an open system is a system whose boundaries of influence cannot be precisely delineated. Scientists have their tidiest

results when they study systems that are: small, with few interacting parts; simple, whose known interactions are relatively straightforward and well understood; and closed, such that the full set of relevant parts can be readily specified. Physics has great problems at very small and very large levels of interaction, where the systems are harder to observe and inaccessible for experimental purposes; biology has problems with its most complex and open systems, those studied in ecology; and psychology has them when it considers social communication and complex mental processes. In parapsychology, our studies become more artificial and sterile as we try to simplify and close off the systems under scrutiny, and it is difficult if not impossible to place the researchers guaranteeably outside the system of study. Such considerations are especially germane when attempting to evaluate the importance of a failed replication attempt.

Parapsychology has difficulty in generating and testing theory-based hypotheses

Largely as a consequence of the above factors, parapsychology has not been able to reach a strong consensus about its domain of enquiry; the range of phenomena, of genuine events and experiences it is studying. As a result, it has been unable to specify the range and strength of phenomena that any theoretical system is obliged to explain. This makes theory construction difficult. Need we account for macro-PK phenomena, or can we settle for explaining information-based effects, disturbances in the randomness of large sets of events? Must we explain ghosts, poltergeists, reincarnation, and the healing power of crystals? Also, as a result of the factors discussed in earlier sections, we have had difficulty in producing any psi phenomena under adequately controlled conditions consistently enough to allow for effective systematic hypothesis testing.

Parapsychology has often been labelled a pseudoscience by philosophers and sociologists of science

Due to the frequent abuses of science in areas of strong popular interest, philosophers of science have long been interested in the demarcation problem: can we evolve criteria to help us all (including the public) demarcate between competent and incompetent science, or between real science and pseudoscience? Some writers, such as Andrew Lugg, have advocated that the focus should be on examining scientific practices, rather than on labelling entire areas of research defined by their subject matter to be science or pseudoscience. For various reasons, such as those mentioned in earlier sections, however, many find it convenient to regard the whole of parapsychology as a pseudoscience, because they see it as having a subject matter that is obvious nonsense with a methodology that produces only chance results when properly administered. Such writers tend to reject the subject matter out of hand, confusing the problem area (of why such phenomena occur) with speculative interpretations (e.g., such phenomena prove the soul). Or, they may acknowledge the possibility that the phenomena are legitimate but argue that if there truly were new principles of nature at work here, even if purely mental ones, then we should see much more evidence of these principles around us, we should have mastered these abilities a long time ago, and practitioners who are obviously successful should be in abundance and readily available to sincere researchers. Any controversy should long ago have been resolved favourably. This hasn't happened, so parapsychology's subject matter is obviously non-existent, they argue, and we should close the book on it.

Once the subject matter has been so conveniently dismissed for whatever reason, such writers find little need to examine the actual research methodology and published research reports in any real detail. Any positive evidence is seen as having flaws somewhere, and it's too much work to read the original reports in sufficient

detail to ferret out the difficulties. Sometimes, misrepresentations of the research procedures are offered up (as has been shown by Irwin Child for descriptions of the Maimonides dream research) and so a false public perception of the strength of the methods used is fostered. Or, poorly designed studies will be presented, and well-designed studies conveniently ignored. Lines of research may be inadequately described, yet held up to public ridicule. In short, writers who see fit to label parapsychology as a pseudoscience tend to be poorly informed and to pass on their ignorance to their readership. Those critics who do take the trouble to read the experimental reports in more detail, such as Ray Hyman and Marcello Truzzi, are more inclined either to regard parapsychology as a protoscience, in its early stages of development but with the potential to become a true science if it continues its path of development; or to focus more on individual practices, *à la* Lugg, noting that parapsychology contains a wide range of methodological sophistication, some woefully inadequate and some state-of-the-art.

The writings of sociologists and philosophers of science can be both blessing and curse. When well done they can be productive, helping ourselves and others to discriminate better between effective and ineffective practices, keeping our own thinking sharp. When done cursorily, or with an a priori bias, or with insensitivity to the complexities of working with humans rather than inanimate objects, they can do considerable damage, especially if they resort to rhetorical strategies such as ridicule and *ad hominem* attacks. They discourage popular support, potential funding sources, creative and intelligent students, potential colleagues and collaborators with badly needed expertise, and so on. If well trained researchers are chased away, the field is more likely to be populated largely by creative and innovative workers who do sincere research but do it poorly, due to a lack of training; or by amateurs with points to prove, whose research will also be of poor quality.

Much of the criticism directed toward parapsychology is, of course, legitimate and useful. We can learn from it and derive constructive feedback for our own ideas about our strengths and weaknesses, and about how to improve. Our topic has some very difficult problems associated with it, socially, philosophically, and methodologically. Taken together they are a fairly unique constellation, calling for unique and creative strategies for solution. Our participation as a discipline in tomorrow's world will be directly related to our efforts to confront and overcome these challenges. Only if we face them directly can we become a sufficiently active discipline, integrating with existing bodies of knowledge and extending them, producing the level of understanding that is needed to be both philosophically meaningful and practically useful to members of society.

The Koestler Chair Research Approach

At the University of Edinburgh, we have a group that averages between 10 and 12 people, including postgraduate students. We are evolving our own strategies for addressing the issues raised earlier, which I would like to put forward as starting points for discussion on how best we can all proceed. Some strategies work better for us than others, for some it's too early to tell, and all of them could stand considerable improvement.

The particulars of the Chair

The Koestler Chair came formally into being in 1985, and I assumed the post in December of that year. It is housed physically and administratively within the psychology department, and all of our students are psychology students. We are supported by the interest earned by the Koestler Trust, plus the earnings of the literary estate, plus small grants from a variety of sources. Prior to the Chair, parapsychological research had, for 15 years, been conducted at the department by Dr John Beloff and his students.

I do some undergraduate teaching and supervision of 3rd and 4th year undergraduate research projects, on parapsychology and related topics, in addition to coordinating our unit's research, supervision of postgraduate students, and various administrative and public relations activities. Dr Deborah Delanoy is a full-time postdoctoral research fellow, who shares administrative activities, supervises some research projects and pursues independent lines of research, primarily in the areas of free-response ESP training, target variables and individual differences. Ms. Caroline Watt is currently a full-time research associate but will shortly become a postdoctoral research fellow when she completes her doctoral thesis. She is involved in our training research and in looking at individual differences in perceptual defensiveness and vigilance in response to emotionally salient ESP targets. Mrs Helen Sims is a full-time secretary/administrator. Dr John Beloff is a retired senior lecturer, now honorary senior research fellow, who participates in our activities, writes on historical and theoretical issues in parapsychology, and edits the *Journal of the Society for Psychological Research*. Dr Julie Milton serves currently as an honorary consultant and specializes in free-response judging procedures and spontaneous case evaluations. Alasdair MacLulich is a medical student working part-time to develop new computer-based psychokinesis procedures. Delanoy, Watt and Milton share *EJP* editorial duties with myself and Dr. Sybo Schouten.

Four of my current postgraduate students are scheduled to complete their theses in 1992. Emily Cook, who took leave of absence from her work with Prof. Ian Stevenson at the University of Virginia, is doing a historical thesis on Frederic Myers' approach to the study of consciousness within the context of the past and present reluctance of the psychological research community to confront the problem directly. Richard Wiseman is working towards the development of a general theory of deception and has completed a set of experimental investigations of the role of prior attitude upon the recall of the details

of videotaped fraudulent psychic events. Robin Taylor is combining sports psychology with parapsychology, looking at the role of imagery in enhancing sport performance and in enhancing psychokinesis performance on a random event generator linked with a computer display. Caroline Watt, as noted above, is looking at perceptual defensiveness and vigilance as predictors of ESP performance.

Three others are expected to finish in 1993 or 1994. Shari Cohn is interviewing family groups within the Scottish Highland communities, looking to see if 'second sight' experiences show distinctive cultural or genetic patterns within family units. Chris Roe is looking at strategies used by fake psychic readers to be convincing to clients, including the use of statements designed to be regarded by most clients to apply uniquely to themselves. Last but not least, we are fortunate to have been joined for a 3 year period by one of the world's foremost parapsychological researchers, Charles Honorton, who is attempting for his doctorate to replicate and extend his successful automated Ganzfeld studies with us, using video clips as targets for people undergoing partial sensory deprivation. His approach, as described in various places later on, was developed at the Psychophysical Research Laboratories in Princeton, and his new research with us should be regarded as a joint endeavour between PRL and Edinburgh.

For the remainder of this section I will describe how our work attempts to address each of the problems described earlier: by setting our research within the wider context of society as a whole; by developing models for understanding how we can be deceived by ourselves and others into exaggerating the role that psi may play in our daily lives; and by pursuing the best lines of evidence for psychic functioning under methodologically valid conditions. In each of these we have had some successes and failures, with much room for improvement.

Problem 1: Parapsychology's linkage to metaphysical issues

According to the terms of the Koestler bequest, parapsychology is taken to mean, 'the scientific study of paranormal phenomena, in particular the capacity attributed to some individuals to interact with their environment by means other than the recognized sensory and motor channels'. This definition makes no metaphysical presumptions. We are committed to study a capacity attributed to certain individuals, with no necessary assumption that such a capacity actually exists. And the capacity is for interactions through means other than the recognized sensory and motor channels, which definition avoids any presumption about the nature of any psychic interactions that we or others may uncover. This approach is consistent with my own, which defines parapsychology as 'the study of apparent new means of communication, or influence, between organism and environment'. It's a bottom-up approach, oriented toward building a more complete understanding of the phenomena, experiences, and experimental data that suggest that psi exists. It is not wedded to a specific theoretical approach, but is data-driven, seeking to develop models that will, we hope, come to link our empirical data with the various partly-developed theoretical systems that currently exist. In turn, such systems may eventually enable a firmer linkage with some of the main metaphysical questions that fostered the origins of psychical research, but such links will only form gradually, in good time. That linkage, once made, may serve to confirm, modify, or completely disconfirm. By focusing on communication as our central concept, we readily build bridges conceptually to a variety of disciplines for which communication in the broad sense is a natural component.

In addition to my own recent writings in this area, Emily Cook's thesis work

serves to compel us to re-examine the origins of psychical research and the dedication of its earliest researchers to the application of scientific method to the nature of mind and consciousness, while avoiding glib or trivially clever pseudo-solutions to the relationship of mind and experience to brain events. In the course of our work, we remain aware of basic metaphysical questions and expect eventually to develop top-down approaches as well.

Such an approach helps separate us from occult traditions in the minds of those who learn of our approach through our writings and presentations. We're not dealing with clandestine or hidden traditions and practices; our focus is on open, concrete evaluation. By studying psi training techniques and developing better techniques for evaluating them, we build toward a firmer understanding of how to evaluate any practices and procedures that come to our attention or that we may locate ourselves. By developing an understanding of fraud techniques and how to circumvent them, we enable eventual fair evaluation of claimants from esoteric traditions. Thus we don't automatically disavow the utility of any such traditions, but we do not seek to support them either. Our approach would focus eventually, when we feel our methods are sufficient, on their practical, empirical claims rather than any associated metaphysical system. For those procedures that appear to work, we can then build toward a fresh understanding, that may or may not draw from concepts already extant within the esoteric tradition.

Comparably, we neither espouse nor deny a spiritualistic interpretation of psychic phenomena. Our work is aimed more at building a fundamental understanding of the phenomena most suggestive of survival, using our knowledge of fraudulent and deceptive techniques and of the properties of various forms of psychic functioning. A researcher wishing to understand the basis of impressive displays of mental or physical mediumship would need to have a firm grounding in 'spirit theatre', the area of performance magic devoted to faking seance room phenomena,

as well as an understanding of the conditions favourable to ordinary psychic functioning. If mediumship is to serve as evidence for true survival of bodily death, these 2 major alternative sets of explanations must be made explicit, evaluated, and ruled out. Even when (and if) ruled out, the findings from mediumship and related phenomena would need to be related to specific models of survival, that posit the properties of what it is that survives, in what form it survives, and how any surviving entities go about communicating with or influencing ourselves and events in this physical world. Once again, we do not ignore the questions posed by spiritualism, but focus instead on building a firm basis for later, more effective, investigations. The work of Richard Wiseman in critiquing early evidence for the physical mediumship of Eusapia Palladino serves as a good example.

Problem 2: Parapsychology's identification with exploitable beliefs

We attempt to confront this problem directly by studying the techniques of exploitation and their social context, to build as detailed an understanding of them as possible. This information can then be used both to design and conduct better research on claimants of the sort that may be involved in public exploitation, and to help people who may have been exploited in the past, or are currently at risk. There are several aspects to this investigation.

(1) *Understanding physical effects.* This is done mainly through collection of the magic community's literature, including catalogues of available effects as well as manuals of techniques, such as Fitzkee's *Trick Brain*, modern writings on special visual and electronic effects, and so on. Some of these already attempt to order the effects conceptually, but we do this ourselves as well, synthesizing themes and patterns from groups of related techniques.

(2) *Understanding mental effects.* These effects are also best understood through

analysis of items from the existing literature, such as Corinda's *Thirteen Steps to Mentalism*. Included in these writings are techniques for simulating telepathy, clairvoyance and precognition, as well as other special mental skills such as 'lightning calculation', and mnemonic devices.

(3) *Linkage with the observer.* Much of the psychology of deception, about which much less has been written, involves understanding how to present effects to observers so that they will interpret them in the manner desired by the deceiver. The observers' schemata, their ways of organizing their perceptions and of making sense of them, play a crucial role, in ways that we are gradually coming to understand, thanks to Richard Wiseman's work. Schemata can affect expectations; expectations can affect what we select to observe, how we interpret it, and how we will recall it later on. Competent frauds understand the schemata of their intended observers or audiences; pseudopsychics succeed best when they understand the beliefs of their clients and of those who would do research on their apparent abilities. Such beliefs can lead to patterns of observation, which are then capable of being exploited.

(4) *The social context of the claim and its negotiated acceptance.* Another focus of Wiseman's work is the psychology of negotiated acceptance of psychic claims, and the strategies for their evaluation. This includes the linkage between: an initial claim; a proposal for testing it; a negotiation, either before or during the testing procedures, for alternative ways of testing the claim if the pseudopsychic feels they cannot beat the test, either before or during the testing procedures; re-negotiation of the rules for evaluation after the test; announcement of various excuses for failure; affirming to others the success of the test despite its actual failure; and so on. What appears to be a straightforward, consensually agreed-upon procedure can become distorted in the course of things, especially if the test presents the pseudopsychic with unexpected problems. We are attempting to

develop procedures to overcome these difficulties, to allow fair formulation and testing of a claim while preventing unfair reinterpretation after the fact. Wiseman's protocol for evaluating the SORRAT claims is an example.

(5) *The evaluation of written or audiovisual archival material bearing on claims.* Features of the areas described above can be applied in evaluating earlier spontaneous cases, field investigations and experimental studies, which must be reconstructed from existing records, generally written but occasionally audiovisually recorded as well. Detailed reconstructions of primary records can be time-consuming and often evaluators will rely on indirect or secondary sources, including other evaluators, thus propagating any errors present in the secondary sources. By understanding the problems of evaluating archival material, we can learn how better to conduct our present investigations such that when we convert them into archival material that material will itself be more effectively evaluated by future researchers. Wiseman, for instance, noted that the writers of the Feilding Report on Palladino described in great length how they prevented her from physically cheating by restricting her own body movements, but they apparently gave little attention to the possibility of there being an accomplice. As observers, their schemata were organized primarily around an understanding of a specific set of tactics that Palladino was known to employ to cheat, and that's what shaped their strategies for conducting and reporting their observations.

(6) *The techniques of the verbal reading.* Much of the simulation of psychic ability occurs within the context of the psychic-client interaction. Once again there is a considerable literature, by writers such as Robert Coultie and Barton Jones, about fake psychic reading techniques. Chris Roe has developed an organization of the general groups of strategies that pseudopsychics can use: to gain information in advance about clients; to glean information

from their appearance and nonverbal behaviour; to make statements that represent sound advice or promise exciting future events; and to make so-called Barnum statements, that by their nature appear to any client to be uniquely descriptive of only themselves. Roe's experimental research focuses on the nature of Barnum statements themselves and their processing by the client, because that is one of the least understood features of pseudopsychic readings. By understanding the techniques of fake psychics, we can help spot fakes earlier and avoid wasting time on them, and can design more effective tests for those people who seem to have genuine talent.

(7) *Developing a general model of deception.* Parts of all of the above are being combined with the deception literature in other areas, such as military and commercial deception, to develop an over-arching general theory of deception. This is designed to help us understand the range of psychic simulation techniques at various levels, from the specific tactics to general strategy, so that we can relate our work and needs to comparable circumstances in other areas and thus have a broad range of application in areas beyond just parapsychology. Our work in this area is still in its early stages.

(8) *The pseudopsychic as confidence artist.* One of the most difficult aspects of psychic fraud to organize conceptually is the emotional side: the techniques by which a pseudopsychic can persuade others so completely of their sincerity (gain their confidence) that the client will allow the pseudopsychic to work under much more casual, informal conditions than they would if the client were sceptical or on guard against possible tricks. Deborah Delaney has published a vivid description of her own interactions with such an individual, and is interested in developing a more complete understanding of how the confidence artist works.

(9) *The social context of such exploitation.* We are only gradually beginning to build a picture of the use of such techniques in exploiting public interest, such as in recruiting people to cult groups, misleading members of the public through fraudulent practice, and so on. We have informal, indirect contact with groups concerned about these kinds of abuse, but this aspect of our work is not extensive and we view ourselves primarily as a resource; a source of information for cult counsellors, law enforcement officials and others who must deal with actual or potential victims.

One product of this work is the development of a manual for parapsychological researchers, written by Richard Wiseman and sponsored by the John Björkhem Memorial Fund of Sweden. The manual will be distributed to the major research centres and individuals later this year, and should be a valuable resource for improving research techniques, especially with psychic claimants. Included is information about various products designed for the security industry that will help us ensure that our research protocols are not readily violated, and that will allow us greater freedom in the strategies we can use to test people under more natural circumstances.

A feature of our work that should also be mentioned is that, largely through Richard Wiseman's efforts, we have very solid contacts within the magic community that we value greatly and hope to maintain.

Problem Three. Parapsychology and delusional systems

Our field's social responsibility includes helping people who have active delusional systems that involve what seem like psi components, to link up with the expertise they need to help them, if in fact it exists. That can include medical expertise, psychological expertise, social expertise (especially for isolated individuals) and parapsychological expertise. We also have a responsibility to help people who are at

risk of forming delusions; who are acquiring beliefs that could be detrimental to themselves or others, e.g., who are making ill-founded, misinformed attributions of psychic causality to themselves, others, and physical events. Our efforts extend to include helping people who are presently merely puzzled or somewhat fearful about their own experiences or someone else's, distressed by uncertainty or by potentially troublesome implications of these experiences.

We are addressing this responsibility in both general and specific ways. The deception work described earlier includes the study of self deception. The models we are developing are useful for describing human error, and can be readily linked with models from other areas, such as social attribution theory and human factors research in industry and technology. Caroline Watt's paper in this issue of *EJP* summarizes recent approaches in psychology as they relate to the study of attributed coincidences, and she has written earlier on the assessment of pattern in clusters of spontaneous experiences. Shari Cohn's research involves in part understanding very vivid spontaneous experiences, their phenomenological characteristics, and their integration into their sociocultural setting. My own recent writings and presentations to public and professional groups (including various medical groups) have dealt extensively with models of what's not psychic but looks like it.

At a more specific level, we are working to build concrete links to the public and professional communities, pacing ourselves as best we can, bearing in mind our limited resources. Our administrative secretary, Helen Sims, has taken training in counselling skills to enable her to deal more effectively with those who phone or come by in person with specific questions or problems to do with their personal situation. She attempts to learn the basic details of their circumstances and then either provides them directly with relevant information or else arranges for them to be linked with the appropriate sources of information, advice, or counselling. Often this must be done

through their existing medical contacts, so that we do not appear to be bypassing their own medical authority or colluding with their belief system. This is often difficult and our present arrangements are not always able to provide satisfaction. Additionally, we have clinically-oriented staff meetings approximately 8 times per year, attended by 2 psychiatrists (one also a minister), a clinical psychologist, and a liaison to community groups. At these meetings we review case material, discuss matters of proper legal and ethical procedure, liaise with other counsellors, and have presentations on relevant topics. The aforementioned mental health professionals provide limited volunteer short-term counselling, where appropriate, to people who have contacted us and are in difficulty, and they help those people seek any more extensive aid that they may require. Because our staff are not qualified medical professionals and our colleagues are volunteers, we are not in a position to serve as a true counselling centre such as has been available elsewhere, for example in Holland and Germany. Fortunately, at present there does not appear to be a major problem in Scotland, despite public awareness of our existence, and we hope to help keep it that way, emphasizing prevention rather than cure.

Although we are just in the very early stages, we are beginning to relate our work to existing models of mental dysfunction, including delusion formation, that have been developed within the psychiatric and clinical psychology communities, and it is our hope to contribute productive, fresh insights into these areas. Such contributions may involve helping counsellors assess the likelihood of genuine psychic functioning in their clients. If the likelihood is high, counsellors need to know how to help their clients deal with the possibility that the client will occasionally have experiences about which we know very little at present. They may also need to be able to advise clients who are tempted to increase or enhance psychic functioning; thus our psi-oriented research in general and specifically our study of training techniques and

individual differences in psychic functioning become relevant.

If the likelihood of genuine client psi is low, we hope to contribute techniques to help clients work through their past experiences and explore alternative interpretations of them. Much of our contribution may involve helping people understand how their unusual experiences interact with their belief systems. Some theorists, such as Brendan Maher, argue that people who have dramatic anomalous experiences may regard those experiences as inherently meaningful and that these people then function like rational scientists, attempting to form hypotheses and look for confirming or disconfirming events. Eventually they may evolve beliefs that have stood the tests of time and may well therefore have properties that render them unfalsifiable. Examples include: (1) I have special mental powers but they are hard to control; (2) I have been selected by a powerful external entity for special attention, either good or evil; (3) I have become the focus of a major conspiracy by an organized group. Each belief can easily accommodate a wide range of coincidences between internal and external events.

Some disturbances may involve exaggerations in one's perceived volitional mentation, or intentions. If my perceived intentionality is high, I may regard myself as responsible for everything; all events are in accord with my own wishes. If my intentionality is low, I may feel that my thoughts and actions are not my own, and that they therefore must be due to the intentions of others. Our psychokinesis work has led us to explore the nature of volitional mentation, including the notion of volitional styles and strategies, in ways that may be germane both for the understanding of delusional systems as noted above, as well as for areas such as habit management and performance enhancement, which also have clinical implications.

Finally, we need to help counsellors assess the likelihood that they themselves may be using psychic functioning, intentionally or unintentionally, in the process of providing therapy. This may show up in

interactions between therapist and client, as many have argued; or in the process of achieving clinical insight itself. Our work is only indirectly related to this issue at present, and there are many therapists in Europe as well as elsewhere who are addressing these issues.

Problem 4: Parapsychology and current scientific methodology

Science's existing standard methodologies are excellent for some purposes but greatly impoverished for others, as is being increasingly recognized. As parapsychologists, our job in part is to help point out areas where science's current tools do not work so well, and to suggest and implement improvements in methods so that we may more richly explore a wider range of areas of potential inquiry. As mentioned earlier, such areas can include those that involve complex, open systems, as well as areas especially susceptible to experimenter effects.

One specific way we are attempting to deal with these issues is to apply the general concepts of systems theory to our work: to regard spontaneous cases, field investigations and experimental studies as complex systems, themselves part of larger systems and yet having many subsystems as well. Our efforts are not at the level of sophistication of Walter von Lucadou's work; nevertheless, in constructing our models and designing our studies we try to think in terms of systems. My model for organizing the factors involved in evaluating psychic claims views the observer as a subsystem, which combines with the subsystem observed to constitute a larger system. The previously cited work of Wiseman, Roe and Watt, regarding deception and the evaluation of claims, can all be regarded in systems terms. We study open systems, because by the nature of our work we are exploring communication systems whose boundaries we cannot completely regulate. It is important for us to liaise with other disciplines that have similar topics; in the past, researchers doing systems-related work in fields as diverse as epidemiology

and accountancy have found thematic similarities between their problems and our own.

By its nature, parapsychology compels us to regard individual researchers, as well as larger segments of the research community, to be part of the overall system in which our work is done. This can be seen both in dealing with experimenter effects and in coming to grips with the nature of replication attempts. How we might best take extraneous (e.g., observer and experimenter) psychic effects into account in our studies remains to be seen, and is likely to emerge only after considerable additional research. Certainly this issue is at the heart of the observational theories. At present our research programme has no features aimed directly at this question, save for the considerations of the observational theories in the RNG-PK research of Gissurason while he was with us. Any research is potentially relevant, to the extent that it contributes to a fuller understanding of the nature of psychic functioning. In various of our studies, perhaps especially in our training work and in Honorton's automated ganzfeld research, the contributions of the research planners as well as those who actually conduct the experiment are both explicitly acknowledged. Honorton's most recent work has specifically incorporated direct measures of the degree of homogeneity of effect sizes, to enable us to assess the robustness of various effects beyond the idiosyncratic contributions of individual experimenters. In my own writings, I have tried more fully to develop psi-liberal versus psi-conservative models for daily psychic functioning. The former envision psi as pervasive in daily events but generally at a low and undetected level; the latter assume true psychic functioning occurs only sporadically, governed largely by the convergence of a small number of psi-conducive factors. Each way of regarding psi suggests different strategies for the design and conduct of overall research programmes as well as individual experimental studies.

A systems approach may help us understand why psychic functioning isn't more manifest in non-psi experimental

studies. Psi-liberal models would posit that psi functioning may be present but not detected, especially by researchers not oriented toward looking for it. Psi-conservative models would posit that psi functioning occurs relatively rarely, because sufficient convergence of psi conducive factors would rarely occur; when they did, they would tend to be discarded as bad data or anomalies to be ignored, if they were non-recurrent. Both kinds of models would allow for multiple observer effects that may either cancel or reinforce each other. To be useful, any such model needs to be developed sufficiently to generate testable hypotheses, a process with its own difficulties.

Problem 5: Parapsychology and the problem of consciousness

Our work intimately involves human experience, be it the conscious expression of information in ESP or the volitional mentation that accompanies PK. Parapsychology also embraces a variety of transpersonal experiences, and suggests a host of direct ways that consciousness may interact with the environment. Science has difficulty enough in exploring the psychological features of the brain states associated with such experiences, let alone making any real progress in understanding what produces the frequently vivid, complex and even bizarre contents of such experiences. Psychobiology and cognitive psychology are turning their attention increasingly in these directions; our own work as parapsychologists should ideally complement these more mainstream endeavours, both adding to them and benefiting from them in turn.

Much of our work explores aspects of consciousness as they relate to psychic functioning, and may contribute to our general understanding of these elements of experience and of how to study them. Imagery, in visual as well as other modalities, is an important part of several lines of research. Robin Taylor's thesis looks at imagery in both a PK and a sport performance context. Many athletes use mental

rehearsal to aid them, a set of techniques involving visual or kinaesthetic imagery of the actions being learned or perfected. Taylor's thesis explores imagery training procedures and internal versus external imagery perspectives, as they affect mental rehearsal and athletic performance. He is also looking at ways to build better controls into such research, that would take into account increased expectation of success when mental techniques are used to enhance performance. The psi training research by Delanoy, Watt and myself also explores techniques for training or improving imagery abundance, vividness and controllability. The research also looks at the use of directed and nondirected guided imagery procedures for linking with psi targets or for other forms of creative activities, problem solving, information retrieval and so on. Imagery training is combined with training in relaxation and attention regulation (concentration) techniques, that reduce external noise and allow internally generated mentation to dominate. Charles Honorton's research employs techniques such as auditory and visual sensory reduction, to enhance imagery production. All these projects are interested in individual differences in natural imagery production and in other cognitive/personality variables, and all are essentially exploring relatively mild techniques for producing altered states of consciousness. We hope to build up a firm understanding of such mild states before proceeding to more intense ones, of the sort reputed to be better able to facilitate psi functioning and transpersonal experiences, and that in general are more challenging for orthodox psychobiological explanations.

Intense transpersonal states are relatively less common in industrialized cultures such as our own, having had their origins in early shamanistic and contemplative traditions. It becomes important for us to understand the functioning of such states in other societies so that we can help them to be of use to us in our own style of society. Shari Cohn's exploration of imagery in 'second sight' traditions, in the Highland areas of Scotland, focuses in part on the

function of such experiences in those cultural systems. Additionally, I have supervised a series of advanced undergraduate projects looking at the use of mild versions of imagery, concentration and relaxation exercises within a stress management context, to learn how members of more orthodox segments of Scottish culture respond to such procedures, and to help us learn better how to introduce them and to whom to introduce them.

An additional area of exploration is in the area of volitional mentation. Loftur Gissurarson's doctoral thesis paved the way for exploring individual differences in volitional mentation through the use of interviews, questionnaires, behavioural indices and assigned volitional strategies. Early writers on volition such as Charles Spearman divided it into choice (or decision-making), and conation (or striving). Both are important for our understanding of volitional mentation in PK situations, because both are linked to the 2 main schools of thought about the linkage between mental activity and the biasing of probabilistic outcomes in PK tasks. The hypotheses based on a 'choice' model of volition, such as those championed by Edwin May and others, posit that success is the result of effective decision-making, of choosing psychically to initiate each trial at the proper moment. The conative hypotheses posit that the intentions of the PK agent matter, rather than the choices, that it is the direct willing or wanting that is important. Observational theories would be included here.

Gissurarson's work has involved questionnaires designed to elicit attitudes towards PK, usage of volition in daily life, volitional styles or strategies, auditory and visual imagery, and so on. He has interviewed his participants about their volitional strategies and catalogued their responses. In his PK research he has compared different assigned volitional strategies, and different sets of instructions about what kind of imagery to use in attempting the PK task. Finally, he has measured the delay time between consecutive trials, as an indicator of how long it takes participants

to implement their assigned volitional procedures each time they make an effort. This delay time has correlated significantly with assigned strategy, thus providing both an objective indicator that the assigned strategies were in fact being carried out, and a way of indicating which kinds of instructional differences matter for participants' mentations and which do not. This behavioural measure of inter-trial mentation time may be useful in studying differences in volitional strategies in a variety of other contexts as well, thus giving the study of volition within orthodox psychology a potentially valuable new tool, to be used alone or in tandem with other measures.

In addition to Gissurarson, who received his doctorate for this work, a medical student, Alasdair MacLulich, has been helping us develop volitional styles questionnaires to be used in a variety of contexts, including other volitional anomalies, habit management (e.g., smoking, drinking, use of other drugs), anomalous medical recuperation rates, and so on. His own experimental results have been quite encouraging so far.

It seems evident to us that parapsychology should not be seen as problematic for psychology and psychobiology because it compels us to re-examine concepts, such as consciousness and volition, that have been difficult and perplexing in the past. Instead, we should strive to be seen as colleagues, offering additional avenues for pursuing such questions. After all, part of the public's seeming indifference or antagonism to psychology may well stem from psychology's obvious reluctance to address the many aspects of human experiences that are truly of most interest to the lay community.

Problem 6: Parapsychology and our current understanding of the laws of nature

Parapsychology, like all other studies of extensive sets of anomalies throughout history, suggests that our present understanding of the world is seriously incomplete. We do not know yet where parapsy-

chological research will lead; it may appear at various times to support first one interpretation or world-view and then another, and may even come to suggest directions quite beyond those with which we are currently familiar. As noted earlier, we do not ally ourselves with any specific metaphysical view or theoretical system, and prefer a bottom-up approach. Although we don't completely ignore metaphysical concerns, we do maintain that none of the present world-views seems acceptable as it is; some modification is inevitably needed. It seems important to reaffirm that parapsychologists as a group are not engaged in some major spiritual quest, just as we are not dedicated to debunking spiritual interpretations. Secular humanism, on the other hand, should be seen as a major world religion in many respects, with its own set of metaphysical positions; as with other religions, we neither support nor disavow its main tenets. The question of survival of bodily death is a completely legitimate question, not resolvable within the evidence currently available, but amenable to systematic investigation nevertheless. This is not fence-sitting; it is merely the most honest, accurate picture we can offer, given the present softness of the data.

Problem 7: Parapsychological research and ethical issues

We are working to develop research procedures that will allow us to explore unusual experiences that have strong, meaningful messages. Our progress is very gradual, because we are aware that there are complex ethical concerns. There are several general features of our work which are designed with such considerations in mind. This process starts in our initial participant recruitment stage; we recruit through word of mouth, courses and lectures, as well as through screening the various people who contact us. Each candidate is sent a Participant Information Form, which helps us to identify those who are clearly goats, have had mental difficulty with psi in the past, are uncomfortable with the notion of exploring internal events or

displaying psychic functioning, or would be otherwise unsuitable. Also, it enables us to match participants' specific interests and past experiences with the study that should have most appeal for them, and gives us various individual differences measures that can then be correlated with participants' results. Researchers needing participants can select potential candidates based on their responses, contact them, explain the study to them, and let them decide whether or not to participate.

In the experimental settings themselves, we try to spend extra time so that we get to know our participants, their preferences, how they are reacting to their participation, and so on, and can take this into account in our interactions with them. We also try to keep people eager and excited, suggesting that we hope for and expect positive, interesting results, yet we try to convey a sense of balance; if the results of a given session are negative, that's no problem because we can learn from whatever happens. Some of our lines of research are aimed specifically at introducing intensity and meaningfulness into the research sessions:

(1) In the training project, participants are screened to be positive in attitude and interested in improving their level of psychic functioning, but not involved with the media or other commercial activities related to psi. We help participants develop mental skills in relaxation, imagery production and concentration, and they practice responding to pictorial targets previously selected to be visually interesting. Each week we discuss their progress with them, to exchange feedback. Attitudes towards developing psychic ability are explored in some detail, ideally in a positive way, allowing people to express and work through any concerns. It is made clear at the start that they should think through the personal consequences of their involvement before they commit themselves to the 12 sessions of the study proper. Although they know they are free to drop out at any time if they wish, we realize that there is an inevitable sense of obligation to finish something once it's been started. In the ses-

sions themselves, people are introduced to simple, and then to more elaborate and involving exercises, and are allowed to choose the exercises and psi testing procedures they wish to emphasize and explore more thoroughly in the later sessions. Most of the learning takes place in home exercises between sessions, which are then discussed during the next session. This allows us to understand the impact of the exercises in more detail and to ensure that no problems arise, as best we can. This is designed in part to let us gradually work toward more intense exercises and testing situations in later studies. We've tried to explore devising exercises to follow up on any target domain declared meaningful by the trainees, but have not yet had much luck, because they so far have not readily identified areas of interest that are amenable to experimental procedures. The training research is designed both to study the psi training process itself and to help us evaluate many of the claims made for various techniques, as well as to help us build up a pool of potential participants for our more theoretically oriented research. It is important for us to be able to work with people who are already familiar with us and comfortable with the idea of participating in process-oriented research. Those who have participated fully in the training sessions hopefully will have developed a good sense for the internal states, target types and other conditions that work best for them.

(2) Caroline Watt's perceptual defensiveness-vigilance work is aimed in part at locating people who readily respond to emotional material, allowing it to manifest itself readily in conscious impressions. In later stages, this work will move towards physiological monitoring and the exploration of emotionally powerful target material.

(3) Charles Honorton's automated Ganzfeld research has focused on recruiting participants who have already had a variety of psychic experiences and have a positive attitude toward psi. The target ma-

terial includes emotionally salient film clips, presented to senders of the receiver's own choosing. The Ganzfeld procedure itself is designed to facilitate a reduction of external noise, from the environment as well as bodily sources. Additional instructions are designed to help minimize internal sources of noise as well. With the care put into conducting each session, the participants are given the opportunity to have a fairly intense personal experience, while in a supporting, friendly social environment and in an infomal and pleasant physical location.

(4) Our psychokinesis work has focused in part on helping people pursue a variety of volitional mentation strategies, eventually to allow them to select those they prefer, so that they can explore those strategies in more detail. Gissurarson's and Taylor's work explores individual differences and imagery training with take-home exercises. Gissurarson specifically encouraged people to relate the target displays to goals that they found important in their personal lives, but he let each participant select their own goals. Taylor's work focuses on athletes as naturally goal-oriented, motivated people, whose PK target-displays featured graphics relating specifically to the sporting activity they were working on with their mental rehearsal techniques. Alasdair MacLulich has focused both on individual differences and volitional mentation strategies, and has devised a PK display that allows a variety of motivation to come into play, including health-related imagery, habit management and gambling strategies. Konrad Morgan earlier completed a doctoral thesis with us, looking at evidence for PK in the real-world setting of novice and sophisticated computer users learning about problem-solving routines. The PK element came from the presence of noise inserted into the problem-solving algorithms, which could enhance or degrade users' performance. As our work progresses, we will continue to develop increasingly salient PK target material that is readily amenable to various imagery-related volitional strategies, as well as

being easily related to strong needs and motivations.

(5) Shari Cohn's work looks at vivid spontaneous cases, having strong emotional impact upon the people involved. Her interviews are building up a rich picture of such experiences from a phenomenological perspective, and also in terms of sociocultural and familial factors.

(6) We are developing take-home tests that are adequately safeguarded from tampering so that we can turn them over to participants to work with on their own, without external supervision. Such a procedure allows more flexibility for participants to explore techniques that they may wish to keep private, or that call for special environments, or that need to be paced over a longer time than would be convenient in a scheduled, supervised laboratory experiment. This pertains both to the impression period itself as well as to the blind judging process, which can also be an artificial situation when done in the laboratory. Part of our interest comes from the dramatic successes our training participants had in their informal take-home exercises, which we unfortunately could not count because they were not adequately safeguarded from deliberate tampering. Part also comes from the often repeated complaint made by potential research participants, that they do not feel comfortable interacting with a target in the sterile laboratory environment. We realize that, by increasing the flexibility of our tests in this way, we also yield some experimental precision in terms of not being able to specify in great detail the environmental variables that may be responsible for whatever successes may occur.

There are additional ethical issues that need to be considered, by different labs in their own ways. It is important to have research procedures that are adequately safeguarded, to enable researchers to feel positive regarding successes without becoming concerned about whether good results are simply indicators of fraud or a

flaw in design. The participant also deserves to be protected, by having the procedures sufficiently well controlled that any legitimate successes cannot be easily dismissed. Privacy safeguards enter here as well, to protect participants from becoming public figures against their wishes, as well as to discourage potential participants who merely wish to exploit the public relations aspect of working with a respected experimental team.

Such ethical concerns are not always clear-cut. Richard Wiseman's Guidelines for Testing Psychic Claimants, when published, will make recommendations for various aspects of privacy safeguards, fraud prevention and detection, and matters of protocol when preparing to work with persons making strong psychic claims.

Problem 8: Parapsychology and the study of complex systems

We are still proceeding gradually in this area, because it represents a more complex, and in some respects less precise way of doing research. The usual strategies of controlled experimentation, with independent and dependent variables, may often be quite inappropriate. We may need to focus more on strategies for evaluating the output of definable psi conducive systems, foregoing at the start the systematic exploration of specific causal linkages. This is especially true of studies done in other cultures, or in evaluating training techniques or individual claimants with idiosyncratic procedures, and so on. Our efforts to apply a systems approach in our work have already been described above and there is no need to elaborate them here.

Problem 9: Parapsychology's difficulties in generating theory construction

There are several aspects to the problem of improving the construction and testing of theory in parapsychology. First, it is important to define our domain of inquiry. Ultimately, we all strive towards a 'theory of everything', but to arrive at that stage we must first do business in more manageable

areas of human enquiry. Earlier I defined parapsychology as the study of apparent new means of communication or influence between organism and environment. We are thus not obliged, as parapsychological theorists, to account for areas of anomaly such as ball lightning, UFO observations, crop circles, sightings of other species and the like, unless and until the data from those areas indicate a strong thematic overlap. Within parapsychology, we can further refine our domain of obligation by learning how to eliminate observations that involve fraud and other deception, or that can be accounted for by more sophisticated models of self-deception, human errors of observation and judgement, and mental or physical pathology. Our own involvement in these areas is in its early stages, but some progress here has been made.

Secondly, we need to generate more complete descriptions of the phenomena that do seem to be genuinely parapsychological. Such clarification is important for spontaneous cases and recurring natural phenomena. The models mentioned above for orthodox explanations can be applied to aid in data collecting and archival evaluation. For fieldwork, there is no substitute for full, rich description. We cannot say today what range of factors will be of interest to tomorrow's theorist, but we must do our best to anticipate. New data-gathering techniques are needed (and are emerging within the social sciences) to allow large-scale data acquisition, so that the data reduction process is either speeded up or is done selectively, as needed, with the remainder available archivally to future researchers.

Clarification is also needed of the patterns of experimental findings that have shown up with reasonable consistency under adequately controlled conditions. We are now in a much better position to do this than ever before, thanks to recent advances in meta-analysis techniques. The leading researcher in their application to parapsychological data bases has been Charles Honorton, who has continued his work in this area since joining us. Meta-analysis allows us to examine overall effect

sizes for specific experimental procedures, to see how strong and how consistent they are from study to study and from researcher to researcher. This enables us to identify psi-conducive procedures, such as Ganzfeld stimulation, as well as procedures that consistently produce weak results, such as group ESP testing. The latter often may be convenient in terms of resources, but appear to be wasteful of resources given the small effects they produce. Effect sizes can be calculated for differences between conditions as well as for each condition, thus allowing identification of the kinds of patterns in results that tend to show up most consistently, such as the difference between dynamic and static targets in the Ganzfeld, defensiveness correlates of ESP performance, and so on. Additionally, meta-analysis can be used to assess the effect of methodological adequacy, by coding studies for specific flaws and seeing which flaws are in fact correlated with effect size. When this has been done in the past, many flaws have turned out to have less impact upon effect sizes than was expected; and some flaws did seem to affect results more than others. Flaws seemed to have relatively little influence upon random number generator PK studies, but were correlated with success in dice-based PK studies, suggesting that the basic soundness of the RNG procedures blunted the impact of flaws that ordinarily might have artifactually inflated results.

Once we have a picture of the patterns in our data, we can: (a) compare them with existing models as well as existing theoretical systems, where those systems are sufficiently refined to generate predictions; and (b) develop new models based on the patterns observed. Such new models can then be compared with existing theoretical systems, and can be tested by new data. Some researchers have been very active in the construction of theoretical systems and of testable models, such as Bierman, Houtkooper, Millar, Sheldrake, and von Lucadou in Europe; Braud, May, Schmidt, and Walker in the USA, and so on. Their models have led to programmatic research in the past, with varying degrees of success,

and each is capable of making predictions testable by new patterns in old data as well as by the generation of new data.

To establish firm links between theory and data collection, however, we need tools for collecting the most valid data we can, in forms that allow it readily to be linked with theories and models. This is a major portion of Shari Cohn's work, in interviewing her informants in depth and reducing the data from the transcripts into theoretically usable form; and it was Caroline Watt's concern in her review of the value of spontaneous cases and their use in formulating and testing hypotheses.

For the experimental data, the issues are straightforward: as with any other scientific endeavour we need results that are as valid and reliable as we can make them. Our efforts in this direction have already been described, and can be summarized as a plan with the following features:

(1) better initial selection of participants, based on better knowledge of what subject characteristics are most desirable for the specific testing situations at hand;

(2) use of training procedures, to help individuals learn to produce results sufficiently consistently to be of use in process-oriented studies;

(3) use of emotionally powerful targets, to increase the strength and meaningfulness of the message;

(4) use of testing procedures that can be self-administered, yet are secure from tampering;

(5) development and use of cognitive models to determine best when and how to tap into psychic functioning before the information has become unrecognizably distorted. The testing of such models would include psychophysiological as well as behavioural monitoring;

(6) more effective use of noise reduction models, including techniques such as the Ganzfeld, designed to dampen external

noise, and progressive relaxation or autogenics to dampen internal body noise, to permit more psi-conductive internal states;

(7) more intensive involvement with participants during the testing sessions themselves, and more care given to understanding their individual needs and interests;

(8) use of more motivating, absorbing PK target material;

(9) exploration of volitional strategies, to provide PK participants with the opportunity to evolve their own most effective volitional mentation styles.

Brian Millar once noted how boring it was to read original research reports that simply looked for evidence of psychic ability, but that made no attempt to test any hypothesis derived from theoretical considerations. His observation is important, because far too much research has been done in a theoretical vacuum, aimed solely at obtaining evidence of psychic functioning, to persuade oneself and others that it actually exists. It is equally boring, however, to read a study based on elegant theory-driven hypothesis testing, incorporating a clever research design, that nevertheless obtains such weak effects, so little evidence for psychic functioning, that we learn nothing at all about the theory in question. An effective research programme incorporates research procedures likely to produce effect sizes sufficiently strong that serious testing of models, of theory-driven hypotheses, can be done.

Problem 10: Parapsychology is used as an example of pseudoscience by philosophers and social scientists

This problem stems in part from the other problems, and is addressed in the same ways. However, this one can have advantages as well as disadvantages. As students of science have attempted to separate science and pseudoscience, good and bad science, they have proposed demarca-

tion criteria to distinguish them. Such criteria can be helpful to us, as indicators of the criteria by which we may be able to judge our own progress. Some criteria may be misapplied to parapsychology, the result of misinformation about us, in which case we have become aware of a misconception that needs to be corrected. Often this may be a matter of distinguishing in the public eye between serious parapsychological research and frivolous or exploitative occult practitioners. Other criteria may have some partial validity, such that awareness of them provides us with useful guidelines for where we need improvement, how we can best proceed beyond being a proto-science.

Toward the Future

The above problems, and the description of our strategies for addressing them, have been offered up for discussion and to provoke thought about how best we can proceed in our work, both individually and collectively. Our research must proceed with both sensitivity and rigour, using flexible strategies combining both theory-driven and data-driven programmes. It must also be set within the contexts of our cultural systems. Cultural diversity gives us both the challenge and the opportunity to explore many ways of doing this, for comparing perspectives, and for seeking balance and commonalities while retaining our important uniquenesses. The accompanying symposium on cultural differences in parapsychological style indicates some of these areas, where we may have much to learn from each other. If our endeavours can be set more effectively within our cultures, our societies, we will be more likely to address important social issues in ways that will be meaningful to those involved. Society badly needs and wants a firm understanding of psychic experiences. Other scientific institutions have let people down by ignoring these experiences or belittling them, dismissing them with arbitrary, simplistic, glib explanations. It's up to us to pull together to help rectify this failure of mainstream science. It is hoped that the specifics and their attendant issues

will be explored more fully in the pages of this journal and in the related journals of our community of countries.

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Department of Psychology
University of Edinburgh
7 George Square
Edinburgh EH8 9JZ
SCOTLAND, UK

Parapsychologie dans les années 1990: Aborder le défi

Résumé: La parapsychologie a plusieurs caractéristiques que lui créent des problèmes et que l'on doit aborder si l'on veut qu'elle devienne plus qu'une curieuse protoscience. On a relié dans le passé la parapsychologie à des traditions métaphysiques et occultes. L'acceptation des phénomènes psychiques (psi) a été exploitée par des charlatans. L'acceptation du psi peut facilement profiter à des systèmes d'illusion. La parapsychologie menace la précision et l'ordre de la méthodologie scientifique traditionnelle. Elle nous force à réexaminer des concepts comme ceux de conscience et de volition qui ont été largement ignorés par la science. Elle défie les idées reçues, matérialistes ou non, sur comment le monde fonctionne. Des considérations éthiques apparaissent dans la mise au point de programmes de recherche. La parapsychologie comprend l'étude de systèmes complexes et ouverts. Elle a des difficultés à générer et tester des hypothèses basées sur une théorie. Pour ces raisons et d'autres encore, la parapsychologie a souvent été qualifiée de pseudoscience par des philosophes et des sociologues de la science. A la Chaire de Parapsychologie de Koestler, on tente d'aborder ces questions en plaçant notre recherche dans le contexte plus large de la société prise comme un tout, en développant des modèles pour comprendre comment l'on peut se tromper soi-même ainsi que les autres en exagérant le rôle que le psi peut jouer dans notre vie quotidienne, et en recherchant les meilleures preuves de fonctionnement psychique. Nous cherchons à augmenter sa disponibilité sous des conditions méthodologiquement solides et néanmoins écologiquement valides, par différentes lignes de recherches. De cette façon nous faisons de notre mieux pour aborder les problèmes soulevés principalement, avec un certain succès, tout en accordant plus de place à l'amélioration. Dans le futur proche, les parapsychologues seront amenés à aborder ces questions de façon plus systématique que par le passé; notre programme est offert en exemple d'une telle tentative, afin de provoquer la discussion.

Introduction to *EJP* Symposium: Problems of International Communication and Collaboration in Parapsychology

Carlos S. Alvarado
Institute for Parapsychology, Durham, NC

Although science is considered by many to be a venture with no geographical barriers, the fact is that there are many problems that hinder the creation of a truly international scientific community. The language barrier problem is still discussed in this context. Citation analyses show that researchers more frequently cite work conducted in their own countries and published in their own languages than work conducted elsewhere and published in a different language (e.g., Inhaber & Alvo, 1978; Narin & Carpenter, 1975).

Similarly, there is little international collaboration, as measured by the rate of co-authorship (e.g., Frame & Carpenter, 1979). But language is not the only factor affecting communication and collaboration in science. Cultural differences may affect the scientific enterprise by creating, between groups, separations based on different concepts of nature. An example of such conceptual differences appears in Motokawa's (1989) comparison of Western and Eastern perspectives of scientific issues such as humans' relationships with nature and with each other. Cultural factors that lead to different ideologies seem to underlie the many differences in medical theory and diagnosis seen between physicians in different countries (e.g., Payer, 1988).

Problems of international communication and collaboration in science have also been of concern to parapsychologists. The papers that follow discuss this topic. They were first presented at a symposium held at the 1990 Annual Convention of the Parapsychological Association (PA). These papers - authored by Deborah Delanoy, Mario Varvoglis, and David Hess - are representative of recent interest in inter-

national communication and collaboration between parapsychologists. I have discussed aspects of this problem in print (Alvarado, 1989), as have parapsychologists from such other countries as Argentina (Kreiman, 1991) and Germany (Hövelmann, 1986). A recent official PA statement conveys this interest in increasing contacts between English and non-English speaking parapsychology: the PA recently established travel grants for PA members to fund travel from the country in which one lives to the one in which the PA Convention is being held (Rao, 1990).

The establishment of this grant program is a welcome development because some of the PA members who are non-native speakers of English feel neglected by the PA's habit of conducting its conventions in English. In a recent letter to me, veteran Italian parapsychologist Piero Cassoli (1990) said that he usually tells those who approach him for advice about getting into parapsychology that they should be aware that 'Parapsychology speaks in English,' and that, consequently, if they do not know the language they will be left out. Cassoli also complained about the lack of sensitivity of many English-speaking parapsychologists to the problems foreigners face in following conversations or presented papers. These problems are in part the topic of Deborah Delanoy's paper, in which she discusses the role the Euro-PA plays in combating both language and geographical difficulties in international communication and collaboration in parapsychology.

The two other papers are about cultural and social factors that underlie communication and collaboration between different communities. Mario Varvoglis - whose

paper focuses on French parapsychology - includes in his discussion the notion of scientific centres and peripheries. This is the idea that science usually has a central, main, or highly developed geographical location whose scientists lead a field not only in terms of the magnitude of their output of findings and publications, but also in terms of establishing standards and providing leading theoretical and methodological developments (e.g., von Gizycki, 1973). It is important to remember that national centres of science are always changing. As Ben-David (1971) noted, between the sixteenth and the twentieth centuries the centres of science have moved from Italy to Britain to France to Germany, and, finally, to the United States. Because of its economic and technological resources, many people now consider the United States to be the centre of science in general, as well as the centre of parapsychology. As such, the United States is in a position to have a tremendous influence on parapsychology. But this was not always the case and will not always be the case. After all, as recently as the nineteenth century, the United States was dependent on, and attempted to emulate, European science (Bruce, 1987).

In addition, both Varvoglis and David Hess - whose paper focuses on Brazil - approach the topic in terms of identifying culture-specific conceptual constructs that determine the topics studied in a particular field, and that affect the way science is conducted and interpreted. The study of these national traditions or styles of science is a topic of great interest today to sociologists and historians of science (e.g., Harwood, 1987). We may ask, for example, in what sense do groups conceptualize their phenomena differently; and what assumptions underlie methodology? For example, physical models of psi seem to be more prominent in the Soviet Union and in East European countries than in other places, a fact that may reflect the political and philosophical assumptions prevalent in those countries. Issues such as these, as Varvoglis and Hess remind us, are

complicated by competing traditions within a specific community.

I hope that Delanoy's, Varvoglis' and Hess' analyses will not only help us map our differences but will start providing us with ways to bridge the gap between international communities, encouraging communication and collaboration or, if necessary, to determine when and if such interactions are desirable or productive. In addition, these papers open up vast areas of sociological research which, if followed up by further studies, will help us to reconceptualize parapsychology as both a cultural and social phenomenon as opposed to a mythical universal and value-free intellectual enterprise.

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Institute for Parapsychology
Box 6847, College Station
Durham, NC 27708
USA

Improving Communication and Collaboration Between European Parapsychologists: The Euro-PA

Deborah L. Delanoy

Department of Psychology, University of Edinburgh

Abstract: European parapsychologists face a set of interrelated problems that militate against the existence of an effective communication and collaboration network between researchers. The main problems are differences in language, geographical isolation of researchers, the decrease in the number of research centres, and the low level of funding. In an attempt to combat some of these difficulties, European parapsychological researchers have started to hold an annual conference, the Euro-PA. The history and goals of the Euro-PA are discussed, and its effectiveness in overcoming some of the problems facing European parapsychologists is considered. A questionnaire was circulated to European PA members, collecting their opinions on a variety of questions relevant to communication and collaboration issues. The responses to this questionnaire are presented.

Good communication and collaboration between researchers are of paramount importance in any field of study. In a relatively small field such as parapsychology, where researchers are often working in relative isolation from their colleagues, the importance of a good communication and collaboration network is greatly increased. Parapsychology, in common with other disciplines, publishes a variety of journals and has a professional body, the Parapsychological Association (PA), which, among many other activities, sponsors an annual convention that helps to provide such a network. But are the conventional means successful in providing the necessary communication and collaboration network?

Researchers in parapsychology living in Europe confront a set of interrelated problems, none unique to the European situation, which may be seen as militating against the success of these conventional communication and collaboration strategies. Foremost among these problems are language differences, the physical isolation of many researchers from each other and from established research centres, the dwindling number of established research centres, and, last but by no means least, a lack of funding. It is not the intention of

this paper to examine these various problems in detail. Instead, the reasons why these factors can defeat the conventional means of communication and collaboration will be highlighted. The efforts being made by European parapsychologists to combat these difficulties will be discussed, and data collected examining the opinions of European PA members on a variety of communication and collaboration issues will be presented.

The problems raised by the language differences found between researchers living in different countries have recently been admirably addressed by Carlos Alvarado (1989) in his article 'The Language Barrier in Parapsychology'. In this article, Alvarado describes the various problems that arise from language differences, the most important being that researchers, particularly those for whom English is their first language, are unaware of important work that has been and is being conducted by non-English speaking researchers. As Alvarado notes, such ignorance has detrimental consequences:

Clearly, a researcher should be aware of any publication in his or her field of specialty, not only for complete cover-

age, but also for practical reasons, such as having knowledge of successful or failed replications, avoiding the repetition of mistakes or problems found in previous work, and obtaining ideas for further work. (Alvarado, 1989, p.129)

These problems are compounded by the number of languages spoken within the relatively small geographic area of Europe. Whereas many Europeans, especially non-native English speakers, are fluent in several languages, there are few who have mastered all the languages that would be necessary to have a comprehensive knowledge of all the parapsychologically-relevant work published in Europe. Another problem created by language differences concerns more direct communication between European parapsychologists. Because most Europeans know some English, this tends to be the language used when one is trying to reach a broad European audience. Hence, the *European Journal of Parapsychology (EJP)* is published in English, as are a few other parapsychologically-oriented European journals and bulletins (e.g., the *Synchronicity Research Unit Bulletin*, and the *Journal of the Society for Psychical Research*). But many Europeans, even those who are multi-lingual, do not have a truly fluent command of English. This can make direct communication between individual researchers very difficult, because one may lack the necessary vocabulary with which to express one's ideas and/or comprehend those of others. These difficulties can defeat attempts at communication and make any form of collaboration exceedingly difficult.

Another problem that hinders communication and collaboration among researchers involves the geographic isolation of researchers from each other and from centres of research. The lone researcher often lacks the facilities, technical knowledge, and equipment necessary to conduct research that would be considered competent by current, justifiably demanding, standards. And, research centres that could supply these are an increasingly rare commodity in Europe, as elsewhere. Also, being isolated from one's colleagues can be very demoralizing. One needs colleagues

who understand the complexities of our field, with whom one can explore new ideas, and so on. Communicating with distant colleagues, even when no language barrier is present, can still be a difficult undertaking. Whereas modern technology has provided us with some excellent communication aids, many individual researchers do not have access to a fax machine or to computerized electronic mail, and the telephone can be prohibitively expensive. The postal services can provide an affordable alternative, but it is nonetheless a very time-consuming and frustrating means of exchanging information, because it entails long delays between each communication.

The lack of research centres in Europe is a major problem, leading to virtually no employment opportunities within the field of parapsychology in Europe. Furthermore, of the few centres that do exist, only the Koestler Chair of Parapsychology, located at the University of Edinburgh in Scotland, is headed and staffed by full-time personnel who are PA members or associates, and has the facilities and equipment necessary to carry out a wide variety of research. And regrettably, in common with other European centres, the Koestler Chair does not currently have the resources to sponsor research in other countries, nor to have guest researchers come to work at Edinburgh.

The other research groups in Europe generally lack the resources associated with a fully functioning research laboratory. They are most commonly 'staffed' by dedicated part-time researchers, who must look to other full-time occupations to support themselves and their families. Given the limited resources of these groups, they tend, very sensibly, to concentrate upon fairly specific lines of research. This can sometimes have the result of further isolating other researchers or research groups, who, although they may share a common language and be living in relatively close proximity to one another, have differing research interests, or perspectives, or both. Nonetheless, despite the many obstacles that confront such groups, they continue to

play a very important role in European research.

The lack of funding is undoubtedly the greatest problem facing European parapsychology, a problem shared by the field in general. This problem underlies all the others mentioned thus far, and is the main contributor to the difficulty of solving them. We lack the funds to hire translators to help resolve problems created by there being different languages. And whereas many European researchers are multi-lingual, being (relatively) fluent in a language does not necessarily involve having the skill required to translate accurately carefully-worded technical papers. Furthermore, most multi-lingual researchers are unable to support themselves via parapsychology (due to lack of funding), and must turn to other occupations for their livelihood. Thus, even those who do have the ability usually do not have the time to act as translators, either for their own work, or for that of others. The lack of funding also means that most individual researchers and research groups lack the resources necessary to conduct many types of work and are unable to communicate with others as readily as is desirable. Indeed, most European researchers find it impossible even to attend conferences that are not located in their vicinity. The lack of research laboratories in Europe is a direct consequence of the lack of funding, which subsequently leads to the virtual complete lack of employment opportunity within parapsychology in Europe. Furthermore, lest we become myopic, it must be remembered that in Europe generally, there has been a marked reduction in academic and research funding of most fields in recent years. This reduction, whilst affecting most subject areas, will be especially felt in fields such as parapsychology which, at the best of times, has never enjoyed more than a subsistence level of funding.

One means of responding to these problems was to hold meetings in Europe specifically for European parapsychologists, which provided the opportunity for greater interaction between researchers. In 1976, this idea was put forth by Martin Johnson,

who proposed creating a European branch of the PA (Johnson, 1976). Johnson (1978) raised the issue again two years later, and in the following year he announced the formation of a European Regional branch of the Parapsychological Association (ERPA) (Johnson, 1979). One objective of this group was to arrange regional colloquia for PA members and associates, active in research, where they could exchange ideas and receive constructive criticism of proposed research projects. The other main objective was 'to stimulate and guide students and persons interested in carrying out some piece of research but living in areas which are less integrated in the mainstream of parapsychological research' (Johnson, 1979, p.2). According to John Beloff, who was elected to the working-committee, the ERPA met a few times at other conferences (PA conventions and SPR conferences), but never held a meeting independently of another organization. Apparently, even these rather informal meetings ceased in the early 1980s (Beloff, 1990).

The idea of having a specific meeting for European parapsychologists was revived in the late 1980s. Again, it was thought that a conference for European PA members and researchers would help address some of the communication and collaboration problems discussed above. The necessity for a European parapsychology conference, in addition to the annual PA convention, stemmed from many European parapsychologists being unable to afford either the time or the money to travel to the PA conferences when they are held outside Europe, as is usually the case. Thus, it was decided to hold a 3-day conference for European PA members and associates who were actively pursuing parapsychological research. This first conference was brought about by the combined efforts of Jeff Jacobs and Hans Michels of the Synchronicity Research Unit (SRU), Dick Bierman of Amsterdam, and Robert Morris from the Edinburgh research unit.

The first meeting of European PA members and associates (the Euro-PA) was

held in October 1988, in Holland. The format for the conference was that each participant was required to make a brief presentation lasting no longer than 15 minutes, which would be followed by a lengthy discussion period of approximately 30 minutes. With the format being aimed at encouraging open discussion, attendance was to be limited to 25 participants, and in fact, only 23 attended. Given the funding situation in Europe, conference costs were kept to a minimum. Holland was chosen as a venue because it occupies a relatively central European location, thus being readily, and relatively inexpensively, accessible to many Europeans. There were no registration fees, and a very pleasant yet inexpensive venue was found for the meeting.

A wide variety of primarily theoretical and methodological topics were discussed the first 2 days of the conference, and the half-day session of the third day was devoted to a roundtable discussion about inter-laboratory research (for further details of the conference see Blackmore, 1989). The main outcome of this first conference was that communication between researchers was greatly improved. The lengthy discussions that followed each presentation, and continued during the breaks, over meals, and at the bar into the not-so-wee hours of the morning, resulted in all of us having a much better knowledge and understanding of each other, of our ideas and perspectives, and of our individual problems in pursuing parapsychological research. Further, it is thought that most participants would agree they left the conference with renewed enthusiasm and an increased impetus to continue pursuing their parapsychological work, despite the many difficulties this entailed. In short, it was a great success and it was unanimously decided to hold a second conference the following year.

Before holding the second Euro-PA conference, the Edinburgh research unit circulated a questionnaire to all European members and associates of the PA, eliciting their opinions on a variety of issues concerning the organization of the Euro-PA and other topics relevant to European

parapsychological research. Of 58 questionnaires distributed, 28 were returned. Those who returned questionnaires tended to be those most actively engaged in conducting parapsychological research. The answers received to the questionnaire were to be adopted as policy in those cases where a clear majority opinion had been expressed, and in other cases, the issues would be discussed and decisions made at a business meeting to be held at the end of the next Euro-PA conference (held in November 1989). It should perhaps be mentioned, that at the second annual Euro-PA meeting there was no restriction on the number of people attending the conference, excepting that one had to be a member or associate of the PA.

The questionnaire items asking what the main goals of the Euro-PA should be and how it should be administered and coordinated, are the questions relevant to the present discussion of communication and collaboration (see Appendix for details of the questionnaire and a summary of the responses received). A key question was whether European parapsychologists should establish a formal organization. A related question involved whether to organize by country. Given the many different countries represented by the Euro-PA members and the different, sometimes divergent, research interests within each country, was it best to have a committee established in each country, elected by the PA members and associates of that country and to coordinate communication within that country, or would it be preferable to have a central organizational committee? The responses to this question were clear cut. Approximately 65% of the responses favoured having one central committee whose membership would rotate annually, and only one respondent wished to see committees organized by country. Furthermore, the majority opinion was against establishing a formal organization, the primary reason for this appearing to be the lack of resources to support such an organization.

Another question asked whether there should be an annual conference. Twenty-

five (89%) of the respondents favoured having an annual conference. Other decisions regarding the conference were: that the short paper, long discussion format would be retained; that the conference would be open to all PA members, associates, affiliates and to a limited number of invited guests; that each conference would focus on a wide variety of issues as opposed to one specific topic; that conference presentations would be refereed; that the location of the conferences would vary within Europe; and, as a major priority, that conference costs would be kept as low as possible.

Other questions posed concerned fund-raising, public relations, inter-lab research, and publications. With regard to fund-raising, 10 (36%) of the respondents were in favour of conducting fund-raising projects, but only one was willing to help with the organization and administration of such projects. It was ultimately decided that fund-raising activities were outside the scope of an informal organization with an annually rotating committee. However, it was stated at the business meeting following the second Euro-PA conference, that members would make an effort to keep one another informed of possible funding sources, and that this would continue to be a topic of discussion. Indeed, an informative presentation on how to pursue funding for research projects had already been presented at the conference (Parker, 1989) (for further information on the second Euro-PA conference, see Watt, 1990).

The issue of public relations is complex given the many differences that exist among the European countries' lay populations. Whereas there is a generally high level of lay interest in parapsychology in Europe, it is especially marked in some countries, with the Netherlands possessing arguably the greatest degree of public interest. Other countries are confronted by a most vociferous sceptical community, the former West Germany being a notable example (the amount of interest, sceptical or otherwise, in what was previously known as East Germany has yet to be determined). Much of the discussion

regarding public relations focused on the interest accorded our field by the lay population. To address and support this population, several members were in favour of supporting a popular journal where formally conducted research, including that published in our professional journals, would be re-written in a manner accessible to laypeople. Here again, language difficulties intervened. It was feared that an English language journal would not address the needs of many non-English speaking laypeople, and producing such a journal in several languages was obviously beyond the means of Euro-PA members. In the end, it was decided that the pursuing of both public relations and publication activities were beyond the scope of the Euro-PA members. However, with regard to professional publications, a motion to support the *European Journal of Parapsychology* by publishing research in it was strongly carried at the business meeting.

Twenty-one (75%) of the respondents thought that inter-laboratory research projects should be a priority of the Euro-PA, with 11 indicating interest in participating in such projects. Supporting this idea is difficult due to many reasons previously discussed (e.g., lack of labs, distance between researchers, and lack of funds). Nonetheless, means of making inter-lab or inter-researcher projects more of a reality continues to be a topic for active discussion, and, as will be shortly discussed, some strides are being made in that direction.

The question of language was not raised in the questionnaire, because prior to holding the first conference, it was decided that English would be spoken at the conference; indeed, all communications about the conference, and the Euro-PA in general, have been in English. The reason for this is simply that it is the language of which the greatest number of Euro-PA members have at least some knowledge. Yet, some of our members, even those who are multi-lingual, find communication in English very difficult. And, as previously mentioned, not only is it difficult for some to express themselves in English, but it can also be difficult for others to understand them, espe-

cially those for whom English is not their first language. Also, there have been a few instances in which language difficulties have dissuaded researchers from attending the Euro-PA conferences. Furthermore, it has been evident during the discussion periods, that some conference participants find communicating their ideas in English problematic, and they are therefore less active in the discussions. The possibility of hiring translators was raised, and rejected for obvious financial reasons. Thus, for lack of a better alternative, English has been adopted as the language of the Euro-PA, although it offers a less than ideal solution to the language problem.

Also, language problems may be a contributing factor to an observed North/South divide that occurred at the first 2 Euro-PA conferences (1988 and 1989), both of which were held in the Netherlands. Most conference participants were from northern European countries, most notably Britain, Germany, and the Netherlands, whereas neither conference was attended by anyone living in Spain or Italy. There are many possible reasons other than language differences for this apparent divide, such as differing approaches, interests, standards, and the more northerly-oriented location of the first 2 Euro-PAs.

Partly as an attempt to address a few of these North-South divide possibilities, the 1990 Euro-PA was held in France, a country that occupies a central location having boundaries with both northern and southern European countries. Although this particular conference did not attract any Italian or Spanish parapsychologists, there were a large number of French researchers present, who had not attended the earlier 2 conferences. Language problems were again encountered, because many of the French had considerable difficulty expressing themselves in English and in understanding the presentations of others. Others, in turn, were not fluent in French. The conference organizer, Mario Varvoglis, helped by providing simultaneous translations upon occasion, but this was an arduous, time-consuming task and provided only a partial solution to the problem. Also,

considerable differences were apparent at the conference within the approaches taken to parapsychology by different French researchers. Furthermore, most of these approaches differed from those that are typically found at PA conventions, and at the previous 2 Euro-PA conferences. These factors may suggest that language, location, and differing interests and approaches may all be contributing to the apparent North-South divide.

The principal problem facing European researchers has been identified as a lack of funding. The Euro-PA has not been able to find a direct answer to this problem, and, as discussed above, given its present (lack of) organization, it is not currently in a position to do so. Thus far, the most the Euro-PA has been able to accomplish is to host very low-cost conferences, which makes the conferences more accessible to its members. Yet there are still potential Euro-PA participants who, not living on the continental mainland, find travel expenses a prohibitive factor. An example of one means of assisting those who cannot afford travel expenses is provided by the PA, who currently allocate approximately \$2,000 per annum for travel grants to enable their overseas members and associates to attend their conventions. At the 1990 Euro-PA a similar scheme was approved, which would be funded by charging all conference participants a minimal fee on top of that required to meet conference expenses.

However, there are several indirect ways in which the Euro-PA may have a positive effect upon the European funding situation. One such means involves the Euro-PA's decision to invite researchers from other fields to our conferences. Parapsychology is an interdisciplinary field, and through increased interaction with those from other related fields, there may arise joint research projects that examine issues of concern to both parapsychology and to other disciplines. Such projects may have better funding opportunities than do projects concerned solely with parapsychological issues.

Another possible way the Euro-PA could indirectly aid the funding situation, is via the increased communication with one another that has resulted from the conferences. Most European countries (those who are members of the European Economic Community, the EEC) are currently involved in the process of combining economic forces, and many of the boundaries, economic and otherwise, that previously separated the EEC countries are vanishing. One outcome of this is that research proposals that involve researchers and institutions in different EEC countries are currently being encouraged by the various governmental bodies who normally finance scientific research, by offering preferential consideration to such joint research proposals. The increased communication between researchers arising from the Euro-PA conferences may encourage the formation of such projects. Also, some speculate that we may also shortly be finding similar funding encouragement (from the EEC or specific governments) to conduct joint research projects with the newly independent Eastern European countries.

Also, at the 1990 Euro-PA, there was considerable discussion about the advisability of reversing the decision, reached at the 1989 conference, not to become a formal organization. Some believe that if we were to become a formal, legal entity, the Euro-PA may be able to reap financial benefits from the new EEC regulations that encourage multi-member-country projects, conferences, and so on. The feasibility of becoming a more formal organization is currently being investigated, and a decision regarding this should be made at the 1992 Euro-PA (to be held in France on 2-4 October).

Language also remains a problem for the Euro-PA, to which no immediate solution is apparent. To this observer, it appears that most of the younger members of the Euro-PA are increasingly fluent in spoken English. Yet an increasing level of fluency in English does not necessarily address many of the problems that Alvarado (1989) noted, because writing in, and translating to and from English can

remain a very difficult and time-consuming task. Indeed, the number of languages spoken by European parapsychologists (and all the problems inherent therein) could increase if we gain new members from Eastern European countries.

In his article, Alvarado (1989) suggests 4 strategies that could minimize the problems raised by language differences, namely: (1) publishing in English the summaries, bibliographies and reviews of works originally published in other languages; (2) English-language journals actively seeking and encouraging publication of the research of foreign-language speakers; (3) developing an active translation policy into English of works previously published in foreign languages; and, (4) efforts on the part of the PA to encourage greater foreign participation. These suggestions were aimed at the 'conventional' communication and collaboration network, and they are all excellent strategies, the implementation of which would benefit all. However, as Alvarado notes, 'possible solutions such as conferences and translations may prove to be expensive and, consequently, beyond the means of groups and individuals' (p. 134). As has previously been discussed, they would certainly be beyond the means of the European parapsychology community.

However, there is one problem area which, in the opinion of this author, the Euro-PA has successfully addressed. It has decreased the isolation of individual researchers and of research groups from each other and, by this means, greatly increased communication and collaboration between individual researchers and also between various research groups. As has been previously stated, as a consequence of the conferences we know one another better on a personal level, and have a much better understanding of each others' ideas and research perspectives. We are able to exchange research ideas and learn of specific developments made by members, who have thus far always expressed a willingness to share ideas, software developments, and so on. Also, it should be noted that this increase in communication has not been

restricted to those who attend the conferences. Through the questionnaire, all Euro-PA members were able to voice an opinion of what they wanted to see develop in European parapsychology. Indeed, over a third of the returned questionnaires (36%) were received from people who have not attended a Euro-PA conference, as of yet. Also, all Euro-PA members are kept abreast of developments via mailings, regardless of whether they attend the conference or return questionnaires.

Another measure of the success of the Euro-PA may be reflected by the growing number of European PA members. As shown in the PA membership directories, between 1986/87 and 1992, the number of European members increased by 21 percent (i.e., from 58 members to 70). This increase contrasts sharply with the decline found in the overall, and more specifically in the US, PA membership. As noted by John Palmer in his President's Message to PA members (Palmer, 1992), the overall membership of the PA has dropped by 10.2 percent during this same general period (1986-1991), with this decrease being due to fewer US members (US membership dropped by 17.6 percent). Furthermore, Europeans are hoping to see more increases in their numbers as new members are attracted from Eastern European countries.

In a further attempt to aid communication, a directory was compiled by the Edinburgh research unit and has been sent to all Euro-PA members. Although similar to the PA Directory, it includes some additional information, such as whether members are interested in conducting research and/or in corresponding with one another, and whether they have other areas of expertise relevant to but outwith parapsychology *per se* about which they would be willing to offer help and advice. It is hoped that by having a separate directory, focused on

European parapsychologists, members will be further encouraged to interact with one another.

Thus, although many of the problems facing European parapsychologists remain unaddressed, the formation of the Euro-PA has helped improve communication and collaboration between researchers. There is still much room for further improvement, and it is hoped that future years will produce greater interaction and see the development of inter-lab research projects. At the least, the Euro-PA has provided us with an opportunity to improve some of the circumstances confronting European parapsychology, and it places us in a better position to take advantage of other opportunities which may develop.

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Appendix

The Euro-PA Questionnaire and
Summary of Responses¹

Name:

Country:

When completing this questionnaire, please feel free to write on the back of the sheets, or include other sheets, if you need more space than has been provided for your answers.

1. What should the main Euro-PA goals be? Please tick as many of the following as appropriate:

- (25) An annual conference
- (7) I would be willing to help with the organization and administration of the conference
- (10) Fund-raising
 - (1) I would be willing to help with the organization and administration of fund-raising projects
- (18) Public relations/publications
- (15) I would be willing to help with public relations
- (21) Inter-lab research projects
- (11) I would be interested in participating in inter-lab research projects

Other (please specify):

- (4) generally supporting parapsychological or interdisciplinary research
- (1) education (workshops, etc.)
- (1) scientific recognition of European parapsychology
- (3) contact with each other

2. What type of administration or coordination would be needed to achieve these goals?

a. Do you think there should be just one central Euro-PA committee, or different committees for specific areas (conference committee, public relations committee, etc.)? If so, please specify

¹The numbers in parenthesis before each answer show the number of respondents who agreed with that answer. As mentioned earlier, 58 questionnaires were sent out and 28 were returned.

which committees you think should be established.

- (17) one central committee
- (7) few smaller committees
- (1) minimum possible committees

b. Do you have any other suggestions as to means of organizing the administration and coordination of the Euro-PA? How formal an organization do you think we need? Should it be organized by country as well?

- (1) formal organisation similar to PA
- (3) formal European branch of PA
- (5) against a formal organisation
- (7) semi-formal organisation
- (12) against organising by country
 - (1) for organising by country
 - (1) affiliation with other scientific bodies

c. How should we attempt to ensure rotation of responsibilities whilst maintaining continuity? Should we have an election at each annual meeting?

- (16) in favour of annual election
- (2) infrequent election
- (2) postal vote
- (6) voluntary duties
- (1) decide Euro-PA aims first

3. Would you normally be interested in attending an annual Euro-PA conference?

- (23) Yes
- (2) No

If yes:

a. Please specify what the presentation format should be for the conference (e.g., short papers with long discussion periods, formal papers with shorter discussion periods, etc.). Should presentations be refereed?

- (18) short papers, long discussion
- (3) long papers, short discussion
- (4) flexible format
- (10) in favour of refereed papers
- (5) against refereed papers

b. Please express your views on whether the number of [people attending] any given conference should be limited, whether non-PA members should attend, whether any

restrictions should be applied to who makes presentations and/or enters into any discussion periods, and any guidelines which should be used in deciding the above.

Attendance?

- (6) PA members only
- (14) PA members plus guests/ invited Speakers?
- (4) anyone can attend

Present [papers]?

- (4) no restrictions
- (2) PA present [papers], guests attend
- (3) firm chairmanship

c. Would you like each conference to be centred around a specific problem area of parapsychology? If so, please specify different areas which you would like addressed by such a conference.

- (13) against
- (4) partly/occasionally
- (2) specific topic linked to a general theme
- (1) healing

d. Should the conference be held in the same (centrally located) country each year, or should its location vary between different countries? Please list, in order of your preference, in what countries you would like the conference to be held. Also specify any countries which are located at such a distance from you that you would be unlikely to attend the conference if it were to be held there.

- (15) varying locations
- (7) central locations

Preferred Countries:

- (3) Holland, (1) England, (1) France,
- (1) Italy

e. Should low cost be a priority in deciding the conference venue or would you prefer more expensive conference venues, facilities, etc.?

- (18) in favour of low cost
- (2) low to medium cost

f. Please list in what months of the year it would be most convenient for you to attend the conference, and when it would be least convenient:

Opinion very varied, but generally May to October most favoured, November to April

least favoured, with May and October the two most popular months.

4. If you think that fund-raising should be a priority of the Euro-PA, please provide further information as to any fund-raising strategies and/or projects which you think may be successful.

(5) against fund-raising as a priority Policy?

(2) Euro-PA support individual fund-raising activities, by letters of support or coordinating efforts to minimise interference

(1) against use of professional fund-raisers

(1) ask successful fund-raisers

Projects?

(1) raffle of videos, RNGs, etc.

(1) seek practical spin-offs in applied psychology

(1) seek EEC support

(2) ask international companies for support

(1) use ESP/PK for small-scale gambling

5. Please provide further information regarding what, if any, public relations activities you think the Euro-PA should pursue. Also, it has been suggested that the general public may appreciate having a 'journal' available which presented high quality research in a popular, non-technical manner. Do you support this idea, and if so, would you be willing to contribute popular versions of any technical articles you may publish in other professional journals? As such a journal would need to be published in different languages according to country, would you be willing to help with the translation, editorial, and/or administrative work involved in producing such a journal in your country?

Publicity?

(3) occasional public/press releases by Euro-PA

(2) use existing media

Popular Journal?

(8) in favour

(11) against

(7) volunteer to help write or produce

Other?

(1) help translate popular books

(1) produce six-monthly newsletter

THE EURO-PA

6. Presently, the *European Journal of Parapsychology (EJP)* is the primary technically-oriented research journal published in Europe which is aimed at an exclusively professional parapsychological audience. (We now have the *Journal of Theoretical Parapsychology* as well.) The future of this journal is currently uncertain. Do you feel that the Euro-PA should be involved in supporting the continuance of *EJP*, or some other similar professionally-oriented journal? Would you be willing to contribute to such a publication as an author, editor, translator, and/or administrator?

Support *EJP*/Technical Journal?

(15) for supporting *EJP* or similar technical journal

(9) volunteer help to write or produce

Other?

(1) combine *EJP* and *JSPR* resources

(1) Euro-PA supports but doesn't run a technical journal

(1) reorient present journals to wider field of abnormal psychology

(1) too many technical journals

(1) *EJP* too dry

7. What other comments do you have?

(1) English should be the official Euro-PA conference language

(1) Euro-PA should seek to affiliate with other scientific and professional bodies

(1) membership of Euro-PA should be through contribution to development of parapsychology and election by council, as PA

(1) Euro-PA members should lower barriers by writing articles for other professions

(1) research would suffer if time devoted to popular journal

(1) too few parapsychologists to allow Euro-PA to be active in public relations, fund-raising, publications

Department of Psychology
University of Edinburgh
7 George Square
Edinburgh EH8 9JZ
SCOTLAND, UK

Améliorer la communication et la collaboration entre parapsychologues européens: L'Euro-PA

Résumé: Les parapsychologues européens font face à un ensemble de problèmes interreliés que vont à l'encontre de l'existence d'un réseau de communication et de collaboration effective entre chercheurs. Les problèmes principaux sont les différences de langues, l'isolation géographique des chercheurs, la baisse du nombre de centres de recherche, et le bas niveau de financement. Afin de combattre certaines de ces difficultés, les chercheurs européens en parapsychologie ont commencé à tenir une conférence annuelle, l'Euro-PA. L'histoire et les buts de l'Euro-PA sont présentés, et son efficacité à surmonter certains de ces problèmes rencontrés par les parapsychologues européens est considérée. Un questionnaire a circulé parmi les membres européens de PA, en vue de recueillir leurs opinions sur une variété de questions concernant les problèmes de communication et de collaboration. Les réponses à ce questionnaire sont présentées.

'Anglo-Saxon' vs. 'Latin' Parapsychology: Behind the Communication Barrier

Mario P. Varvoglis

Laboratoire de Recherche sur les Interactions Psychophysiques, Morsang sur Orge

Abstract: Based on interviews of French-speaking researchers, an attempt is made to determine some of the issues which may contribute to communication and collaboration problems in parapsychology. It is argued that these problems reflect broader issues than just language barriers. American parapsychologists are the most 'successful' of parapsychologists, in terms of organization, recognition, funding, and social standing. Insofar as they are in a leadership position, they are largely responsible for defining the field's subject matter and methods, as well as qualitative standards for experimentation, journal reports, and Parapsychological Association membership. The situation has contributed to the creation of hierarchical, rather than peer-like, relationships within the field, in which 'Anglo-Saxon' parapsychology dominates. This tends to alienate foreign researchers who disagree with some of the priorities or approaches of their American colleagues, and who do not wish to feel inferior to them. It is suggested that, if we truly wish to improve international communication and collaboration, we must come to recognize the socio-economic, cultural and philosophical relativity of our own approach, and thus be more open to divergences in style and philosophy within the field.

Problem? What Problem?

In his *Journal of Parapsychology* paper 'The language barrier in parapsychology' (Alvarado, 1989), Alvarado deplors the low level of communication and collaboration in international parapsychology, citing Americans' limited awareness of research or publications in foreign countries, and foreigners' lack of participation in the PA and in Anglo-Saxon psi journals. He proposes several measures to counteract these trends, including the use of travel grants to encourage broader participation in US conventions, and increased efforts to locate and translate foreign publications.

But while focusing largely upon these 'formal' measures, Alvarado also cautions that more basic cultural and philosophical issues may obstruct quick and easy solutions. In this context, the opening quotes of his article are quite instructive, because they exemplify the divergence in American vs. European perspectives on the status of international collaboration in the field. J.B. Rhine states that there is 'a spirit and vitality in the research that is general and

international and in no sense localized' (Rhine, 1953, p.233) whereas Tenhaeff darkly observes that 'some (English and American authors) seem very chauvinistic and seem to believe that only the researches done in their country are important' (Tenhaeff, cited in L.E. Rhine, 1968, 189-190). Thus, in contrast to Rhine's cheery assessment, Tenhaeff, voicing the point of view of the continent, refers explicitly to 'chauvinism' on the part of the Anglo-Saxon parapsychologists; he seems to be implying that unfamiliarity with foreign works is based on cultural biases and is, hence, suggestive of darker dynamics than mere ignorance.

My own interactions with a number of Europeans active in contemporary parapsychology suggest that the mood in continental Europe has not changed much in the decades since Tenhaeff's statement. Thus, I think that the 'language barrier' is just a facet of the communication problem in parapsychology; indeed, it may be the least significant one. My feeling is that if we seek to address the problem through formal measures alone, without dealing

with deeper issues, we might end up reinforcing, rather than resolving, alienation or mutual intolerance.

So, in this paper I would like to analyse some of the conflicts which may underlie the communication barrier. Toward this end, I compare the situations and mentalities of two groups - American vs. French-speaking - in the hope that this will also clarify issues dividing broader groups in our field ('Anglo-Saxon' vs. 'Latin', or 'Northern' vs. 'Southern'). I must apologize, in advance, for the stereotyping and 'flattening' of individual differences associated with this approach. In order to render my communication manageable and relatively clear, I present global trends which inevitably caricature reality; I hope to be excused for the multiplicity of exceptions to the trends described.

In order to gain some perspective on the French views, I exchanged views with a number of researchers who are specifically familiar with American parapsychology. In most cases, these exchanges were informal, two-way discussions, in which I first presented the theme of the symposium, and then asked individuals to present their opinions on 2 questions: what specific issues, if any, might exist between American (or Anglo-Saxon) and French (or Latin) parapsychologists, and what factors or dynamics may underlie these issues? In two cases, responses to these questions were given by letter, rather than through direct exchange.

In all, I was able to exchange views with 9 researchers: Pierre Janin, Remy Chauvin, Jean Dierkens, Michel Ange Amorim, Christine Hardy, Jean-Remi Deleage, Francois Favre, Yvonne Duplessis, and Yves Lignon. I will offer my own synthesis of what they have said, focusing on a few global areas which, I believe, contribute most to the communication barrier.

Socioeconomic Constraints Upon Research

After a year or two in France, one cannot help but feel that French parapsychology is decades behind its counterpart in the US; indeed, it is not clear if it makes sense to refer to a 'field' of parapsychology in this country. Recognition of scientific parapsychology is very limited, and external support practically non-existent. Research efforts, involving a few isolated investigators dispersed over the country, are largely self-funded, personal affairs. Little distinction is made between parapsychologists, and psychics, clairvoyants or healers: the term 'parapsychologue' can be used liberally by any 'practitioner' who wants to attract clients, and the media further confuse issues by presenting a parapsychologist on the same level with an astrologer, medium, or dowser¹. Predictably, scientists in various fields tend to dismiss as unimaginable the possibility of serious parapsychological research. The situation is so bad, that the French scientific journal of parapsychology is called *Journal de Recherche en Psychotronique* - 'psychotronics' being seen as less provocative a term than 'parapsychology'.

In short, French parapsychology is confronted with a familiar vicious circle. The field is tainted with negative connotations, due to its lack of internal organization or cohesiveness, and its limited means. These negative connotations, in turn, discourage scientists from an open identification with parapsychology, isolate those already active in the field, weaken efforts to

¹Actually, even some parapsychologists here contribute to the confusion. In a recent 'National Congress of Parapsychology' organized by Yves Lignon and his group, a roundtable with the promising title 'Parapsychology and Society' turned out to be on practitioners, and how to take nasty fake psychics to court. I tried to suggest that, given the roundtable's title, we may wish also to address other social issues, more pertinent to our discipline *per se*. Apparently Lignon (who was alone on the table, during this roundtable) did not appreciate the need for such semantic distinctions, and stuck to his own reading of 'Parapsychology and Society'.

organize the field as a distinct discipline, and further remove any chance for funding or respectability.

Why is the situation so 'backward' in France, one of the most developed and progressive countries in Europe? A partial answer, I believe, can be found by considering the socio-economic structure of the French scientific scene. The socialists have been in power for less than a decade, but centralization has a very long tradition in France, and extends beyond social services, utilities, banks, public transport, etc., reaching into the core of the country's intellectual and scientific activity. The national research organization, the Centre National de Recherche Scientifique (CNRS), has a hold on all branches of science, both within universities and in other centres, and essentially constitutes a means for controlling the nature and funding of the scientific enterprise.

Centralized political and socio-economic structures have proven to be a handicap for innovative research; they are tradition-oriented, discouraging bold advances, initiative and change. For example, the universities and (to a lesser degree) the CNRS operate by a kind of 'quota' system, and applying for a position is generally possible only following the retirement of someone from the corresponding post. Even then, approvals must be collected by a seemingly endless review by committee, which of course translates into a preference for known quantities, not for newcomers, and certainly not for 'strange' topics like parapsychology. It must be recalled that the 'rationalist' movement has a very long tradition in France, and is strongly opposed to anything resembling religious, esoteric or occult claims. This is perhaps why efforts explicitly to establish some research within officially approved centres - e.g., the universities - have generally met with insurmountable resistance. Remy Chauvin was unable to get an official parapsychology Chair established, despite the support of one of the most powerful men in French industry and government. My own attempt to enter a university and the

CNRS through the experimental psychology department was unsuccessful. Christine Hardy has some prospects for discreetly establishing some research, in cooperation with some university faculty members; but even if successful, the research would have no immediate access to funds, and would have to remain hidden behind some innocuous-looking departmental 'front'. Yves Lignon, a mathematics instructor, has succeeded in openly maintaining a small psi laboratory for a number of years, at the University of Toulouse; however, the laboratory's existence has never been officially approved from the top, and the university's president openly denies its legitimacy. The survival of this lab would appear to be a paranormal feat, but can perhaps be explained by Lignon's extensive relations in the media and a tacit threat of a scandal, should anything happen to him.

What about less 'formal', privately funded efforts? Although tax-break measures have been instituted to encourage contributions to non-profit organizations, they are still not truly exploited; the French are not as advanced as the Americans in the fine tradition of donations and humanitarian foundations. Thus, research has been largely self-funded, and, invariably, short-term. Christian Moreau, who had been keenly interested in dream telepathy and psi in psychoanalysis, has long since abandoned parapsychology in favour of psychiatry. Pierre Janin, the inventor of the tyroscope, also left the field to pursue his clinical interests full-time. Rene Peoch, who conducted a series of successful anpsi (animal psi) studies with Janin's moving RNG (the tyroscope), has been progressively forced to limit his parapsychological activities and, and to return to his medical practice. Christine Hardy and I, having established a modest laboratory dedicated to computer-RNG research, are feeling the financial pinch, and are wondering how long we can finance our research. Remy Chauvin has managed to get research done, over the years, but he remains quite isolated, and is now forced to act as his

own subject in his experiments, due to his remoteness from major centres.

Besides lacking opportunities for conducting research, either within the system or independently of it, French parapsychology also lacks cohesion; there is no single organization which well-represents the field. The 'Institute Metapsychique International' (IMI), once the well-funded and internationally recognized centre of psychological research, is broke, and plays practically no role in the field today. GERP (Group d'Etude et de Recherche en Parapsychologie), an interdisciplinary reflection group which sustained lively interest in parapsychology through the seventies, had to fold. Its livelihood was too closely tied to a couple of individuals and thus could not be sustained once they decided to move on.

A more recent effort towards organizing the field was undertaken by Yves Lignon's group, and, in particular, Marc Michel, a PhD student at the University of Toulouse. Their 'Organisation pour la Recherche en Psychotronique' (ORP) publishes a scientific parapsychological journal, and has organized several research congresses and work sessions. However, these activities have depended largely upon the efforts and good will of Marc Michel; they are not sure to survive shifts in his life situation.

The Upper Class and All the Rest

In general, then, the socioeconomic conditions in France render parapsychology a marginal, poorly organized activity, with researchers facing great difficulties conducting research, or even establishing the legitimacy and desirability of such research. This, in turn, means small budgets, limited opportunity for cooperation and exchange with others in the field, and, given the language barrier, little exposure to contemporary Anglo-Saxon parapsychology.

By comparison to this situation, the socio-economic conditions for American parapsychologists have been quite favourable: the field is well organized, enjoys a

growing recognition (even by the skeptics), holds regular national and local conventions, involves research activities both in universities and in independent centres, and has concrete, if sometimes shaky, funding opportunities. Similarly - though to a lesser extent - parapsychologists in northern European countries generally have better socio-economic 'status' than those in Latin countries.

Of course, French researchers welcome the relative success of American parapsychology; it is a source of hope and encouragement for them, and constitutes a convenient argument for the legitimacy of their own research. At the same time, the higher 'social status' of American parapsychologists indirectly introduces communication and collaboration problems, insofar as it encourages hierarchical, rather than peer-like relationships. The dynamic seems reminiscent of that between our field, as a whole, and 'establishment science' - only that in the present case it is American parapsychology which is seen as acting as the guardian of scientific purity. Thus the Americans tend to define the field's nature, methods and objectives; inasmuch as they control the PA and the most important journals in the field, they are also in the position of enforcing their point of view. As a result, the French seem forced to choose between adopting the American style of parapsychology, being ignored, or being labelled 'marginal'.

I've discovered that some French researchers prefer to follow their instincts rather than to feel like subordinates to American parapsychology. As mentioned, the ORP of the Toulouse group has been attempting to promote cooperation and exchange between researchers through a series of 'work-sessions'. One of the first topics discussed in these sessions was the organization of a European congress (Euro-Psi), which would serve as a launching point for subsequent cooperative research projects. The objective was eventually to establish a trans-European association of psi researchers, which could eventually legitimise parapsychology.

In response to this, I suggested that the basis for European cooperation in parapsychology may already exist in the form of the EuroPA. I proposed that the French coordinate their efforts with the members of the EuroPA, and added that, insofar as participation in the EuroPA was at that time restricted to PA members, this would be a good occasion for several French researchers to join the PA. As members of the PA, they could more effectively elicit the cooperation of other European parapsychologists, while at the same time establishing a more prominent French presence in the internationally recognized organization of scientific parapsychology.

I proposed this during two different work sessions, and both times the reactions ranged from cool to hostile. The arguments against my suggestion were at no point clearly phrased or explicated. Rather, from a number of side comments and snide remarks, I gathered that these researchers simply had no desire to join the PA, to adhere to what they perceived as an American (rather than international) organization. Surprisingly, the most negative responses came not from the clinicians or anthropologists, but from those whose work falls most clearly within the Rhinean tradition of experimental parapsychology.

My initial interpretation of all this was that I had stumbled upon a clear cut case of territorialism. I, a foreigner (worse, an American) had invaded the territory of French parapsychologists, and, by suggesting that they join the PA and EuroPA, was challenging their claim to fame as leaders in European parapsychology. I still think in retrospect this interpretation is valid. However, I have also had a glimpse of what it's like to be in the shoes of a foreigner seeking to join the PA. This experience made me realize that some tacit criteria underlie the explicit PA admission policies, allowing for discrimination against candidates who come from another culture, and have published works outside the officially sanctioned Anglo-Saxon journals. Insofar as admission to the PA is controlled by a committee largely representative of American parapsychology, it is easy to see

how foreigners can come to the view that the PA is in fact an American, rather than international, organization. It is also quite understandable that they would react aggressively when asked to seek PA membership. Why should individuals prominent in their own country risk a humiliating rejection?

Of course, it is possible to defend the need for strict criteria for PA membership, as well as the more general need for strong leadership (hence, 'hierarchical' relationships) within the field. Given differences in recognition, in numbers, and in funding, it could be argued that American parapsychology is, *de facto*, the leader in the field. Money translates into improved research conditions, better equipment, more talent, more extensive exchanges with other scientists, and so forth. Consequently, one could say that, like it or not, the Americans have outstripped other researchers in competence and authority, and have the responsibility of promoting the field as they see fit; in the interest of the field's progress they must exclude those who don't measure up to the defined standards.

Needless to say, these kinds of arguments are hardly apt to promote communication and collaboration. More importantly, they underestimate the cultural relativity involved in our perceptions of 'competence' and 'progress'. The criteria as to what constitutes valid and significant psi research, and, hence, as to who is and who isn't a 'good' parapsychologist, are not universally agreed upon. To the extent to which French researchers view the priorities in a way different from the Americans, they are bound to resent the message that the 'American model' is the only one acceptable. But the issues here clearly transcend socio-economic considerations, and touch upon much thornier cultural, psychological and philosophical divergences.

Cultural and Psychological Issues

I mentioned earlier that heavy, centralized bureaucracies in France may impede the evolution of scientific inquiry and

research. However, in complement to this bureaucracy, French society is characterized by a tremendous individualism. People are in an informal but permanent struggle against the establishment, and will go to great lengths to 'beat the system', even when they don't have to.

This anti-conformism is also apparent in the intellectual scene; passion and expressiveness pervade the entire culture, and not just the arts. Of course, when it comes to science, much is necessarily built upon the modest and persistent work of technicians and specialists. And, as everywhere else in the world, most scientists are conservative in nature and suspicious of upstarts. Yet, the French pride themselves above all as creators, not as technicians or specialists; the image of the free thinker is far more of an inspiration than that of the systematic scientist. This is particularly true now, as the 'New Age' vogue has pulled a number of scientists from their conventional tasks and thrust them into Kuhnian shifts and currents.

Apart from the centrality of individualism and creativity in French culture, also of relevance is the trait of ethno-centricism. As in other Mediterranean countries, nationalistic pride is pronounced; the French do not take kindly to the idea that they may be playing second fiddle to someone else. Of course, their self-image as independent and superior was challenged by the enormous economic power and political influence of the US in post-war Europe. But along with other European civilizations, the French have increasingly sought to distance themselves from complete loyalty to the US, and to reaffirm their distinct identity. This tendency has been reinforced by the anti-conformist and anti-authoritarian sentiments described above, since the US has often been perceived as an over-dominating economic and military force.

What does all this have to do with communication and cooperation problems in parapsychology? I think that a number of our problems within the field may have little to do with parapsychology *per se*, and may instead be strictly related to such cultural issues. The traits of our culture rub

off on all of us, and, inevitably, affect the kinds of relationships we sustain with those from other cultures.

For example, the individualist and anti-conformist traits of the French imply a desire to remain free, distinct, and unclassifiable; and, hence, resistant toward invitations to join groups and organizations. Such cultural traits may have been one of the main reasons why the French have had difficulty organizing parapsychology in their own country. Coupled with the slightly paranoid sentiments *vis-a-vis* American chauvinism (or imperialism), these traits probably induce considerable psychological blocks *vis-a-vis* organizations such as the PA. But additionally, individualist and anti-conformist feelings could also lead to resistance toward methods, rules and standards 'imported' from American parapsychology, especially when these seem out of sync with Latin values and traits.

American parapsychologists spend much energy organizing the field, defining its subject matter and standardizing research methods and reporting styles. A good chunk of their time may also be spent on formal budget proposals, annual reports, or public-relations activities (including responding to irresponsible critics). All these activities move the field toward planned and systematic, rather than spontaneous or improvisational research programmes. It is a trend which is entirely justified, inasmuch as the goal is to render parapsychology more 'professional', and thus more apt to be welcomed by the scientific establishment. But it is a trend which has its price, as well; in other cultures, researchers may see little reason to orient themselves in the same direction. The contingencies and constraints are not the same for those who work in isolation, without budget proposals, annual reports, or Csicops axing the doors down. There may therefore be little concern with standardization, replicability, or other marks of professionalism. The feeling might be that, when it comes to psi research, the top priority is to explore new directions creatively, even

at the risk of committing errors or wandering down some blind paths.

Of course, to the extent to which American parapsychology is 'calling the shots', the French (or Latin) parapsychologist is bound to be penalized for not following; inevitably, this leads to a widening of the communication gap. An example here is provided by Remy Chauvin, who several years back submitted an article to the *Journal of Parapsychology (JP)*, reporting apparent PK effects upon water congelation. Given the centrality of water to living organisms, Chauvin considered this a potentially important finding, worthy of replication and further investigation. However, it seems that the *JP* did not appreciate the 'manual' measurement techniques used, and wondered why computer-controlled data collection and data processing had not been adopted instead. To Chauvin, who had spent many months devising his apparatus and collecting results, this demand for computer-control seemed excessive and irrelevant; not everybody is equally able to utilize computers, and computers are by no means necessary for good research. He ended up publishing the article in the *Journal of the Society for Psychological Research*.

In my interviews with Chauvin and some other French researchers, I had the impression that there is some rebelliousness *vis-a-vis* the American criteria for good psi research, or acceptable reporting styles; there is a desire to find approaches involving complementary values and priorities. These feelings were of interest to me, because they reminded me of similar feelings which underlie a movement called 'Latin management'. As described to me by a well-known business consultant, it is an attempt to gear French managerial styles away from the dominant Anglo-Saxon or American models, and to cultivate styles which are more consistent with Mediterranean values and traditions. I thus wonder whether some of the communication issues in parapsychology are part of a larger development; the emergence of a 'Latin science', emphasizing individuality, expressiveness, personal implication, and human

interaction, rather than standardization, detachment, objectivity, and formal means for regulating exchanges.

A Paradigm Conflict?

Since the writings of Kuhn, we have become increasingly sensitized to the central role of tacit motives, beliefs and conceptual frameworks in scientific research. Such tacit factors define the questions we consider meaningful or significant, the tools and procedures we utilize to address them, and the responses we are likely to find. When frameworks with different ontological or epistemological premises collide, then the minimum we can expect is a lack of communication and collaboration between the groups involved.

One of the most obvious obstacles to collaboration in parapsychology is the metaphysical 'split' between interactionist-dualism and monism. Many, if not most American parapsychologists are tacitly or explicitly committed to dualism. Even recent theories, inspired by quantum physics, retain a distinction between the observing consciousness and matter. By contrast, the French, who have been struggling to rid themselves of their Cartesian heritage, are generally hostile toward dualistic concepts, and much more prone toward monistic world-views, whether materialistic or idealistic in nature. Thus, in seeking to explain psi phenomena, they are more likely than Americans to use concepts often found in the East or in Russian parapsychology (such as 'bio-fields' or 'bioplasma') and to explore the possibility of detecting 'psi-energies'.

Inevitably, of course, the differing world-views lead to clashes. To many Europeans and Russians, dualism seems reactionary, like a left-over from the days of spiritualism. On the other hand, to most American parapsychologists, concepts like 'psi energies', and the work associated with these concepts, seem rather 'marginal'. But the 2 views do not have equal opportunities of expression; while research consistent with the dualistic viewpoint receives much coverage, some feel that the Americans are

prone to ignore work which is more consistent with a monistic view. Yvonne Duplessis, for example, complains that her work on dermo-optic perception did not receive the attention it deserved, even though it is conspicuously relevant to a substantial amount of psi research (i.e., clairvoyance tasks with sealed envelopes). When Carroll Nash sought to explore protocols analogous to those of Duplessis, he concluded that his results pointed to something other than psi phenomena; the results were 'too good' to be based upon psi. Perhaps this is true. But to those who assume that psi is a subtle physical energy, rather than a 'pure' mental phenomenon, this attitude seems incomprehensible. It translates to abandoning a promising research lead, in favour of pre-established assumptions about the nature of psi; and it also implies the perpetuation of parapsychology's isolation from 'normal' science.

Another issue which may act as a divisive force in the field is the very ancient and persistent confrontation between two epistemological frameworks: empiricism and rationalism. The empiricist approximates truth by accumulating more and more data, relying upon these to diminish the 'interference' of erroneous ideas and conceptions; his or her preoccupation with methodological purity and replication reflects this search for 'hard facts'. By contrast, the rationalist seeks to approximate truth by constructing increasingly compelling theoretical structures. His or her focus is upon formal systems or semantics, and he or she is preoccupied far more with the coherence of thought than its correspondence with data.

In the US, parapsychology is clearly rooted in the empiricist tradition. Rhinean methods have been inspired largely by behaviourism, and thus, indirectly, by positivism; both extreme expressions of the empiricist tradition. And parapsychology in the U.S. continues to be modelled largely after experimental psychology, emphasizing systematic data collection and methodological purity, and showing restraint in modelling and theorizing. Similarly, the trend toward atheoretical terminology,

(e.g., references to 'anomalies', rather than psi) reflects the data-orientation of American parapsychology.

By contrast, French parapsychologists, while certainly empiricists, are nevertheless operating within a culture with a long rationalist tradition. Positivism has never been warmly received in France, and it is unlikely that a purely behaviouristic approach to psi phenomena could ever really take root there. Not surprisingly, the concept of an atheoretical 'anomaly' is nearly intolerable; it seems preferable to start out with some theoretical framework from the outset, and view the facts as part of a meaningful grid. The intellectual climate is such as to encourage ambitious theories, and innovative conceptual efforts; it is less important that these be based on many facts, than that they be internally coherent and consistent with their own premises.

This divergence in epistemological outlooks between Americans and the French could help clarify, though by no means resolve, some disagreements regarding research methods and priorities. American parapsychologists' preoccupation with polished experimental protocols and near-perfect controls are consistent with the empiricist goal of seeking out 'pure' data; facts that are so elementary and certain that they cannot be said to be distorted by subjective opinion or error. It is assumed that only such hard data can persuade the skeptics of the reality of psi.

On the other hand, in the rationalist tradition, there can be no such thing as elementary data, independent of premises and frameworks. Data are not ends in themselves, but only means intended to ascertain or clarify an existing theory or model. An 'anomaly', even if well-demonstrated, is uninteresting if not embedded in a conceptual context that lends it meaning. From this point of view, methodological sophistication assuring data purity, though laudable in terms of public relations (i.e., for skeptics), is not the most important priority. At this point, those influenced by rationalist perspectives feel that there is no need for more experimental 'hard data';

what is needed is the integration of all available clues in search of an understanding of the nature of psi.

The climate in the US is such as to encourage specialization of 'solid' data, even if the effects observed are near the vanishing point. The climate in France, on the other hand, is likely to reinforce theoretical, phenomenological or field work, and a courting after risky 'macro' effects, through studies with gifted subjects, clinical case studies, anthropological and ethnological investigations, and so on.

There is little doubt that the experimental approach is more likely to gain us favours with hard-headed scientific audiences, and an entry into establishment science; the earlier mentioned successes of American parapsychologists attest to this. However, the more adventurous approaches have their own appeal. It may be these which, in some wild chase over the landscape, will unveil the true forms behind the walls of data, and satisfy our thirst for meaning.

Conclusion

We are all drawn to the ideas of communication and collaboration. Communication implies enrichment, expansion of knowledge, broadening of vision; collaboration suggests the warmth of shared creativity, and promises levels of achievement beyond the reach of isolated individuals. In our field, especially, plagued as it is by chronic funding problems and endless battles for recognition, communication and collaboration are necessities, not just luxuries. But neither communication nor collaboration 'just happen', automatically; they must be actively pursued and reinforced. This is especially true when geographical, linguistic, political, cultural, or philosophical factors obscure and obstruct sharing and interchange.

I think it is clear, at this point, that differences in parapsychology are inevitable and that, at this stage in the development of the field, we cannot specify priorities, objectives and methods that are universally

preferable over other ones. Our criteria for 'good science' reflect specific assumptions and values, which in turn may be culturally bound, or the result of a particular historical tradition. Consequently, in reflecting upon how better to communicate, it is important that we appreciate the relativity of our own perspective, and develop a tolerance, and respect for, differences. Once we accept that all approaches probably have some strengths, and some weaknesses, we may begin to exchange more freely and make room for collaboration. After all, to work together, we don't really need to speak the same language; we just need to understand what the other is saying.

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Postscript

Two years after writing this paper, I feel it necessary to introduce some qualifications.

For one thing, the situation of American parapsychology has changed considerably since the 1980s: a number of major laboratories have closed down, others are barely keeping afloat, and several parapsychologists are no longer actively involved in the field. Given the difficult circumstances that we are all facing, it now seems somewhat out of place to speak of 'status' differences within the field, or to bemoan its hierarchical nature. Even in the US, the field is far too small and fragile.

Also, despite apologies and caveats in the paper, I must further stress that in adopting a sociological, rather than psychological perspective, the presentation inevitably gives an unbalanced view of the communication or cooperation issues of the

field. Any analysis of such issues through broad-based, relativistic metrics (culture, socioeconomic conditions, etc.) must be complemented by a focus upon the personalities and skills of particular individuals in the field. For example, after several years of participation in the French parapsychological scene, I am persuaded that the current situation here can be traced largely to the conduct of just one or two highly visible *personnages* in the field. Thus, a case-by-

case analysis of the main figures of parapsychology in a given country may go a long way towards explaining the local status of the field, and the dynamics of exchange with researchers elsewhere.

Laboratoire de Recherche sur les
Interactions Psychophysiques
51 bis, Av P.V. Couturier
91390 Morsang sur Orge
FRANCE

Parapsychologie - 'anglo-saxon' contre 'latin': derrière la barrière de communication

Résumé: Essai de détermination, sur la base d'interviews de chercheurs francophones, de certains facteurs pouvant amener des problèmes de communication et de collaboration en parapsychologie. Il est proposé que ces problèmes sont dus à des dynamiques plus profondes que de simples différences linguistiques. S'étant montré les plus efficaces en terme d'organisation, de reconnaissance scientifique, de financement et de statut social, les parapsychologues américains sont, de fait, les 'leaders' du champ, largement responsables de la définition du domaine de recherche, de ses méthodes, et des critères de sélection en ce qui concerne les standards d'expérimentation, les articles scientifiques, et l'admission dans la Parapsychological Association. Cette situation tend à créer des relations de type hiérarchique plutôt qu'égalitaire, dans lesquelles le point de vue anglo-saxon domine. Cela a pour effet d'aliéner ceux parmi les chercheurs étrangers qui ne sont pas complètement en accord avec les priorités ou les approches de leurs collègues américains et qui ne veulent pas se sentir en position d'infériorité. Il est suggéré, si nous voulons vraiment améliorer la communication et la collaboration internationales, que nous devrions reconnaître les relativités socio-économique, culturelle et paradigmatique de nos propres approches, et être plus ouverts aux différences de style et de philosophie.

Parapsychology in Brazil: Relations with non-Brazilian Researchers and the Context of Brazilian Culture

David J. Hess

Rensselaer Polytechnic Institute, Troy, NY

Abstract: In Brazil there is no community of secular, academic parapsychologists. Instead, what is called parapsychology is largely the product of allegiances to Catholicism or Spiritism. The essay surveys Catholic and Spiritist parapsychology, then discusses the relations between PA and Brazilian parapsychologists. These relations may be viewed as exchanges in which the legitimacy of First-World science (here represented as parapsychology) is traded for access to Brazilian psychics or paid trips to Brazilian conferences. Alternative arrangements are considered.

This essay will review some of my findings on the historical, social, and cultural aspects of parapsychology and Spiritism in Brazil (Hess 1987a, 1987b, 1987c, and 1991a), and it will present some suggestions for researchers from North America, Europe, and other countries who wish to do work in Brazil. The paper is divided into two sections: an overview of Brazilian parapsychology and a discussion of the relations between Brazilian parapsychologists and those from other countries.

Parapsychology in Brazil

Brazil does not have a coherent community of academic parapsychologists, and there are few, if any, people in Brazil who research and publish at the standards of the Parapsychological Association. Instead, what is called 'parapsychology' in Brazil is largely defined by rival groups of Catholic and Spiritist (Kardecist) intellectuals.

Foremost among the Catholics are the conservative Franciscan Boaventura Kloppenburg, currently a bishop in Bahia, and a group of Jesuits who are located primarily in São Paulo and southern Brazil. Kloppenburg has written numerous tracts against Kardecian Spiritism, Umbanda, and various other religious or philosophical movements that practice spirit mediumship. Most of his work was published before Vatican II, but an examination of his

work since Vatican II shows that his rhetoric may be somewhat more restrained but his general position has not changed. His book *Spiritism in Brazil* (1960) included a discussion of parapsychology, which was used mainly to disprove Spiritism. When I spoke with him in 1986, he said he was preparing a new edition of the book, but he was considering dropping out the section on parapsychology, and he suggested that one reason was that he had grown more critical and skeptical of parapsychology, or at least its utility for his purposes.

There is also a group of Jesuits associated with the publisher Loyola and the former Latin American Centre for Parapsychology (CLAP) of the Anchieta College of São Paulo. When I visited the college in 1983, everything but the Centre was closed down, and its leader, Padre Oscar González Quevedo, was under a prohibition to give interviews or speak in public. The Centre had a large library, and in previous years it had published the *Revista de Parapsicologia*. As far as I was able to ascertain, the Centre and College were closed for economic reasons. González Quevedo (or, as he is known in Brazil, Padre Quevedo) was under prohibition because he had published a book that used parapsychology and psychology to criticize the demonic interpretation of possession. The book violated Church doctrine, and most copies of the book were collected and burned. When I returned in 1985, Quevedo was at a

Catholic high school in São Paulo, and he was unable to grant me an interview. On my return trip in 1988, I learned that he had since been released from his prohibition to speak.

Quevedo has written a number of books, some of which are extremely polemical attacks on Spiritism, Umbanda, and the Afro-Brazilian religions (e.g., 1978). Others are more didactic: they are expository accounts of what parapsychology is, of course with a careful sifting through the sieve of Catholic doctrine (1964, 1968, 1974). Survival research and reincarnation - two areas which would support the Spiritist viewpoint - do not fare very well, and non-spiritic interpretations of mediumistic phenomena are favoured. Quevedo's parapsychology books emphasize studies of mediums and spontaneous case research rather than experimental parapsychology, and he generally rehearses the old literature rather than publish new and original research. Where he does discuss Brazilian mediums (e.g., 1978), it is generally to debunk the claims of Spiritists. In a sense, Quevedo is a Brazilian Randi; Quevedo is a debunker who would himself be debunked in the context of American culture.

Another important Jesuit is Padre Edvino Friderichs, who lives on the campus of a Jesuit high school in Porto Alegre. He has published many expository accounts of parapsychology and psychology directed towards lay readers (e.g., 1979), and he has earned a reputation as a therapist who sometimes treats poltergeist cases. One of his many books includes some case histories, most of which are culled from newspaper clippings, some of which reflect first-hand interviews, but none of which goes into detailed psychological analysis of family dynamics (1980). Friderichs is considerably more ecumenical than Quevedo and Kloppenburg, and he is more concerned with using parapsychology to teach Brazilian laypeople the error of their 'superstitions' than to attack Umbandists and Spiritists.

A review of the back issues of the Catholic *Revista de Parapsicologia* revealed little careful empirical research. I found

only one publication of an experiment, and it appeared to suffer from serious design flaws; instead, the case studies of poltergeists and other popular psychic experiences were more interesting, and in some cases the accounts gave some information on family dynamics.

The Spiritist intellectual and former engineer Hernani Guimãraes Andrade is probably the only Brazilian to have produced and published a body of original case studies. His theoretical research (1983, 1984a, 1986) is probably too influenced by Spiritist assumptions to attract the average member of the Parapsychological Association, but he has written Brazil's only textbook on experimental parapsychology (1984b) and he has produced some very well-researched studies of poltergeists (1988a) and reincarnation cases (1988b) along Stevensonian lines, all of which conclude that alternative explanations (such as super-psi or dissociation) are less convincing than the Spiritist explanation. In another paper, I have discussed how the spirit interpretation of events such as poltergeists is related to Brazilian cultural values of hierarchy and personalism, just as the RSPK interpretation is congruent with the modern Western values related to individualism (Hess, 1989).

One also finds some interest in psychical research among the members of AMESP, the Spiritist Medical Association of São Paulo; in the Spiritist college in Curitiba, where there is an institute of psychobiophysics (Andrade's term for parapsychology); and among relatively isolated Spiritist intellectuals across the country (see Sobral, 1984; Souza, 1985; Souza and Deitos, 1980). In 1985, AMESP co-sponsored the First International Congress of Alternative Therapies, and it has also published the *Medico-Spiritist Bulletin*. The articles in the issues I have seen are generally rehearsals of Spiritist doctrine, but occasionally there is some empirical research, such as some case histories by a Spiritist psychiatrist (Lyra, 1984).

That is more or less the extent of parapsychological research in Brazil in the 1980s. The medical profession and the

universities have shown little formal interest in research into the widespread claims of psychical phenomena, and they have generated little empirical research. Exceptions include some experimental research on precognition (Lessa, 1975) and a psychoanalytic study of telepathy (Levy-Júnior, 1970). In general, doctors or scientists who are interested in parapsychology tend to have an allegiance either to the Catholic or Spiritist position. An important exception is the work of Osmard Andrade Faria, a medical doctor and hypnosis specialist who has authored a textbook on parapsychology (1981; see Hess, 1991a). There is also a nominally neutral parapsychology organization, and from what I saw of the Rio-based Brazilian Association of Parapsychology (ABRAP), its members were not all Spiritists, but many of the ones I spoke with believed in spirits and extraterrestrials. In any case, the organization held classes and meetings but did little research. André Percia de Carvalho, a psychology student in Rio de Janeiro who has also studied at FRNM, is one of the few parapsychology researchers I met in Brazil who is interested in a more nonaligned, empirical research.

Relations Between Brazilian and PA Parapsychology

The tendency for Brazilian parapsychology to be divided into rival camps is an expression of a deeper division in Brazilian society between the Catholic Church and the Afro-Brazilian religions (see Hess, 1987a), and it is a legacy of the colonial experience involving the enslavement of Africans by Europeans. Spiritists, who tend to be more of a European background and more middle class, have sometimes participated in the oppression of African-Brazilian religions, and at other times they have been victims of the same forces of oppression (see Hess, 1987b). For example, Spiritists suffered during the 1930s and most of the 1940s, when Brazil was controlled by Getúlio Vargas, who after 1936 assumed dictatorial powers. Vargas worked closely with Cardinal Leme, and this partnership

was at least partially responsible for closing many Spiritist centers and African-Brazilian temples and jailing their leaders.

Current debates on parapsychology in Brazil therefore must be situated in this broader historical and cultural context. As I have discussed in *Spirits and Scientists* (1991a), there have been three attempts to get a law passed that would require parapsychology education in the nation's medical schools. At least two of these proposals bore a Spiritist imprint, and the Jesuits have actively attempted to block the proposals. There is also a controversy over the faith healing (*curandeirismo*) law, with Spiritists in favour of a more lenient law and Jesuits opposed to it. After the transition to civilian rule in 1986, Spiritists became actively involved in attempts to reform the *curandeirismo* law.

To label any one type of Brazilian parapsychology 'unscientific' or 'less scientific' than the other could therefore mean playing into the hands of opposing groups. Likewise, developing a working relationship with, or participating in the institutions and conferences of, one type of parapsychology in Brazil could mean legitimating one group at the expense of the other. For example, since anything 'international' or 'First World' in Brazil confers additional status, the participation of PA parapsychologists at Brazilian conferences that represent one group could mean legitimating either Catholic or Spiritist parapsychology at the expense of the other group (see Hess, 1987c).

I have had more dealings with Spiritists than Jesuits, so my observations will be restricted to Spiritists, but I think the same could apply to the Jesuits. Alvarado (1989) has discussed the meaning of language barriers in parapsychology in the context of written communication, but in the context of face-to-face and oral exchanges, the language barrier may involve some complicated manoeuvrings. For example, when Spiritists speak to each other in Portuguese, they think of their own parascience as 'true parapsychology,' whereas the narrower, experimental type of the PA conferences is to them part of the 'materialist' scientific

orthodoxies which they oppose. However, both Spiritists and Catholics will still appropriate PA parapsychology and the older studies of psychical research for their own purposes. They like to have the foreigners do the empirical research (the 'dirty work'), while they sit back and put it into their own philosophical or theological frameworks. Old attitudes of the plantation owner's *noblesse oblige* spirit therefore enter into their way of 'doing science' (see Hess, 1991a, ch. 2).

From my observations, it appears that parapsychologists from the 'exterior' who come to Brazil tend to work more closely with Spiritists or those who maintain friendly relations to the Spiritist movement rather than with Jesuit parapsychologists, probably because Spiritists offer somewhat better access to mediums or others who are making claims of paranormality. Other factors, however, may be involved. For example, Spiritists (or those friendly to Spiritism) have generally been the ones willing to pay for invited lectures at conferences, and in addition those members of the PA who do research or give lectures in Brazil - and that is only a small portion of the PA membership - may come from non-Catholic backgrounds or share survivalist assumptions with the Spiritists. Certainly historical accounts have shown a strong link between Protestantism and the early leaders of psychical research in Britain and the United States.

In any case, as I have pointed out before (1987c), there may be an exchange of access to Third-World mediums for access to First-World status and legitimacy. Both parties may benefit, and while PA parapsychologists privately lament to themselves how unscientific the Spiritists are, Spiritists will privately lament among themselves how materialistic or positivistic the PA parapsychologists are. The language barrier may actually contribute to the exchange by allowing each side to pretend to ignore its differences from the other side.

If researchers speak Portuguese fluently, then they are likely to get long lectures which Spiritists call 'indoctrination'. The Spiritists' theory is that if one listens to only

ten percent of what they say, then they have made some inroads. The practice is not so widespread among Spiritist intellectuals, who are more cosmopolitan, but it is fairly common among the leaders of the local centres, who often control access to mediums who claim to have psychic abilities.

Leaders of local centres are often very happy to be visited by foreign researchers, since these visits legitimate their mediums and their centres. As in the case of conferences, any international participation confers upon the center or medium more credibility and legitimacy. However, once this purpose has been served, there is little interest in or cooperation with the idea of testing the claims of paranormality made by mediums affiliated with the centre. For example, in the case of Dr. Edson Queiroz, whose impresario introduced me to hundreds of waiting lay-people as a 'great scientist from New York,' I found there was no cooperation for or interest in doing follow-up interviews. Instead of controlled experiments, he was more interested in controlling experimenters. This is probably true of other well-known psychics or mediums in Brazil.

I am therefore rather skeptical of the prospects of 'collaboration' between PA and Brazilian 'parapsychologists.' Perhaps it is best *not* to collaborate with Brazilian 'parapsychologists,' and the prospect of flying down to Rio to investigate a psychic or to give a talk at a 'parapsychology' conference may *not* be the best use of scarce resources. Instead, I might suggest two alternative strategies.

As a research strategy, it may be better to bypass the well-known psychics and healers, who in turn are aligned with different religious groups and may have their appointed parapsychology patrons, and instead to go directly to families who believe they are experiencing or even suffering from various kinds of supernatural phenomena. Of course, this strategy depends on developing networks and personal connections, and such networks in turn require fluency in the language and a long exposure to the culture. Without fluency,

one will probably rely on local Spiritists or Catholics as translators, and they will inevitably try to control the kinds of questions one asks. For example, as I found during an interview with a poltergeist family in Puerto Rico during a field trip last summer (I am not very fluent in Spanish), the Spiritist translator interrupted my line of questioning about family dynamics to explain to me that the cause was due to a spirit. However, if one can achieve fluency in the local language, research on claims of sorcery, possession, poltergeists, reincarnation, and so on reveals a great deal about folk illnesses and popular culture in general, and its results will be of interest to anthropologists, clinicians, and others (e.g., Hess, 1991b).

This last point raises a final issue, what one might call the 'paradox of Brazilian parapsychology': the fact that, despite the almost complete absence of empirical research, my impression is that most of the people in the country regard 'parapsychology' with a high degree of respect, much more so than the more 'scientific' parapsychology of North America and Western Europe holds among its populations. That may be simply because Brazilians are a less skeptical people than most North Americans and Western Europeans, but it is also true that in Brazil parapsychology often involves counseling and clinical work, and Brazilians therefore know that parapsychology has some useful role to play in their lives. This possibility raises the interesting question for the social studies of science, where some sociologists (e.g., Pinch, 1979) have questioned whether or not the replicability issue is really what stands between parapsychology's status as a heterodox or orthodox science. Brazilian parapsychology's therapeutic orientation suggests that the legitimacy issue may stand or fall more on 'applicability' than 'replicability,' and this might be something that Brazilians have to teach the relatively 'orthodox' parapsychology of the Parapsychological Association.

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Sci. and Tech. Studies Dept.
Rensselaer Polytechnic Institute
Troy, NY 12180-3590
USA

Parapsychologie au Brésil: Relations entre chercheurs non-Brésiliens et le contexte de la culture brésilienne

Résumé: Au Brésil il n'y a pas de communauté académique séculaire de parapsychologues. A la place, ce qui est appelé parapsychologie est largement le produit d'allégeances au catholicisme ou au spiritisme. On passe en revue la parapsychologie catholique et spirite, puis on examine les relations qu'entretiennent parapsychologues Brésiliens et membres de PA. Ces relations peuvent être vues comme des échanges où la légitimité de la science occidentale (sous le nom de parapsychologie) est récupérée en permettant d'accéder à des sujets psi brésiliens ou en payant des voyages à des conférences au Brésil. Des arrangements alternatifs sont considérés.

Implications and Applications of Laboratory Psi Findings

William G. Braud

Mind Science Foundation, San Antonio, Texas

Abstract: Usually one looks to everyday-life psi experiences for clues about what to explore in the laboratory. In this paper, we reverse the process by looking at what laboratory psi findings might offer to those who wish to apply such findings in everyday life. The extent to which this strategy may be helpful depends upon what is ultimately learned about the 'laws of psi', and this issue is addressed in view of 5 possible scenarios. Next, various physical, physiological, and psychological facilitators and psi-inhibitors are discussed. The psychological facilitators interestingly coalesce as forms of faith, hope, and charity (love). Finally, applications and findings of 2 specific research programmes (biological psychokinesis and psi-blocking) are discussed.

In 1967, Louisa Rhine published a book entitled *ESP in Life and Lab*, in which she described the then-current understanding of the psi process and how that understanding derived from both laboratory investigations and reports of psi occurrences in everyday life (Rhine, 1967). She argued that spontaneous cases of psi in everyday life, although not conclusive in an evidential sense, could be extremely useful in providing insights into the operation of the psi process and in providing suggestions for more evidential work that could be carried out later in the laboratory. With the conference, 'Parapsychology: From Lab to Life', we have come full circle. We were asked to ponder our laboratory findings to learn whether they might yield conclusions or insights that might be applied usefully in the everyday-life setting. Are there suggestions from the lab that might be applied to life?

Part I. A Continuum of Possibilities: 5 Scenarios

It would be unwise to attempt to make general, unqualified predictions about the likely implications or applications of laboratory psi findings. Rather, the particular applications that may become possible would depend importantly upon the specific nature of discoveries made in the laboratory. I can foresee 5 possible scenarios that would arise from different types of discoveries that might be made regarding the essential nature of psi processes.

Scenario A: Psi is a Delusion

This first, and most pessimistic, scenario is included primarily for completeness, because I believe it is extremely unlikely. After decades of research, it could

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be discovered that our psi findings are illusory, that psi is not real, and that we have been victims of a thoroughgoing delusion. A particular flaw, singularly powerful yet exquisitely secretive, may have been latent in all our successful experiments. Alternatively, several sets of weaker, more subtle confounds may have been present in varying mixtures in our experiments, interacting and combining their effects to yield apparently positive outcomes in some studies. In view of the vast number of successful experiments (Honorton, 1985; Honorton et al., 1990; Honorton & Ferrari, 1990; Radin & Ferrari, 1991; Radin & Nelson, 1989; Schechter, 1984) conducted by many different investigators, with very different methodologies, the likelihood of this outcome is exceedingly small. The probability is not zero, however, and we should never completely rule out this scenario, however unlikely it appears to be.

Of course, no psi applications would arise from this scenario. It would be, however, rich in implications; implications that perhaps even extremely cautious investigators are capable of methodological oversights and capable of self-delusion over long time-periods, or implications about the validity of statistical tests, which in turn would have major implications for all areas of science in which similar statistics are used in decision making.

Scenario B: The Reality of Psi is Suppressed

It is entirely possible that laboratory research reveals the reality of psi, but that this knowledge is actively and intentionally suppressed by persons who feel threatened by positive psi findings. If such campaigns should prove successful, the fate of possible psi applications would depend upon which particular social groups are made to discount the psi evidence. If scientists are convinced that psi effects are not real, those interested in the investigation and possible application of psychic processes would be denied an important avenue of legitimation of their work. Alas, in the past and in the present, there has been very little support

of psi research by the scientific community, so things would change very little. There would be a loss, however, in relation to potential future support of psi application research and development endeavours. Similar comments would apply to attempts to discourage the interest and support of governmental and other large funding agencies. Negative effects upon smaller or more specialized funding sources would be much more serious, and would seriously curtail formalized, systematic investigations of application possibilities and of the basic nature of psi.

In the context of possible suppression campaigns, the target populations of most interest are the specialized practitioners and the general public, for these are the individuals who would be most likely to attempt to apply psi processes for practical purposes. These are also the individuals whose trust in the reality of psi, due to their own frequent and often dramatic psi experiences (in the case of the public) and familiarity with the psi effects and psi-influencing techniques used in their work (in the case of practitioners) would make them relatively unsusceptible to disinformation campaigns. Although some persons might succumb to suppression tactics, I would hope that most would resist, maintain their beliefs, and continue their practices; some, perhaps, going underground.

Scenario C: Psi is Real but Capricious

It may be discovered ultimately that psychic functioning is real, but extremely unpredictable, uncontrollable, and non-manipulable. Psi may remain intractable to the customary methods of Western science. In this third scenario, the reality of psi would indicate that the dominant scientific world-view is incomplete. Its intractability could indicate that the scientific method, as it is commonly understood, cannot accommodate all of nature. Psi phenomena may indicate that there are limits to the explanatory, predictive, and controlling powers of science, and an understanding of psi phenomena could enhance our appreciation of what those limits might be. Psi

could point to other realms or aspects of reality and experience that do not completely overlap the world that has been constructed and is seemingly so well understood by twentieth century Western science. It should be pointed out that whereas capricious psi may not be captured fully by *experimental* methods, we may be able to learn a great deal about psi through *empirical* methods; here, 'empirical' is used in its original meaning of being guided by *experience*, rather than theory.

Within this third scenario, practical applications would be extremely chancy. It might be difficult to know when psi applications would succeed and when they would not, and it might not be useful to rely heavily upon psi or to make critical decisions based upon psi information. On the other hand, psi's visits, although infrequent and unpredictable, could be rich in accurate information that could be useful indeed. The key to successful applications would be a strong familiarity with psi derived from keen subjective analyses, combined with good judgement.

Scenario D: Psi is Real and Somewhat Predictable

Persistent and incisive research may indicate that psi is real, and that it follows definite laws, some of which we might learn and use to our advantage. This sort of knowledge about psi would have important implications for science and would be directly applicable in expanding the scientific world-view. Enough might be learned about psi to permit it to be somewhat manipulable, in the sense that we might be better able to set the stage properly and encourage psi's appearance. Some practical applications would be possible. Which particular applications would be most likely would depend upon the extent and nature of psi's 'lawfulness'. For example, it may be discovered that the occurrence of psi depends importantly on the absence of egocentric motivations in the persons involved in the psi interactions. This finding, in turn, would suggest that psychic

healing applications would be successful, whereas serious attempts to use psi in gambling or in stock market predictions would not succeed. There is even preliminary evidence that this is indeed the case (Harary & Targ, 1985; Puthoff, 1985; Targ, 1988).

The danger in this scenario is that we could easily become victims of self-fulfilling prophecies. If we expect that psi is elusive or weak or untrustworthy or only somewhat lawful, we may indeed discover or create evidence consistent with that view. If we expect greater lawfulness or stronger effects, perhaps we shall find them.

Scenario E: Psi is Real and Quite Lawful

In this fifth and final scenario, psi is discovered to be real, to follow definite laws which we eventually come to understand, and to be quite controllable. A large number of applications would become possible. These applications could include distant mental healing, psychic diagnosis, alternative communication channels, psychic discovery of hidden natural resources, finding of missing people or missing objects, and various legal and financial applications (Jones, 1983). Not all possible applications would be guaranteed success, even in this most optimistic of scenarios, for some applications might not be consistent with the specific laws that are found to govern psi.

I have presented five scenarios which, from the point of view of possible practical applications, lie on a pessimistic to optimistic continuum. It is unclear, at this time, which scenario will ultimately prevail. My own intuitions and my familiarity with the long history of psychical research suggest that Scenario A is the one that is least likely to be true, and that Scenario D is the one that is most likely to be realized. I could very well be wrong, and it would be unwise to rule out any of these scenarios at this time.

Part II. Some Specific Emergent Suggestions

Are there any specific messages emerging from our parapsychological laboratories that might be useful to psi practitioners in the everyday world? The answer is, 'yes and no'. 'Yes', because we can find helpful hints about factors that either facilitate or interfere with good psi performance. 'No', because practitioners are probably already aware of those factors, since it was the observation of these tendencies in everyday life that initially suggested their study in the laboratory! It might be best, then, to view these findings as laboratory vindications of everyday-life wisdom. Of the many findings that could be mentioned, I'll limit my discussion to those that have emerged in my own research and in related investigations of others. These comments concern conditions that appear to facilitate or inhibit performance of extrasensory perception and psychokinesis tasks.

Physical Facilitators and Inhibitors

Some useful 'negative' findings could be mentioned. Success in learning about ESP target events or in influencing PK targets does not seem to be influenced importantly by the physical nature of the target, or by the target's distance in space or in time, nor have we found any physical substance or material capable of either shielding or amplifying psi effects (Palmer, 1978; Stanford, 1977). These findings should provide encouragement to practitioners, since they suggest that there are few, if any, physical barriers to psi functioning.

There are indications that certain classes of targets may be more susceptible to psychic knowledge or influence than others. Numbers, letters and words do not appear to be as psychically accessible as colours, shapes, textures, feelings, meanings, etc. (Delanoy, 1988; Targ, Puthoff, & May, 1979). However, this is merely a suggestive finding demanding more extensive formal exploration. In the realm of psychokinesis, it appears that target events characterized by randomness, indetermi-

nacy, or 'free variability' are more influenceable than systems lacking these qualities (Braud, 1981a; Mattuck & Walker, 1979). It is not yet clear whether the critical facilitating aspect is this physical randomness *per se*, or, rather, the psychological perception of randomness (which might encourage confidence that a target system can indeed change). Perhaps biological systems are more susceptible to PK influence than are inanimate ones (Braud, Schlitz, & Schmidt, 1989), but this possibility has yet to be evaluated in the proper comparison experiments.

There are indications from several recent studies that certain types of ESP may be facilitated by 'quiet' geomagnetic conditions and impeded by 'stormy' geomagnetic field fluctuations (Persinger, 1989; Spottiswoode, 1990). Preliminary findings from our own laboratory indicate that heightened sympathetic nervous system arousal and success at electrodermal biological psychokinesis (i.e., exerting a distant mental influence upon the electrodermal activity of another person) are both positively correlated with geomagnetic 'storminess' (Braud & Dennis, 1989). It would be unwise to become too enthusiastic about any of these findings until they have been replicated much more extensively.

Successful psychic functioning can occur in nearly any physical environment. Pleasant, comfortable, non-distracting settings, however, are most conducive (Palmer, 1978).

Physiological Facilitators and Inhibitors

Psychic functioning probably can occur in any physiological state. However, effective ESP functioning seems to occur more readily under conditions of reduced muscular activity (Braud & Braud, 1974). Reduction of sympathetic autonomic nervous system activity to a moderately low level (Braud, 1981b), and freeing the brain from heavy information-processing demands also seem useful (Braud, 1981a; Stanford, 1987).

There are indications that heightened sympathetic nervous system arousal may be favourable to the occurrence of psychokinetic effects (Braud, 1985a). More investigations of this relationship are needed, as are attempts to determine whether the key factor here is autonomic arousal *per se*, or, rather, the concomitant increase in focused attention.

Psychological Facilitators and Inhibitors

It is in the psychological area that we have learned most about conditions that are favourable or inimical to successful psi functioning (Schmeidler, 1988). Interestingly, much of what we have learned may be summarized in the context of the 3 virtues, faith, hope and charity (love).

Faith. Psi functioning is enhanced by attitudes of belief, confidence, and trust. The often-replicated 'sheep/goat' effect indicates that beliefs in the reality of psi in general, in the possibility of success under the particular conditions of the experiment, and in the likelihood of one's own personal success are associated with better psi performance than are the absences of such beliefs (Palmer, 1978). There are indications that the more thoroughgoing the belief, the greater are the psi effects (Batchelder, 1982). Perhaps at least part of the usefulness of feedback in psi experiments may be attributed to the belief-encouraging and confidence-enhancing results of such information.

Attitudes of disbelief, distrust, doubt and suspicion are inimical to successful psi performance. (One of the more unfortunate results of the 'Project Alpha' hoax (Truzzi, 1987), in which a professional charlatan planted 2 of his apprentices in a parapsychological laboratory in order to 'fool the scientists', is that it has made psi researchers less trusting of those who volunteer for their experiments; this distrust could diminish the success of their studies).

Hope. An attitude of hope or confident expectation facilitates psi performance. Hope is desire accompanied by expectation

of fulfilment. The 'desire' component provides motivation and incentive, which can 'drive' the psi process, while the 'expectation' component may focus the process, directing it to a particular goal or outcome. The role of hope, in its guise of wishing and wanting, is especially evident in psychokinesis, where a specific outcome is desired and expected. Some of the mental strategies employed in psychokinesis, such as imagery or visualization of the desired goal, may 'feed' the expectation process. The same may be said of other psychokinetically useful procedures such as focusing, concentration, and attention-training (Gissurason & Morris, 1990; Morris, Nanko, & Phillips, 1982).

Charity. The relevance of charity or love to successful psi functioning is most obvious in the case of psychic healing and in healing-analogue studies in which feelings of love for the healee or for the healee surrogate may facilitate successful outcomes (LeShan, 1966). In psi experiments generally, positive dispositions toward the experimental situation and toward all persons involved in the study are found to be psi-conducive (Nash, 1978). The reduction of egocentric motives and methods is believed to be favourable to psi success (Stanford, 1974). It is interesting to note that in several recent attempts to use 'associational remote viewing' to predict stock market and silver commodities outcomes, psi-hitting tended to occur when altruistic motives obtained, but shifted to psi-missing when egocentric motives intruded into the experimental context (Targ, 1988).

Other Psychological Facilitators and Inhibitors

Of use to the psi practitioner are other laboratory findings that ESP may be facilitated by conditions of relaxation, freedom from 'noise' or distractions, and the encouragement of inwardly focused attention (Honorton, 1977). Knowledge of possible distortions and associative and symbolic transformations of target content

is useful to the practitioner, as is knowledge of how overanalysis and overinterpretation may lead one astray (perhaps because such mental strategies impose upon the brain or mind a structure which is too great to be modified by the more subtle psi process) (Stanford, 1987). Another lesson from the laboratory is that negativity or defensiveness, in any forms, may be antagonistic to successful psi functioning (Watt, 1991). Still other lessons contain information about what is *not* psi; we have learned to recognize various processes that masquerade as psi (e.g., trickery, misperceptions, cognitive distortions, psychopathology) and have developed methods of dealing with them (Delanoy, 1987; Morris, 1986).

Some Specific Applications

The foregoing suggestions have been rather general ones. I would like to conclude by describing some much more specific findings which are emerging from recent and ongoing experiments in our laboratory. Some of these experiments are biological psychokinesis studies that we have designed as laboratory analogues or models of psychic functioning. In these experiments, volunteer participants attempt to exert distant mental influences upon living 'target' systems. In most cases, the living target system is another person (26 experiments); however, we have also done experiments in which persons have attempted to influence the behaviours of distant animals (8 experiments) and blood cells in distant test tubes (3 experiments). Here are some of the conclusions that emerge from these 37 studies. These conclusions emerge from the results of many experiments that have been summarised in 2 recent papers (Braud & Schlitz, 1989; Braud & Schlitz, 1991).

1. The distant mental influence effect is a relatively reliable and robust phenomenon, as judged by overall statistical results.

2. The magnitude of the effect is not trivial, and under certain conditions it compares favourably with effects produced in more conventional ways. The average

effect size (r) for these experiments is .33, which compares favourably with typical behavioural and biomedical effect sizes. Rosenthal (1985) has suggested an interesting effect size presentation called the 'binomial effect size display' (BESD). This measure converts an effect size to the change in success rate (e.g., change in rate of survival or improvement) that would be expected if a treatment or procedures having that effect size were to be instituted. According to BESD, a survival, success, or improvement rate that is ordinarily 33.5 percent would be increased to 66.5 percent as a result of instituting our biological psychokinesis 'treatments'. This is hardly a trivial effect.

3. The ability to manifest the effect is apparently widely distributed in the population. In all, over 600 persons have participated in these studies. Many persons are 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 individuals.

4. Persons have been successful in exerting distant mental influences upon another person's electrodermal activity, muscular tremor, and heart rate; upon the swimming orientation of fish and the activity level of small rodents; and upon the rate of haemolysis of human red blood cells *in vitro*.

5. The effect can occur at a distance, typically 20 metres; greater distances have not yet been explored.

6. Persons with a greater need to be influenced (i.e., those for whom the influence is more beneficial) seem more susceptible to the effect.

7. Immediate, trial-by-trial analogue sensory feedback is not essential to the occurrence of the effect; intention/ visualization of the desired outcome is effective.

8. Persons have used three strategies in influencing living systems at a distance: (a) producing the desired effects *in themselves* and intending for similar effects to occur in the *distant organism*; (b) visualising the distant organism in situations that would

normally produce the desired effect; and (c) visualising the *measuring instruments* producing the desired indications.

9. The effect can occur without the subject's knowledge that such an influence is being attempted.

10. 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 visualises protective surrounding shields, screens, or barriers may be effective.

11. Generally, our volunteer participants have not evidenced concern over the idea of influencing or being influenced by another person.

12. The effect may generalise to other physiological measures, but may also be intentionally focused or restricted to one of several measures.

13. The effect does not always occur. Its occurrence may depend upon the presence of certain psychological, physiological, and perhaps even physical, conditions that do not always obtain.

Another series of experiments with application implications explores the possibility of *blocking* or preventing psi effects by psychological and psychic means. We have found that such blocking attempts can be successful in the contexts of our electrodermal bio-PK studies (Braud, 1985b; see item 10 above). Blocking attempts have also been successful in the context of some clairvoyance experiments (Braud, 1985b). Many of our blocking experiments have not been successful, however. We are currently attempting to understand which conditions are favourable to successful blocking, and which are not. The development of effective blocking strategies could be extremely important in the prevention of unwanted psi interactions in everyday life; as a way of closing Pandora's box, once the latter has been effectively opened.

Conclusions

Even limiting my discussion primarily to factors that I have personally explored, as I have done in this paper, I feel we have

learned much about the psi process. Some findings confirm, amplify, and extend hints and suggestions that originated in everyday life contexts. However, some 'surprises' have also emerged (such as the roles of randomness, geomagnetic fields, physiological correlates, etc.), which may not have occurred to practitioners without the laboratory work, or at least without careful analyses of large bodies of spontaneous cases and field work.

We often think of progress in psi research as travelling along an only gradually upward sloping line, or, in our more hopeful moments, along a positively accelerating, exponential function, the steepest portion of which is well in the future. However, another possibility suggests itself, and that is that our psi-knowledge curve has the shape of a logarithmic function with rapid acceleration in the past and a levelling off in the present and future. Could it be that we have learned much about the nature of psi and its laws in a natural, informal manner, through experiences and attention and common sense, much the way a child learns the physical laws of trajectories by tossing and catching balls and stones? Might we not have accumulated much tacit, yet accurate and valid, knowledge of psi principles simply through everyday life experiences with psi, even though we have not yet learned to express this knowledge in more formal ways? (Just as the child cannot express his or her true Newtonian knowledge in the form of functional equations?) If this is so, and I believe it is so, we often underestimate what we know about the psi process, what we have accumulated through decades and centuries of experience as individuals and as members of the human species. It may not be wise to underestimate the value of the knowledge of psi that we have obtained in everyday life. Nor would it be wise for practitioners to ignore or underestimate the knowledge that has derived from careful laboratory studies. As in all areas of life, balance is an important key.

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Mind Science Foundation
8301 Broadway, Suite 100
San Antonio, TX 78209
USA

Implications et applications des resultats psi du laboratoire

Résumé: Habituellement on examine les expériences psi du quotidien afin d'avoir des indices sur ce qu'il convient d'explorer en laboratoire. Dans cet article, on inverse le processus en examinant ce que les résultats psi du laboratoire peuvent offrir à ceux qui souhaiteraient appliquer de telles découvertes à leur vie quotidienne. Cette stratégie peut être utile jusqu'à un certain point qui dépend de ce qu'on aura finalement appris des 'lois du psi'; 5 scénarios possibles sont envisagés. On examine ensuite divers facilitateurs ou inhibiteurs physiques, physiologiques, et psychologiques du psi. Les facilitateurs psychologiques s'unissent de façon intéressante sous les formes de foi, espoir, et charité (amour). Finalement, on examine les applications et découvertes de 2 programmes spécifiques de recherche: la psychokinèse biologique et le blocage du psi.

Psychology and Coincidences

Caroline A. Watt

Department of Psychology, University of Edinburgh

Abstract: The paper presents a selective review of research suggesting possible normal causes for some coincidences. After a brief discussion of hidden causes, predictions with multiple endpoints, and simple probability, the bulk of the paper focuses on psychological research into judgement and decision-making under uncertainty. Shortcuts in information processing that have been held responsible for apparent weaknesses in everyday statistical intuitions are discussed, as are recent criticisms of this heuristics and biases paradigm. Examples are given of studies demonstrating how perception, judgement and recollection may be biased so as to confirm our preconceptions. Some implications of this research for the study of coincidences are pointed out, and research suggesting promising remedial measures to improve judgement is noted.

The *Little Oxford Dictionary* (1986) defines a coincidence as a 'remarkable concurrence of events without apparent causal connection'. This definition begs 2 questions: what makes some concurrences of events remarkable and not others, and how does one establish an apparent lack of causal connection? By their nature, remarkable coincidences are one-off, unique events that cannot realistically be manufactured and controlled in a laboratory setting. Parapsychologists therefore encounter coincidences after they have occurred, and must use techniques of interview and meticulous description to try to reconstruct a picture of events involved in the coincidence, much as a detective has to piece together evidence suggesting the events in a crime. Because one can never be 100% certain that all possible causal links between concurrent events have been fully investigated and eliminated, the paranormality of individual coincidences will always be a matter of degree of confidence. It is only when many coincidences are collected together and analysed that some

common trends or patterns may emerge to suggest possible process-related hypotheses, some of which may be quite normal and others paranormal.

In their article 'Methods for Studying Coincidences', mathematicians Persi Diaconis and Frederick Mosteller (1989) identify 4 factors that, they feel, can account for the vast majority of coincidences. These are: hidden cause, multiple endpoints, the law of truly large numbers, and human psychology. Returning to our dictionary definition, the first factor, obviously, suggests causal connections behind coincidences; the others are related to how we find some concurrences of events more remarkable than others. The term 'human psychology' is extremely broad, however, and overlaps somewhat with the first 3 factors; after all, humans experience coincidences, so by definition human psychology is likely to play a part in all coincidences. One might refine the broad 'psychology' topic into 2 categories which are by no means mutually exclusive in the real world but which represent different schools of psychological research. The first refers to characteristics of our intuitive judgements about probability or likelihood; the second refers to ways in which our perceptions, judgements and recollections are modified so as to confirm our beliefs and expectations.

An earlier version of this paper was presented at an SPR Weekend Course on Psi and Synchronicity, November 1990; some of the other speakers focused on paranormal aspects of coincidences. I would like to thank Charles Honorton, Robert Morris and my referees for their helpful suggestions for improvements.

Unlike Diaconis and Mosteller, I am not confident that all coincidences may be explained away by these factors. Perhaps, though, an understanding of them may help parapsychologists to separate the coincidental wheat from the chaff. In this article I will briefly reiterate Diaconis and Mosteller's arguments on hidden causes, multiple endpoints, and truly large numbers, introducing other related research as we go along. I will then expand considerably upon their brief comments on psychological factors, dealing first with studies of the 'intuitive statistician' and then with ways in which our beliefs affect our perception, judgement and memory. While much of this material may already be familiar to parapsychologists, I hope to provide some service by drawing together many disparate strands of research on human judgement under uncertainty, as well as introducing some of the most recent criticisms of the 'heuristics and biases' literature.

1. Hidden Cause

Marks and Kammann (1980) described 'unseen cause' as 'the second root of coincidence' (their first root is simple probability). A coincidence is not surprising if we discover a simple reason for it. But other surprising coincidences can have perfectly straightforward hidden causes, which we have just not yet discovered. For instance, imagine a case where a woman wakes up from a nightmare in which President Gorbachev is attacked in a coup. She thinks nothing more of it, until she sees from the headlines in the following morning's newspaper that this actually happened. On first inspection this could be a meaningful coincidence, suggesting that in her dream she gained information through precognition or clairvoyance. However, when various members of the family are interviewed, it emerges that she went off early to bed the night before. The rest of the family watched the 10 o'clock news in an adjoining room, and although the woman was asleep, the news could be heard in her room. Even though she did not consciously

hear the newflash announcing the coup, this information may have been subconsciously registered, triggering the nightmare. Thus, further investigation of this coincidence between the contents of a dream and a recent news item revealed a possible hidden cause that made the coincidence less surprising. It's quite likely that a proportion of meaningful coincidences can be explained by a hidden cause. Describing the range of such causes is beyond the scope of the present article, but see Marks and Kammann (1980) and Morris (1986, 1989) for more comprehensive treatments of this topic.

2. Multiple Endpoints

A coincidence can be very impressive if it is very specific. Often, however, a 'close' coincidence is also regarded as impressive, although the chances of a 'close' coincidence happening are far greater than the chances of an exact or specific coincidence.

For example, someone may get the hunch that the phone is about to ring, and it will be Auntie Maude, who hasn't been in touch for years, making the call. As predicted, the phone does ring, only it's Auntie Maude's neighbour. Well, that's still quite an impressive coincidence, but you might also be impressed if it had been Maude's husband Bert on the line, or another auntie, or Maude's daughter...and so on.

The prediction was quite specific, but if the experient allows for 'close' coincidences to count, then the prediction has multiple endpoints. That is, there could be many 'close' coincidences that could also be seen as impressive, although the chances of a 'close' coincidence are so much higher than the chances of Auntie Maude alone being the caller.

What is it that makes a coincidence 'close'? Specific events are members of larger categories (for example, relatives who might telephone); elements in the same category or readily associated with each other (for example, a next-door neighbour of Maude) are seen in degrees of closeness in accordance with the size of the category that is shared (for example, next

door neighbour to Maude is 'closer' than a person who lives in the same town as Maude). Objectively, a close coincidence is more likely and less impressive than an exact coincidence. However, the experiencer of an exact coincidence may be nearly as greatly impressed by a close coincidence, and may even forget how specific the original prediction or hunch was, so that, with hindsight, the experiencer feels 'I knew it would happen' (a well-established phenomenon dubbed 'the hindsight effect' by Fischhoff, 1975).

In the multiple endpoints situation, we begin with a specific prediction and end with several possible outcomes. There exists a corollary to this; the so-called 'selection fallacy' (Falk, 1981-82). Scientists carry out experiments upon a random sample with pre-specified methods and analyses, and if they find the probability of their results to be very low they reject the null hypothesis and see their results as supporting the existence of a process other than chance. When an extraordinary coincidence happens, argues Falk, people often commit the logical error of singling out that one unplanned event and labelling it as significant: 'this is like the archer who first shoots an arrow and then draws the target circle around it' (Falk, 1981-82, p.25). Thus, they start with one unplanned event and proceed, *post hoc*, to regard it as significant. The event has been singled out from a range of possible other events (like categories that are shared or readily associated, as illustrated with the multiple endpoints situation) and the non-occurrence of the other possible events is overlooked.

3. The Law of Truly Large Numbers

Events that are rare per person occur in quantity when there are large numbers of people. So, although these events are amazing coincidences to the individuals involved, they are utterly predictable if you look at the population as a whole. Of course, the precise array of events surrounding any coincidence is quite unique and can never be predicted; but it is quite predictable that something staggering is

happening to someone somewhere at this precise moment. In other words 'with a large enough sample, any outrageous thing is likely to happen' (Diaconis & Mosteller, 1989, p.859).

For example, assume that daily, an incredible coincidence occurs to only one person in a million. This appears quite rare. But the population of Britain is 55 million, so each day there are likely to be 55 amazing coincidences; that makes 20,075 incredible coincidences per year. In a country such as the United States, with a population of 250 million, such incredible coincidences begin to be almost commonplace. Thus, with a large enough number of people you are bound to get amazing coincidences. It's when that statistically predictable coincidence happens to you or to someone you know that it feels spooky and you may attribute meaningfulness to it.

People are not ignorant of the fact that amazing coincidences can occur purely by chance, as only one of many possible events that could have happened. When asked to rate the surprisingness of coincidences that have happened to others, individuals are not very surprised by the accounts. When, however, they compare coincidences that have happened to themselves with those that others have experienced, the self-coincidences are consistently described as more surprising, even though others do not find these coincidences particularly surprising. Further, the more meaningful a self-coincidence is to the person involved, the more surprising it is rated as being (a 'trivial' self-coincidence might involve random numbers that are assigned to us, while a 'meaningful' self-coincidence might involve our personal names or birthdays, Falk, 1989).

This 'egocentric bias' suggests that personal involvement in a coincidence makes it seem subjectively less likely. Although we can appreciate that coincidences happening to others represent only one of a large range of possible events, when coincidences occur to us personally we do not see ourselves as 'part of the statistics'. This is a powerful effect: Falk describes how, when telling academic colleagues of the

increased surprise for self-coincidences, she was often interrupted with 'but you should hear what happened to me....' (Falk, 1989, p.488). Thus, personal involvement is one important consideration in explaining why some concurrences of events are seen as remarkable while others are not.

A similar egocentric bias may explain why, although they may be perfectly aware of the statistics for risk of death in car accidents or for risk of smoking-related disease, individuals consistently underestimate the likelihood that they personally will become victims (Slovic, Fischhoff, & Lichtenstein, 1982). Experience perpetuates this myth; the newspapers only report accidents that happen to other people. It is only when someone close to us is involved in an accident or falls ill that we are suddenly reminded that we are not immune to disaster and we are not immortal!

4. Psychology

There are several aspects of human psychology that affect how we judge the likelihood and frequency of coincidences, and that affect our perception and recall of coincidences. Occasionally these psychological factors may contribute to us mistakenly judging a coincidence to be significant or meaningful.

First of all we will consider people as intuitive statisticians. I will describe the findings of research into how we make judgements under uncertainty, including estimations of likelihood or probability, and frequency or base rate information. Secondly I'll describe psychological research into how our perception, judgement and recall can be biased by our beliefs and expectations. Not all of this research has been conducted with coincidences explicitly in mind, but because the experience of coincidences is one form of judgement under uncertainty, readers may see how general psychological research may be relevant to this question.

The Intuitive Statistician

One popular illustration of how we underestimate the likelihood of a concurrence of events is the birthday problem: how many people would you need to gather together before there was a 95% chance that 2 of them would share the same day and month of birthday? The answer is surprisingly (if you are not familiar with this problem) few people; only 48 in fact. For only a 50% chance of 2 individuals' birthdays coinciding, only 23 people need be gathered together. That so few people are needed is usually quite surprising because we typically underestimate the number of different combinations of pairs of birthdays that can occur with a small number of people. We expect that with 365 possible birthdays you'd need a fairly large number of people before there was a coincidence of birthdays.

Diaconis and Mosteller (1989) have developed a simple formula that enables the calculation of the number of people needed to get a coincidence of birthdays or of any other categories: how many people (N) do you need for there to be a 50%/95% likelihood that at least 2 of them will fall in the same category from among a number of categories (c) such as 365 possible birth-dates?

Approximately,

$$N = 1.2\sqrt{c} \text{ for } 50\% \text{ chance}$$

$$N = 2.5\sqrt{c} \text{ for } 95\% \text{ chance}$$

Using this formula, Table 1 shows how many people are needed for coincidences between different numbers of categories.

Table 1
Guide to solving the birthday problem, and other coincidences of categories(c)

c	= 100	200	300	365	400	500	600	700
N(50%)	= 12	17	21	23	24	27	29	31
N(95%)	= 25	35	43	48	50	56	61	66

It is interesting to note how slowly N rises as c increases, so that having several hundred more partygoers does not dramatically increase the chances of a coincidence of birthdays.

Diaconis and Mosteller extend this calculation to apply to other, more complex situations, for instance, where there is more than one type of category that could coincide (such as birthdays and year of birth), and where 'close coincidences' are accepted (the multiple endpoints situation described earlier). The formula to estimate the number of people needed for a coincidence within k days in the latter, 'almost birthdays' situation (with a 50-50 chance of a coincidence) is:

$$N = 1.2 \sqrt{\frac{c}{2k+1}}$$

With $c(\text{categories}) = 365$ and $k = 1$ day, only around 13 people are needed for a match.

These formulae may be helpful in estimating the likelihood of coincidences where the number of possible categories is known or can be discovered after some research. There remains, however, a large number of events whose frequency is difficult to measure objectively or even to estimate, and which therefore cannot be examined using such formulae. For these, as well as for coincidences that are quantifiable, people may fall back on rough 'rules of thumb'; the so-called cognitive heuristics.

Over the last 20 years cognitive psychologists, led by Amos Tversky and Daniel Kahneman, have developed the idea that people use a number of rules of thumb or cognitive shortcuts in their everyday processing of information. Usually these strategies, called cognitive heuristics, are perfectly adequate to get us through daily life efficiently. When it comes to assessing the statistical likelihood of events such as coincidences, however, it has been argued that the use of these heuristics can introduce a source of bias into our estimations. Hence, this area of research has come to be known as the 'heuristics and biases' school

(e.g., Kahneman, Slovic, & Tversky, 1982; Nisbett & Ross, 1980).

As I said in the introduction, there has recently been a backlash against the heuristics and biases movement. Before I describe the reasons for this in more detail, however, I will briefly introduce 2 major heuristics (judgement by representativeness and judgement by availability) whose use may introduce some bias into people's base rate and probability estimates.

Judgement by Representativeness has been proposed to explain an apparent lack of understanding of the 'law of large numbers' (the larger the random sample, the greater its accuracy in estimating the characteristics of the parent population from which it is drawn). It is argued (e.g., Tversky & Kahneman, 1974) that people judge the likelihood of an event according to the sample's similarity to, or representativeness of, the parent population on certain essential features such as means and proportions. Sample size, which should give some indication of the degree to which one could confidently predict characteristics of the parent population, was frequently neglected by subjects in early studies by Kahneman and Tversky.

For instance, subjects were posed this question:

'A certain town is served by two hospitals. In the larger hospital about 45 babies are born each day, and in the smaller hospital about 15 babies are born each day. As you know, about 50% of all babies are boys. The exact percentage of baby boys, however, varies from day to day. Sometimes it may be higher than 50%, sometimes lower. For a period of 1 year, each hospital recorded the days on which more than 60% of the babies born were boys. Which hospital do you think recorded more such days?' (Kahneman & Tversky, 1972, p.443).

Subjects' opinions were equally divided between the two hospitals, despite the fact that by the law of large numbers the smaller hospital would be expected to show more deviations from the average

50% figure. Later, however, it was demonstrated that subjects could take account of sample size if the wording of questions was simplified (e.g., Bar-Hillel, 1979); indeed, if sample size was the only information provided, then correct responding could approach 100% (Evans, 1989).

Nevertheless, in the real world, people are faced with lots of possibly irrelevant information, which may distract attention from features such as sample size that should be taken into consideration when making judgements under uncertainty. So in the case of coincidences, if people tend not to take sample size sufficiently into account when judging likelihood, they may not appreciate that an extreme outcome is more likely to occur in a small sample, and may therefore mistakenly attribute significant rarity to a coincidence occurring under these conditions.

The representativeness heuristic has also been proposed to explain the so-called 'conjunction fallacy' (Tversky & Kahneman, 1983). Here, subjects judge the conjunction of 2 events as more probable than one of its components because, it is argued, they judge according to the similarity between the paired events and an original descriptive statement; this is despite the basic tenet of probability theory that a conjunction cannot be more probable than one of its constituents. For example, subjects were given the following description (Tversky & Kahneman, 1983, p.297):

'Linda is 31 years old, single, outspoken and very bright. She majored in philosophy. As a student, she was deeply concerned with issues of discrimination and social justice, and also participated in anti-nuclear demonstrations.'

Subjects were asked to indicate which of 2 alternatives was more probable: 'Linda is a bank teller'; or, 'Linda is a bank teller and is active in the feminist movement'. 85% of the respondents indicated that the latter statement was more probably correct, a finding which Kahneman and Tversky interpret as a blatant violation of the conjunction rule.

Judgement by Availability is the second cognitive heuristic that may influence our judgements about coincidences. When we use availability we estimate frequency in terms of how easy it is to think of examples of something (Tversky & Kahneman, 1974). Like representativeness, judgement by availability is usually a good rule of thumb, but it can lead to biased decisions because availability is influenced not only by objective frequency but also by recency, familiarity and vividness. For example, when we estimate how often earthquakes occur in a 10 year period we are too heavily influenced by whether an earthquake has occurred recently.

The apparent neglect of base rate or frequency information in making probability judgements (the 'base rate fallacy') has been widely attributed to the operation of the availability heuristic (e.g., Borgida & Brekke, 1981). Here, it is argued, base rate information is often less vivid, more abstract, less noticeable than other kinds of information and so it tends to get overlooked. In the earthquake example, the base rate or frequency information refers to data about how many earthquakes have occurred in the last 10 years. Typically, this statistical information is overlooked in favour of the vivid memory of a recent earthquake, leading to an exaggerated estimation of the frequency of earthquakes. Studies that have increased the availability of base rate information (for instance by conveying it graphically rather than in tabular form) have shown that it can be taken into account by subjects.

Another consequence of the availability heuristic is that we pay less attention than we should to negative information - to non-occurrences or non-coincidences - because they are less noticeable. Logically, the failure of something to happen can be just as informative for our decision-making as a positive occurrence. Yet, because non-events are less salient or less memorable, their usefulness for judging the frequency of, say, coincidences, is neglected. Take, for example, a person who believes that she can make people telephone her simply by wishing for it to happen. When she

succeeds, and there is actually a close coincidence between the time of her act of willing and the phone ringing, this is especially memorable. Failures of the phone to ring will tend to be less noticeable. Looking back on her efforts, the lady will tend to have a higher opinion of her skills than objectively she should. This is because the successful coincidences of willing and the phone ringing will be more available than the non-coincidences, and so she is likely to overestimate the frequency of her successes. Confidence in her psychic ability may be further enhanced if she accepts close coincidences - such as phone calls coming up to 30 minutes after she willed them - as satisfactory evidence of her abilities. This is another example of multiple endpoints.

In Edinburgh we often get people calling in who think they are having psychic experiences, such as precognitive dreams. People may be wanting us to confirm their abilities, or to get rid of them. As a first step we need to get a good description of what is going on, so we may ask people to keep a diary, noting every possibly precognitive dream and whether or not it 'came true'. Doing this, people often find that they have been overestimating the frequency of their success rate, presumably because successes are so memorable. Recording actual performance can circumvent the availability heuristic to some extent, and make the non-coincidences less easy to ignore.

The heuristics and biases studies have focused on individual cognitions in relatively simple and sterile situations; subjects are often posed problems in paper and pencil form, for example. Experiments in social psychology, however, which have used more realistic or ecologically valid methodologies, have also demonstrated that availability plays an important role in our perceptions of causality. When something is available, it is more vivid, salient, noticeable, or memorable. Simple experimental manipulations of our focus of attention can dramatically influence our perceptions and causal attributions (for a review, see Taylor & Fiske, 1978), such that causality is attributed to salient stimuli.

In summary, judgement by availability may lead us to overestimate the frequency of coincidences that we expect to occur (such as predicting phone calls), and to neglect actual base rate information that conflicts with our expectations or that has low salience (for example, overlooking failed predictions).

Heuristics and Biases Re-Evaluated

Since the original influential experiments by Kahneman and Tversky which provoked a veritable flood of research, some psychologists and statisticians have begun to question the assumptions behind these studies. Criticisms have centred on: the language used to describe the effects of heuristic use; the statistical models underlying many of the studies asking subjects to make probability judgements; the methodology used to demonstrate heuristic use; and the usefulness of the cognitive heuristics in understanding judgement under uncertainty. I will cover each of these in turn, before summarising their impact on the question of coincidences.

1. *Value-Laden Language.* In her article 'The Rhetoric of Irrationality', Lola Lopes (1991) points out that the original heuristics and biases papers by Kahneman and Tversky focused on the *process* of judgement under uncertainty, whereas the summary article that appeared in *Science* (Tversky & Kahneman, 1974), and which therefore reached a wide audience, shifted emphasis from heuristic processing to *biased* processing. Strong evaluative language is used in this second article (e.g., 'severe errors of judgment', p.1130) and in follow-up research by other authors (e.g., 'Probability judgements are notoriously inaccurate', Blackmore & Troscianko, 1985, p.459). This language conveys a clearly critical and negative message about subjects' cognitive abilities. This might not be controversial in itself, were it not for the fact that the original experiments by Kahneman and Tversky were logically set up to identify processes rather than to

evaluate performance. Yet it is the 'inadequate intuitive statistician' message that caught the imagination and tinged the research approaches of subsequent investigators.

Lopes argues persuasively that evaluative language does not belong in scientific articles; these should be concerned with description and interpretation rather than value judgements. The 'rhetoric of irrationality' may serve to titillate authors and readers, who can feel themselves superior because (with hindsight) they can solve the probability problems; the strong language also gives the impression (misleading, as we shall see) that there is an obvious correct answer to such problems.

2. *Statistical Models.* Often the authors of papers on heuristics and biases use phrases such as 'subjects' inability to appreciate the laws of probability' or their 'lack of intuitive understanding of the normative theory of prediction'. Whereas anyone reading a standard textbook on statistics could be forgiven for concluding that there is some sort of 'normative probability theory' that provides correct answers to problems posed in some heuristics and biases experiments, those in the know - that is, statisticians - have pointed out that there is no normative probability theory; and, worse still, that the statistical assumptions behind the probability problems come from a school of reasoning that is held by only a minority of statisticians.

The most authoritative critic of the model of probability used in most heuristics and biases literature is Gerd Gigerenzer (e.g., 1991a, 1991b; see Gigerenzer et al., 1989, for a description of the historical development of the different statistical schools of thought; and see Gigerenzer & Murray, 1987, for a detailed consideration of these as they have been applied to the study of judgement under uncertainty). In a paper entitled 'How to make cognitive illusions disappear: Beyond heuristics and biases', Gigerenzer (1991a) makes a strong critique of the heuristics and biases school:

What is called in the heuristics and biases literature the 'normative theory of probability' or the like is in fact a very narrow kind of neo-Bayesian view that is shared by some theoretical economists and cognitive psychologists, and to a lesser degree by practitioners in business, law, and artificial intelligence. It is *not* shared by proponents of the frequentist view of probability that dominates today's statistics departments, nor by proponents of many other views; it is not even shared by all Bayesians....By this narrow standard of 'correct' probabilistic reasoning, the most distinguished probabilists and statisticians of our century....would be guilty of 'biases' in probabilistic reasoning. (pp.86-87)

Gigerenzer proceeds to demonstrate how 'overconfidence bias' (where subjects answering a series of questions show a discrepancy between their perceived success and their actual performance of a task; overview by Lichtenstein, Fischhoff, & Phillips, 1982), the 'conjunction fallacy' and the 'base rate fallacy' can be made to 'disappear' if questions are re-phrased to take account of alternative statistical models and meanings of probability.

Let us return to the 'Linda is a bank teller and is active in the feminist movement' example used to illustrate the conjunction fallacy. Gigerenzer points out that to choose this description of Linda as more likely is a violation of *some* subjective theories of probability, including Bayesian theory, but it is not contrary to the dominant frequentist school of probability, because in this latter model, single specific events cannot be considered in terms of probability; probability theory is about frequencies, not single events. If the Linda problem is rephrased in frequentist terms 'There are 100 persons who fit the description above (i.e., Linda's). How many of them are: (a) bank tellers (b) bank tellers and active in the feminist movement' then the 'conjunction fallacy' largely disappears, with only 22% of subjects choosing option (b) as most likely (Fiedler, 1988).

Some of the 'errors' identified by Kahneman and Tversky and their followers may therefore be due to the researchers' adoption of an inappropriate statistical model rather than to weaknesses in their subjects' reasoning abilities. Further, away from the relatively controlled and clean world of the laboratory, the confusions and complexities of the real world may make the application of any statistical models controversial and rather difficult.

3. *Experimental methodology.* Earlier, when discussing the 'law of large numbers', I cited a study that demonstrated that people are more able to take account of this law if the question is phrased more simply, and if other distracting information is removed. In a similar vein, many of Kahneman and Tversky's original positions have been refined, following demonstrations that variations in experimental methodology cause variations in the apparent influence of cognitive heuristics upon problem solving and judgement under uncertainty. We have already seen how the 'conjunction fallacy' can be made to disappear by rephrasing the question.

Steven Sherman and Eric Corty (1984), for instance, review a number of studies that suggest that the extent to which heuristics are used to solve a problem may depend on the way in which the problem is presented or structured. If there is plenty of time, if the task is not too complex and is clearly presented, if base rate information is made concrete, salient and specific to an individual case, then individuals may reach the normatively correct solution (where there is one). For example, typical biases in judging random sequences can be eliminated simply by instructing subjects that random events may be present or by providing them with a comparison level of nonrandomness (Peterson, 1977).

Related to the question of experimental methodology is another telling criticism of the heuristics and biases paradigm: its lack of ecological validity. There is a considerable gulf between the sorts of paper and pencil probability problems posed to unsuspecting subjects in typical heuristics

and biases experiments, and the everyday situations where judgements about probability are made (e.g., when placing a bet; when judging what caused a picture to fall off a wall; when reading about or experiencing coincidences). When such artificial situations are used in conjunction with possibly inappropriate models of probability, any conclusions that may be drawn about the use of cognitive heuristics in more complex situations become severely limited. There is a need for the heuristics and biases researchers to adopt more realistic methodologies; for instance, role-playing, simulations of complex situations, and observational studies of individuals' statistical judgements in their natural environment. As we shall see in the next section, studies of the biasing effects of beliefs and expectations on perception, judgement and memory have successfully used more realistic settings, and have produced findings that have practical applications.

4. *Theoretical usefulness of heuristics.* Sherman and Corty (1984) also note that Kahneman and Tversky's heuristics are rather vague and are often identified *post hoc*. They are insufficiently precisely defined to enable prediction of which particular heuristic will be applied in which specific situation. Gigerenzer (1991a) echoes these criticisms thus: 'All three heuristics...are largely undefined concepts and can *post hoc* be used to explain almost everything. After all, what is similar to what (representativeness), what comes into your mind (availability), and what comes first (anchoring) have long been known to be important principles of the mind' (p.102). Heuristics, he argues, are hardly more than re-descriptions of the phenomena seen in judgement under uncertainty.

Conclusions

Do these criticisms of the heuristics and biases literature negate its applicability to the question of what makes coincidences seem remarkable? Certainly, they seriously weaken those aspects of the literature that deal with probability judgements and pre-

diction where some sort of normative theory of probability has been (questionably) assumed. Further, it is difficult to generalise from the typically artificial methods used, to more complex settings. But although 'overconfidence bias' as typified in the heuristics and biases literature may 'disappear' if an alternative statistical model is adopted, in more realistic situations such as in studies of eyewitness testimony, overconfidence nevertheless remains a problem. Wells and Murray (1984), for instance, reviewed studies of eyewitnesses' confidence in their memory reports and concluded that 'the eyewitness accuracy-confidence relationship is weak under good laboratory conditions and functionally useless in forensically representative settings' (p.165).

Gigerenzer's criticisms have, however, been constructive: he suggests that the study of judgement under uncertainty may explicitly utilise various statistical models to get a clearer idea of which model most closely approximates subjects' intuitive reasoning (one might also have to consider the possibility of individual differences in model selection). Also, many statistical principles, such as the law of large numbers, are uncontroversial, and in this section I have tried to focus on aspects of judgement under uncertainty that are not so vulnerable to criticism of underlying statistical assumptions. The research on the effects of salience or availability on focus of attention and causal attributions, for example, reinforces the apparent importance of availability for judgements under uncertainty (e.g., Taylor & Fiske, 1978, Dow (Watt), 1988). Lopes' comments on evaluative language are well-taken, and are a useful reminder to all concerned with heuristics and biases that they should look out for 'creeping value judgements' in their writings.

We have seen that the degree to which heuristics are used depends greatly on the presentation of problems in the experimental situation, and that careful simplification and manipulation of information can modify or overcome heuristic use. There is no doubt, however, that in the real world,

judgements have to be made under much greater uncertainty, with a profusion of distracting information and incomplete data. I believe that it is in these conditions that we are most likely to simplify by resorting to rules of thumb. If relevant information, such as base rates, is readily available and noticeable, then we have seen that it can be applied quite appropriately by individuals. On occasions when all relevant information is not at hand, heuristics may be used. Evans (1989) makes the useful distinction between competence and performance in statistical reasoning. People can be seen in some circumstances competently to apply statistical principles in judgement under uncertainty. What we need to understand is why this competence is not applied under a different set of circumstances.

The final criticism of the cognitive heuristics, that they are vague and *post hoc*, is, to me, the most telling. At the moment cognitive heuristics are largely descriptive (or heuristic!) devices to help psychologists organise their thoughts about other people's thought processes. Description is a necessary stage in the development of theoretical ideas, but the heuristics literature has yet to progress beyond this descriptive phase. We need a theory or theories of judgements under uncertainty to be developed to a stage where they offer 3 things: falsifiable predictions; an explanation of why humans judge the way they do; and predictions of the circumstances under which the various judgemental biases might be expected to operate. Describing theories of human reasoning as 'fragmented', Evans (1991) states, 'while theorists interested in bias emphasize...the role of non-reasoning processes, those interested in competence emphasize...reasoning processes' (p.97). There is a lack of integration between the various approaches to the study of human reasoning, and Evans makes some constructive recommendations for overcoming this problem. I would agree with Sherman and Corty (1984), however, that cognitive heuristics can potentially identify the processes underlying decision-making, and can

potentially suggest how to solve decision-making problems and improve judgement. For these reasons, they may be useful in evaluating coincidences.

4.2 The Influence of Beliefs and Expectations on Perception, Judgement and Recall

Apart from characteristics of our statistical intuitions that may cause some coincidences to seem remarkable, the sense of meaningfulness of coincidences may be enhanced by other aspects of our information processing. In short, how we perceive, interpret and remember events is, to a large extent, determined by our *a priori* beliefs, expectations and theories (or schemata) about how the world works. Information that is consistent with our expectations is readily assimilated to strengthen our beliefs; on the other hand, information that does not fit with our expectations may be distorted to make it fit, selectively ignored, or forgotten, so that our prior expectations or interpretations of an event or a coincidence are not challenged.

How Beliefs Can Influence Perception and Judgement

Not only do people tend to overlook non-occurrences or their failures to get the coincidences they predicted; they also tend to see relationships where there are none. This is called the "illusory correlation" effect, and usually refers to cases where people associate 2 factors, though statistically no relationship exists. Our theories and stereotypes often lead to our perceiving illusory correlations.

The classic studies showing illusory correlation (Chapman & Chapman, 1967, 1969) were concerned with the question of why clinical psychologists persisted in reporting correlations between patients' responses on a projective psychological test, and aspects of the patients' motivations and emotions. Detailed studies of this Draw-A-Person (DAP) test suggested that responses on the test were totally unrelated to clinical symptoms. Yet clinical psycholo-

gists found that paranoid or suspicious patients exaggerated the eyes in their drawings, whereas dependent patients, who like to be fed and cared for, exaggerated the mouth. The Chapmans asked patients in a State hospital to take the DAP test. These drawings were then paired *completely at random* with 6 symptoms, such as suspiciousness and dependence. The Chapmans asked untrained college students to examine the drawings and the symptoms with which they had randomly been paired. Later, the students were asked which features of the drawings had most often been paired with each symptom. The students reported the same kinds of association between symptoms and drawings that the clinicians had, even though it had been arranged that there was no systematic relationship for the students (incidentally, these experiments do not suggest that the DAP test is of no clinical use; it may be helpful to clinicians when taken in the context of a wider clinical investigation).

Sometimes it is valid and efficient for our expectations to influence our interpretation of information; for instance, our knowledge of language may enable us to understand what is being said over a noisy telephone line. At other times, our preconceptions can be misleading; for example, where wishful thinking or preoccupation with a particular idea may lead to a misinterpretation of the caller's words. With regard to the study of coincidences, the challenge is to identify when information may have been distorted or misinterpreted. Though there is no easy answer to this problem, some pointers are given by psychological research.

Nisbett and Ross (1980) identified some factors that increase the likelihood of erroneous bias based on *a priori* beliefs or theories:

1. *Confidence in the theory.* If this confidence is based on emotional commitment to the theory rather than on a solid factual foundation then it is more likely that we will selectively process information so as to strengthen our beliefs.

2. *Availability of the theory.* The likelihood that a theory will influence how we interpret information depends on its availability; its likelihood of being triggered by the data at hand. If you have recently attended a course in Freudian psychoanalysis, this theory might be very available for you and be readily used to interpret the actions and dreams of people around you. A common example of the possible operation of availability in coincidences is where you learn a new word, then suddenly notice it repeatedly cropping up. It is unlikely that you have never before encountered the word; rather, your attention has been drawn to it, and it has become salient or available for you to notice when it occurs again.

3. *Ambiguity of the information.* Evidently, if information is clear and unambiguous then it may be more difficult (though not impossible) for us to put our own interpretation on that information based on our preconceptions. If, on the other hand, the information is experienced in an ambiguous way - say, in poor light, in confusing circumstances - then it is much easier for us to interpret it so as to fit our expectations. Fading of memory and the operation of our cognitive heuristics can render initially clear information ambiguous. This is why it is so important to take note of, for instance, each prediction that we make, plus whether or not it is fulfilled; and to write down details of a coincidence as soon as possible. The note-taking makes the information less ambiguous than our unassisted memory would.

How Information Often Doesn't Influence Our Beliefs

Psychological research suggests that once we have made up our minds about something we are very resistant to revising our theories. Here, I'll give examples of 3 areas of research into the effects of information on beliefs: firstly, what happens when established beliefs are faced with new information; secondly, the effect of new

information on new beliefs; and thirdly, the effect of false information on beliefs.

1. *New Information and Established Beliefs.* Lord, Ross, and Lepper (1979) took 2 groups of university students: one group strongly believed that capital punishment was a deterrent to potential murderers; the other strongly believed it was worthless as a deterrent. Each subject read about the results of 2 supposedly authentic studies on the deterrent effects of capital punishment. One of the studies concluded that capital punishment was an effective deterrent. The other concluded the opposite. Subjects were asked a number of questions after they had read both studies.

There were 3 main findings from this experiment: 1. Whichever study supported a subject's own initial position was found to be significantly 'more convincing' and 'better conducted' than the study opposing their position; 2. When subjects were asked about their beliefs after reading about only one study, which could be in agreement with or in contradiction to their own views, belief in the subject's original position was strengthened if they had just read a supportive study, but belief in the original position was hardly affected at all by reading an opposing study; and, 3. After reading about *both* studies, the subjects were more convinced about the correctness of their initial position than they were before reading about *any* evidence.

In summary, different standards are used for criticizing opposing evidence to those used for criticizing supportive evidence. Mixed evidence, giving equal support to 2 opposing views, does not reduce confidence for holders of either view but instead reinforces confidence for holders of each view.

Perhaps these results were obtained because the subjects were impressionable young students. But even in the supposedly rigorous and objective world of reviewing articles for scientific journals, prior beliefs have a strong influence on evaluations. In a controversial 'real-life' experiment, Douglas Peters and Stephen Ceci (1982) re-submitted 12 already-published research

articles by authors from prestigious American psychology departments to the 12 widely-read and respected American psychology journals in which they had been originally published only 18 to 32 months earlier. The re-submitted articles were virtually identical to the originals, except that the author's name and institution were changed to fictitious ones (e.g., Dr. Wade M. Johnston at the Tri-Valley Center for Human Potential). Only 3 of the resubmissions were recognised as such. Of the remaining 9 articles, 8 were rejected on grounds such as 'serious methodological flaws'. Peters and Ceci suggest that the journal editors and reviewers may have been biased by the original authors' status and institutional affiliation. Findings such as these argue in favour of blind refereeing of academic articles.

Research along these lines provides a useful reminder that our experiences and interpretations of coincidences can be dramatically affected by our prior expectations, and that adopting an 'impartial' scientific cloak may be ineffective.

2. New Information and New Beliefs. Moving on now, we consider the effects of new information on new theories; specifically, the effects of the sequential processing of evidence. Usually we do not get all the information for or against a theory, or, in the context of this paper, an explanation of a coincidence, at once; some comes earlier and some comes later. People tend to base their explanations on the earliest evidence; on their first impressions. This is known as the primacy effect. Logically, when evaluating the evidence for or against a theory, all evidence is important, not just the first evidence that is encountered.

For example, in Jones et al. (1968), subjects were asked to observe another person trying to solve 30 multiple choice problems. The problems were described as being of equal difficulty. The person doing the problems always solved the same number; 15. In one condition - the descending condition - he solved a greater proportion of problems early in the series, and fewer later in the series. In the other condition -

the ascending condition - he did the reverse, solving few problems to begin with, but solving more later on in the series. Subjects were asked to predict how the target person would perform with a second set of problems, to rate his intelligence, and to try to recall how many problems he had solved in the first set of problems. It was found that early performance received undue weight. The target person who solved relatively more problems early on was seen as being more likely to perform well on a second set of problems; judged as being more intelligent; and remembered as having solved more problems than the other target person.

Studies like this highlight the importance of attempting to gather all relevant information when evaluating coincidences, before forming interpretations. Of course this is difficult, especially in complex situations, but simple procedures such as using the 'birthday problem' formula to estimate the likelihood of a coincidence, may assist in a balanced evaluation of coincidences.

3. False Information and Beliefs. The third way in which our initial theories or beliefs fail to be influenced by objectively relevant information is when our theories persevere not in the face of new information, but rather in spite of the discrediting of the information that originally led to the formation of our beliefs or theories.

For example, Anderson (1983) presented subjects with either 2 case histories (vivid, concrete data) or a statistical summary (abstract data). The data suggested either that firefighter trainees who enjoyed risk performed well in their later careers, or that they did not perform well as firefighters. Subjects were then told that the data were fictitious. Later, it was found that they still clung to whatever initial theory they had been led to hold, and the strength of the perseverance of belief was greatest for those subjects who had seen the concrete data (this suggests the operation of salience or availability biases). This is paradoxical, because small numbers of case studies are likely to be less accurate indicators of general population characteristics than sta-

tistical summaries of a wider survey. Anderson concluded that this effect was not due to memory but to the spontaneous generation of causal explanations that seemed to be facilitated by the case histories. In the case of coincidences, of course, the data are also usually concrete; in the form of personal experiences or anecdotes that are told by others.

To sum up this section: we have seen the interplay of human psychology, beliefs, and data. We tend to cling unduly to our own beliefs or theories, even in the face of contradictory evidence, and we apply a double standard to evidence relevant to our beliefs. We have probably all seen this happening in our everyday life; but we may neglect to consider these facts when we ourselves are involved. We can easily see the weak points in other people's beliefs, while being absolutely certain of the truth of our own. This may be one reason why we are less impressed when coincidences happen to other people than when we are closely involved in them ourselves (Falk, 1989).

How Recall Can Change Due to Beliefs and Expectations

Memory is a construction, based partly on our perceptions and partly on our interpretations, and memories tend to fade and alter over time. It appears that when we recollect something we actively reconstruct our memories so as to fit with our theories and expectations. When we recall coincidences that we have heard of or have been involved in in the past, our memory may blur some details and strengthen others so as to make the coincidence seem more impressive than it was to begin with, a process which may be quite unconscious.

In 1971, Bransford and Franks developed their Constructive Model of Memory. Subjects were presented with sets of simple sentences, some of which they had seen a few minutes before and others which were new sentences, including combinations of the earlier sentences. When they were asked to identify those sentences they had seen before, many subjects were convinced

they had seen the new combination sentences before. Bransford and Franks proposed that individuals integrate information from individual sentences so as to construct larger ideas; they think they have already seen these complex sentences because they have been combined in memory and, once combined, they cannot break them down into their original components.

This constructive model of memory is not necessarily limited to recall for sets of sentences. People instinctively try to make sense out of any situation - sets of noises, events happening around them, snippets of conversation - and their memories of these events may contain not only just the original events but also the interpretation put on them by the individual.

One example of the study of recollection change in more realistic situations, which are perhaps more relevant to the evaluation of coincidences, is work in the area of eyewitness testimony (e.g., Wells & Loftus, 1984). In a typical experiment, Loftus and Loftus (1975) showed subjects a film of a traffic accident. Soon after that, subjects were asked questions about their memory of the accident. One of these questions, about the speed of the cars, was asked in 2 different ways. Subjects were either asked, 'How fast were the cars going when they smashed into each other?' or they were asked 'How fast were the cars going when they hit each other?' Apparently, subjects used the different inferences suggested by the words 'smashed' or 'hit' to alter their memory of the accident. 'Smashed' implies a more destructive collision than 'hit'. A week later, subjects were given a memory test, where they were asked 'Did you see any broken glass?' Although there was no broken glass in the original film, those subjects who had been asked the 'smashed' question were more likely to say mistakenly that they remembered seeing broken glass.

The sentence-recall experiment showed how information could be misremembered only a short time after its presentation. Generally, the more time that passes after the original incident, the more chance there

is that recollections will change. You can imagine how recall might change over months or years after an original event. This suggests that sometimes a coincidence that was only moderately impressive to begin with can, over time, be recalled differently, as really very striking.

These experiments into sentence recall and eyewitness testimony demonstrated *misremembering*. Other studies have demonstrated *selective* remembering. Hintzman, Asher, and Stern (1978) explored their hypothesis that coincidences seem to occur more often than chance because of selective remembering of meaningfully related events, by asking subjects to rate a series of concrete nouns and, at another time, a series of pictures of objects, in a task ostensibly unrelated to memory. Some of the nouns and pictures were related to each other, but the rest were unrelated (the authors do not say by what criteria the judgements of relatedness were made). Later, participants were unexpectedly asked to recall as many words from the list of nouns as possible. This was therefore an incidental learning task, and the authors regarded the related nouns and pictures as coincidences. They found that significantly more 'related' words were recalled than 'unrelated' words, suggesting that there was selective remembering of the meaningfully-related words. An experiment of similar design but using events rather than nouns (the former being components of coincidences in the real world) replicated this selective memory retrieval effect (Kallai, 1985, cited in Falk, 1989).

In a review of the literature into 'Alterations in recollections of unusual and unexpected events', Hall, McFeaters, and Loftus (1987) described how new information could be absorbed and interpreted as an original memory. A coincidence, of course, is an unusual and unexpected event. New information might be embedded in a misleading message, or in a biasing question, or in a sketch or photograph. Private remembering of the event, discussion with friends or family, or even questioning by a careless investigator can be a source of misleading opinions and

information. The experiment on eyewitness testimony described above showed how careless questioning can bias recollection.

Hall, McFeaters, and Loftus identified 4 major factors (time delay, warnings, question phrasing, and attitude) which affected the change in recollections for unusual or unexpected events. These 4 factors have been fairly well demonstrated in experiments.

The first is the *time delay* between an event, a subsequent misleading message, and a final test of recollection. It seems that changes in recollection are greatest if there is a relatively long time delay before the misinformation is given; presumably so that the original memory can fade. Then, the change in recollection is greatest if people are tested about their recall of the original information while the post-event misinformation is still relatively recent.

Secondly, it has been shown that if people are *warned* just before they are to be exposed to misinformation that the message may contain misleading information, then they are less likely to be influenced to change their original recollections. This effect is quite specific, though. If the warning is not given immediately before the post-event misinformation, then it's not usually effective.

Thirdly, it seems that the way in which a misleading question is *phrased* affects the likelihood of recollection change. After a surprise intruder interrupted their lecture, subjects who were asked, 'Was the moustache worn by the tall intruder light or dark brown?' were less likely to (mistakenly) recall that the intruder had a moustache than those who were asked 'Did the intruder who was tall and had a moustache say anything to the professor?' (Loftus, 1981). The latter question included the misinformation in an auxiliary clause, suggesting that memory is more easily altered if misinformation is casually or unintentionally absorbed, rather than being given direct and critical attention. Also, misinformation that is slowly scrutinized may be rejected, whereas if you give brief and minimal attention to the misinformation, it

may be added easily to the original recollections.

I described earlier how *attitude* can affect how we perceive or remember information. This has also been demonstrated in the experiments into eyewitness testimony. Information that is consistent with attitudes is strengthened in the process of recollection, whereas information that doesn't fit fades, or is replaced. In a classic experiment, subjects were shown a picture of 2 men in an underground train. One of the men was white, the other black. The white man held an open cut-throat razor in his hand. Subjects were asked to describe the picture to others, who in turn described it to others, and so on. It was found that, over time, the razor moved from the white man's hand to the black man's hand (Allport & Postman, 1947).

Summary and Future Directions

Some of the research described in this paper may not be new to parapsychologists, but by drawing together a variety of psychological studies relevant to the evaluation and experience of coincidences, I hope some readers may be stimulated further to consider the implications of this psychological research for the study of coincidences. I am only too aware of the limitations of this paper, which can be subjected to the same sorts of criticisms as have been levelled against the heuristics and biases approach: I have merely cobbled together a number of descriptions of relevant research findings without providing any useful explanatory framework. The various psychological factors I have described may be applied *post hoc* to account for many coincidences. What would be even more useful would be some theory or theories enabling the prediction of the circumstances under which these factors would be expected to operate. This will probably have to await further developments in mainstream psychology, though Hogarth (1981), Gigerenzer (1991a) and Evans (1991) make some constructive suggestions for how researchers could

progress beyond the stage of cataloguing heuristics and biases.

A consideration of techniques for overcoming the many biases in our judgements under uncertainty would also have been helpful, but would have made the paper unacceptably long. The interested reader is referred to Kahneman and Tversky (1982), Fischhoff (1982), Nisbett et al. (1982), Evans (1989), and Lopes (1987) for further information on debiasing. Research has also been conducted into ways of improving recollection of real-world events; for example, police have an obvious interest in eyewitness recall, and Roy (1991) describes how the 'cognitive interview' has been shown to improve eyewitness recall. Four questioning strategies are used, which aim to enhance memory retrieval: the witness is encouraged to reinstate mentally the external scene and the internal thoughts that existed at the time of the crime; he or she is asked to report everything, even incomplete or apparently trivial information; events are recounted in a variety of orders; and the witness is encouraged to report events from a variety of different perspectives. The cognitive interview has been shown to facilitate retrieval of more correct information than either the standard police technique or hypnotic techniques.

In the meantime, this paper can only provide a few guidelines for coincidence research: where possible, try to get an estimation of the likelihood of a coincidence (the formulae given when discussing the birthday problem may be helpful here); search for hidden causes; guard against predictions with multiple endpoints, by documenting predictions when they are made and noting failures to confirm predictions; ask whether the interpretation of a coincidence might have been influenced by the use of representativeness and availability heuristics, especially where judgements of likelihood and causality are concerned; have several people (ideally with differing prior beliefs about coincidences) document thoroughly coincidences, to try to some extent to circumvent belief-confirming distortions in perception, judgement and memory; beware of mis-

leading a witness with carelessly phrased questions, and explore techniques (such as the 'cognitive interview') that have been successfully put to practical use by investigators in other fields.

Lest readers think that I'm being too negative about coincidences, I should stress that an understanding of the sorts of factors that can cause us to come to the wrong conclusion about coincidences, and about psi in general, works both ways. This is especially true for situations where belief plays an important role in our judgements and recall of what happened. So, just as someone who really has a strong belief that he or she is a psychic may interpret and recall events so as to back up that belief, similarly someone who's a strong goat - who strongly believes that he or she has absolutely no psychic talent whatsoever - may also interpret and recall events so as to fit with his or her belief. In that case, although something paranormal may be going on, it doesn't get acknowledged as such. Either way, an understanding of human perception, judgement and recall under uncertainty may help us, both to weed out coincidences in which no psi was involved, and to retain those which are of more direct interest to the psychical researcher.

Also, many of the points made in this paper are most relevant for the evaluation of everyday experiences of coincidences. I have not dealt with possible techniques for the deliberate quantitative assessment of coincidences, but would refer the reader to an intriguing paper by William Braud (1983) suggesting a possible methodology for such an assessment.

Looking back to my introductory paragraph, which asked what makes some concurrences of events remarkable and not others, it seems clear that beliefs, expectations and personal involvement play a large role in our amazement at coincidences. You should hear what happened to *me* the other day...

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Department of Psychology
University of Edinburgh
7 George Square
Edinburgh EH8 9JZ
SCOTLAND, UK

Psychologie et coïncidences

Résumé: Cet article présente une revue sélective de la recherche suggérant des causes normales possibles à certaines coïncidences. Après une brève discussion des causes cachées, des prédictions à issues multiples, et de simple probabilité, l'ensemble de l'article se centre sur la recherche psychologique sur le jugement et la prise de décision en situation d'incertitude. On examine les raccourcis utilisés dans le traitement d'information jugés responsables des faiblesses apparentes de nos intuitions statistiques quotidiennes, ainsi que les critiques de ce paradigme d'heuristiques et biais. On donne des exemples d'études montrant comment la perception, le jugement et le rappel peuvent être biaisés afin de confirmer nos préjugés. Certaines implications de cette recherche pour l'étude des coïncidences sont soulignées, ainsi que la recherche suggérant des mesures afin d'améliorer le jugement de façon prometteuse.

Amplifying Precognition: Two Experiments with Roulette

Wilfried Kugel
Berlin, Germany

Abstract: It is well known that subjects asked to produce random decisions will show preferences and patterns in their choices. The present paper is an attempt to take such patterns into account for subjects playing actual roulette games in a casino. A computer program was prepared to incorporate a subject's earlier choices and their outcomes in making its own bets on certain trials when the appropriate statistical criteria were met. Two studies were completed, comparing subjects' hit rate with the program's. Study One used a small toy Roulette wheel in informal conditions. In 14 of 20 sessions, the hypothetical winnings of the program would exceed those of the subjects ($p < .06$, one-tailed). Study Two was done in a Berlin casino. The program performed considerably better than subjects; the program's hit rate was significantly positive (169 out of 313 trials, $p < .03$, one-tailed).

Introduction

It is an old dream to find a method of predicting the outcome in games of chance.¹ So, in March 1971, the author made such an attempt, and performed an experiment with a selected subject, giving her, under hypnosis, the suggestion that she was displaced one week ahead in time. She was asked to tell what the actual winning numbers of the German lottery, Lotto were (6 out of 49). Then, the author bought a Lotto ticket, crossing out the numbers given by the subject. Four numbers appeared to be correct and a small prize was won.²

Several attempts have been made to amplify subjects' possible precognition abilities in game situations. In the 1950s, Milan Ryzl (1971) performed experiments to predict the winning numbers in a lottery, and developed a (complicated) statistical method to amplify the subject's perform-

ance (pp. 70-81). In the 1960s, Robert Brier, together with the publisher of the journal *Rouge et Noir*, performed some experiments at the casino of a US hotel (Brier & Tyminski, 1970), also trying to amplify the performance of subjects with the help of a statistical method. Both claim to have won money with the help of their methods.

Under the non-ESP hypothesis, the outcome of a game of chance is completely random, no matter what strategy the subject applies. Under the assumption that precognition might be effective, future events must correspond to the subject's behavior in the present (and past). Thus, in the case of roulette, random events should be detectable in the subject's response behaviour. Furthermore, if the subject acts in a way that is completely strategy-dominated, it can be assumed *a priori* that no precognition is involved.

Knowing that a betting game seems to be a highly motivating test situation for subjects, and that it might be promising to filter the information output at the subject's side (bets) with the help of statistical methods, led the author to develop a computer program that can perform real-time calculations during a roulette game and so may

¹ Questions arising from observing the passion of French aristocrats for gambling led Blaise Pascal (1623-1662) and Pierre Fermat (1601-1665) to lay the foundations of the theory of probability.

² This experiment was not published. The author conducted only one experiment of this kind; a tape-recording of it still exists.

be able to amplify precognitive information in a betting situation.

Response Patterns

Reichenbach (1949) stated that mathematically naive subjects would not be able to produce a random series of alternative symbols taken from a set. This hypothesis was confirmed in several psychological investigations (see Tune, 1964).

In test situations, subjects, when forced to produce random decisions, show preferences and aversions. Furthermore, the subjects avoid repetitions of the same choice (Mittenecker, 1958). Reinforcing the response (feedback) leads the subjects to behave in the opposite way; most of them prefer to repeat the same choice.

The author described the latter effect in 1972 in statistical ESP experiments, and called it 'feedback-susceptibility'. It was first detected in forced-choice (5 alternatives) telepathy experiments when hit scores were calculated for lags between target and response from -3 through +3. Responses with a lag of -1 showed highly significant hit scores, which appeared only due to the fact that most of the subjects tended to repeat the same choice after positive feedback (a hit) and to alter the choice after negative (no) feedback (a miss). This response pattern was investigated over several years and was significantly detected in all experiments that the author performed at the Technical University Berlin research project (Kugel, 1972-1980a).

Thus, every chain of the subject's responses shows a well-proportioned structure, characteristic for the individual like a fingerprint and determined by a strategy. If one takes the subject's decisions as the output of an information receiver, it is clear that transmitted information cannot manifest itself when the subject's behaviour is completely guided by a non-variable, pre-formed pattern; in other words, the receiver has no oscillator circuit.

As is known from quantitative and qualitative ESP experiments (Tenhaeff, 1976), correct readings as well as high hit scores seem to depend on individual habits

of the subjects. Such habits might be imagined as reasoning in the personality structure of the subjects. Habits manifesting themselves in the form of response patterns were investigated by several experimenters. In his 1978 experiments, the author found significant dependencies between strong repetition avoidance and high depressivity (FPI), low permeability (GT) and low social potency (GT), and furthermore between strong feedback-susceptibility and high dominance (GT) and high discrepancy between introspection and extraneous observation with regard to the subject's social resonance (GT).³

Response patterns vary slightly with time (in lab experiments, probably due to tiredness). It is possible that this time dependence of response patterns is also specific for the subject, but no investigations of this problem are known to the author.

In some conflict situations, subjects might change their behaviour patterns dramatically ('Übersprunghalten'); this may be an evolutionary mechanism, activated if common behaviour strategies fail to bring the results wanted or stand in conflict with each other. It is interesting that ESP very often appears in crisis situations. In such situations (e.g., near-death-experiences), learned behaviour patterns sometimes fail to be useful. So other, maybe more randomly determined, behaviour structures can be activated. Activating random patterns may lead to a more sensible transmitting and/or receiving oscillator circuit in the subject, resulting in ESP as an emergency information transfer. A concrete indication for such a mechanism is the habit of many people to cast lots when they want to know more about the future - they use chance.

Of course the experiments reported here have nothing to do with crisis situations. The subjects just played a game. But most of them did not stupidly perform pre-formed response patterns. There are slight random changes in the response structures,

³ The Freiburger Persönlichkeits-Inventar (FPI) and Gießen-Test (GT) are common German personality tests.

maybe the microscopic roots of 'spontaneous alternation'. In the studies reported here, it was attempted to detect and use these effects.

Measuring Response Patterns

Every hypothesis about response patterns has to be transformed into a hypothesis about relative frequencies of such patterns in the response series. Information theory, especially in real-time experiments, is a powerful tool for calculation of values that represent the strength of strategy application.

Construction of a Measure

There is a complete set q of n relative frequencies (of patterns), which represent estimates for n probabilities:

$$q = \{q_1, q_2 \dots q_n\}, \sum_{i=1}^n q_i = 1$$

There is another set p of probabilities, namely the reference set:

$$p = \{p_1, p_2 \dots p_n\}, \sum_{i=1}^n p_i = 1$$

In my application of the theory the set p can be seen as a set of predictions of relative frequencies of higher order, calculated from observed lower order relative frequencies. In the case of order 0 the set p is given by *a priori* probabilities. The set q represents the corresponding observed relative frequencies.

The more strategy patterns appear, the greater the difference between the 2 probability sets. It is now necessary to develop a measure for the difference of the 2 sets. We thus need a functional

$$U(p_i, q_i, i = 1 \dots n)$$

which equals 0 if all $q_i = p_i$, and increases with the difference of the 2 probability sets.

Feinstein (1958) gives a lemma (p.13) for comparing 2 probability sets using entropy measures H :

$$H_{q,q} = -\sum_i q_i \log_2 q_i \leq -\sum_i q_i \log_2 p_i = H_{q,p}$$

with the equality $H_{q,q} = H_{q,p}$ if and only if $p_i = q_i$ for all i .

This allows us to define the functional $U = H_{q,p} - H_{q,q}$ (Kugel, 1979):

$$U = \sum_i \left[q_i \log_2 \left(\frac{q_i}{p_i} \right) \right]$$

which is always non-negative and increases with the divergence between the 2 sets of probabilities. This U -value is independent of the number of trials; it rises and falls with the actual divergence between the 2 probability sets and can be used to measure patterns in the subject's response series with respect to a given reference. It can easily be calculated in real time.

Three Applications on the Two Alternative Cases ($n = 2$)

(1) *Frequencies (transition probabilities of the Order 0)*. In a decision situation, every subject tends to prefer or avoid one of the alternatives. In the extreme case of a strong response strategy, only one alternative is chosen.

The following measure is useful to detect the favouring or avoiding of alternatives. In the case of 'even chances' at roulette, the reference set of *p a priori* is set to

$$p_i = const. = \frac{1}{2}$$

because subjects play in a 2-alternatives forced choice situation.

The estimates for the probabilities q (relative frequencies) are calculated by

$$q_i = \frac{N_i}{N}$$

where N is the total number of trials and N_i is the frequency of alternative i in the response series.

Thus the 2 probability sets are, in the 2 alternative cases:

$$p = \left\{ \frac{1}{2}, \frac{1}{2} \right\}$$

$$q = \left\{ \frac{N_1}{N}, \frac{N_2}{N} \right\}$$

The corresponding U measure is called U_a .

(2) *Frequencies of equal and unequal pairs (transition probabilities of the Order 1).* Investigating the alternation of choice, one finds that the subjects usually alternate much more than expected by chance. They avoid repetitions of the same choice. This behaviour is also observed in rats: on the second of 2 consecutive trials in a T-maze, a rat typically enters the alley that was not visited on the first trial. This phenomenon is called 'spontaneous alternation' (Douglas, 1966). The same results can be obtained when both alternatives are unrewarded or when either alternative is equally rewarded. 'Spontaneous alternation' has also been observed in *Lumbricus terrestris* and in ants. A lot of research has been done to find the factors that determine 'spontaneous alternation'. It seems to be triggered by an internal stimulus and to be a basic function of living systems interacting with their environment.

The following measure is useful to detect whether more pairs of unequal responses (repetition avoidances) or pairs of equal responses (repetitions) appear. We have to distinguish 2 cases, 'equal pairs' (case a) and 'unequal pairs' (case b). For

estimating the p set we do not use *a priori* probabilities, but the combined relative frequencies, calculated in (1):

$$P_{ai} = \frac{N_i}{N} \frac{N_i}{N} \text{ and } P_{bi} = \frac{N_i}{N} \frac{(N - N_i)}{N}$$

Because the number of cases in experiments is usually quite low, the i cases are summed up for groups a and b :

$$p_a = \sum_i p_{a i} \text{ and } p_b = \sum_i p_{b i}$$

resulting in the set

$$p = \{p_a, p_b\}$$

for equal pairs (a) and unequal pairs (b).

The estimate for the probability set q (relative frequencies) is calculated by

$$q_a = \frac{G}{(G + H)} \text{ and } q_b = \frac{H}{(G + H)}$$

where G represents the frequency of equal pairs, and H the frequency of unequal pairs in the response series.

Thus the 2 probability sets are, in the two alternative cases:

$$p = \left\{ \left(\frac{N_1^2}{N^2} + \frac{N_2^2}{N^2} \right), \left(\frac{N_1(N - N_1)}{N^2} + \frac{N_2(N - N_2)}{N^2} \right) \right\}$$

$$q = \left\{ \frac{G}{(G + H)}, \frac{H}{(G + H)} \right\}$$

The corresponding U -measure is called U_b .

(3) *Feedback response.* Positive feedback can be seen as a reward. Thus, the rewarded choice creates in the subject a tendency to repeat that choice. But feedback is an external stimulus that contradicts the internal stimulus for 'repetition avoidance'. Feedback thus causes 2 different effects. On the one hand, feedback-trig-

gered response is a strategy, and as a strategy it weakens the potential manifestation of psi-mediated information in the subject. On the other hand, feedback-triggered response weakens the dominant strategy of 'repetition avoidance'. This double effect may very well explain the controversy of whether or not feedback increases psi performance.⁴

The following measure is useful to detect whether the subjects respond to feedback.

(3.1) *Equal and unequal pairs when the first member is a hit.* We have to distinguish between 2 cases, that of equal pairs (feedback-susceptibility, case *a*) and that of unequal pairs (case *b*). For estimating the *p* set, again, we do not use *a priori* probabilities, but the combined relative frequencies, calculated in (2):

$$p_{a I} = \frac{K}{N} \frac{G}{(G+H)} \text{ and } p_{b I} = \frac{K}{N} \frac{H}{(G+H)}$$

where *K* is the total number of hits.

The estimate for the probability set *q* (relative frequencies) is calculated by

$$q_{a I} = \frac{F}{(F+A)} \text{ and } q_{b I} = \frac{A}{(F+A)}$$

where *F* represents the frequency of equal pairs, *A* the frequency of unequal pairs in the response series, under the condition that the first member is a hit.

(3.2) *Equal and unequal pairs when the first member is a miss.* Again, we have to distinguish between 2 cases, equal pairs (case *a*) and unequal pairs (case *b*). For estimating the *p* set, again, we do not use *a priori* probabilities, but the combined relative frequencies, calculated in (2):

$$p_{a II} = \frac{(N-K)}{N} \frac{G}{(G+H)}$$

and

$$p_{b II} = \frac{(N-K)}{N} \frac{H}{(G+H)}$$

The estimate for the probability set *q* (relative frequencies) is calculated by

$$q_{a II} = \frac{L}{(L+M)} \text{ and } q_{b II} = \frac{M}{(L+M)}$$

where *L* represents the frequency of equal pairs, and *M* the frequency of unequal pairs in the response series, under the condition that the first member is a miss.

(3.3) *The probability sets.* Because the author found in experiments that repetition after a hit always goes with repetition avoidance after a miss, these 2 cases are to be merged. The same has to be done with the opposite pattern, repetition after a miss and repetition avoidance after a hit:

$$p = \{(p_{a I} + p_{b II}), (p_{a II} + p_{b I})\}$$

$$q = \{(q_{a I} + q_{b II}), (q_{a II} + q_{b I})\}$$

Thus the 2 probability sets are, in the 2 alternative cases:

$$p = \left\{ \left(\frac{K}{N} \frac{G}{(G+H)} + \frac{(N-K)}{N} \frac{H}{(G+H)} \right), \left(\frac{K}{N} \frac{H}{(G+H)} + \frac{(N-K)}{N} \frac{G}{(G+H)} \right) \right\}$$

$$q = \left\{ \left(\frac{F}{(F+A)} + \frac{M}{(L+M)} \right), \left(\frac{L}{(L+M)} + \frac{A}{(F+A)} \right) \right\}$$

The corresponding *U*-measure is called *U_c*.

Implementation into a Program

In a betting situation, for every single trial *j*, 3 values can be measured: the subject's prediction (1 or 2 with regard to the colours *noir* or *rouge*), the subject's wager on this prediction, and the colour of the random number (1, 2 or 0) generated after the subject's bet. After a certain number of trials, enough values have accumulated to

⁴With the construction of the measures *U_b* and *U_c* the author tried partly to separate the 2 different effects.

calculate the 3 U -values derived above, using the data from all previous trials ($1...j$), with the help of a computer program. For the actual trial j it can be determined whether or not the U -values are rising or falling with respect to the preceding trial ($j-1$):

$$\begin{aligned} U_a(j) > U_a(j-1) \text{ or } U_a(j) \leq U_a(j-1) \\ U_b(j) > U_b(j-1) \text{ or } U_b(j) \leq U_b(j-1) \\ U_c(j) > U_c(j-1) \text{ or } U_c(j) \leq U_c(j-1) \end{aligned}$$

This leads to $2^3 = 8$ different possible strategies. For each of the 8 strategies, the number of corresponding hits (positive sign) and misses (negative sign), weighted with the related (square⁵ of) subject's wager were stored in the program's session memory.

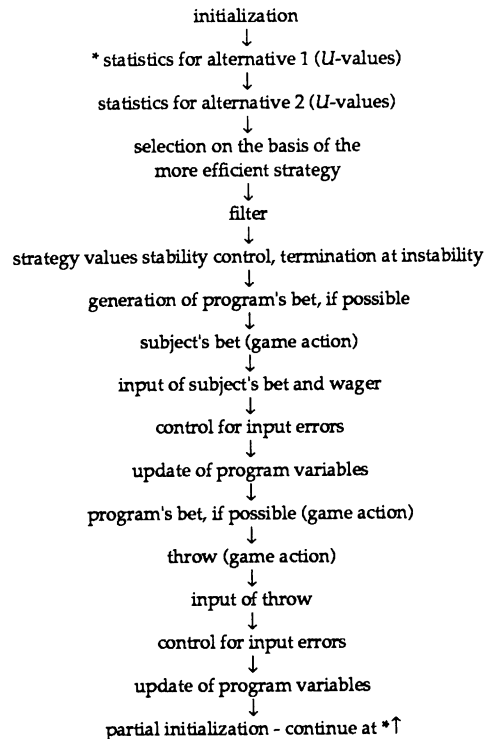
For the forthcoming trial ($j+1$), we can now calculate both alternatives of changes in the U -values with respect to each of the 2 possible predictions that one could make⁶. Thus, one can decide which of the possible 2 predictions would represent the better strategy, with regard to the effectiveness of all strategies applied by the subject during the previous trials. The program's prediction for the forthcoming trial does *not* consider the subject's prediction for the forthcoming trial, but acts completely independently, only on the basis of the data of the previous trials.

In some program versions (from version 2 on), filter conditions were applied to suppress random fluctuation. (These conditions cannot be described here because the corresponding threshold values were updated after every session and so differed

from session to session. Furthermore, the filter conditions were modified from program version to program version.) But in any case, the filter suppressed predictions when not all values were defined (during the build-up of statistics in the beginning of the session) or when some values were equal to each other.

The only termination condition for study 1 was the completion of 20 sessions of 50 trials each. Termination conditions for the session in study 2 consisted of: program errors, input errors by the experimenter, the session time being longer than 1 hour, the real game being performed faster than the calculation time of the computer, unstable strategy values, an uncomfortable situation at the casino, or a request from the subject to stop. Of course, this means 'optional stopping'. But at the casino, we were only in a 'study' and no limits for either the number of trials or for the program's predictions were set.

The following diagram shows the chain of events in a session (input errors were checked to prevent program errors because of the experimenter's computer input errors):



⁵The wager can be seen as a variable indicating how sure the subject was about the single prediction. The square of the wager was used to amplify the differences between wagers from trial to trial.

⁶In program version 1 (Study 1), for every trial a total $U = U_a + U_b + U_c$ was calculated. The program then always predicted the colour for which $U(j+1) \leq U(j)$. For the first trials, when not enough data for calculation were available, the program simply took the same choice as the subject. Since program version 2, the 3 U -values were handled separately.

The Experiments

During the author's work at the PSI-lab at the Technical University Berlin from 1972 to 1981, several experiments were performed, searching for a relationship between strategy application and ESP score. The results were not stable, but they did indicate that subjects with weaker response patterns performed better (Kugel, 1976, 1977b, c, 1978, 1980a).

Study 1 (Technical University Berlin)

The following was the last experimental series performed at the Technical University Berlin PSI-lab. It was published in the last research report of the project (Kugel, 1980a).

In September 1980, the author introduced a computer program (version 1, in FORTRAN) to predict even chances in a roulette game, using the *U*-values as predictor variables. A toy roulette device⁷ was used. The program was run via telephone modem on a CD CYBER 172 of the Technical University Berlin computer centre.

Twenty subjects were tested, only one session per subject was allowed, and each session consisted of 50 trials. The subject could bet any (hypothetical) wager on *rouge* or *noir*. During the first trials of a session, the program had to build up its statistics: it simply made the same predictions as the subject.⁸ After the variables were defined,

⁷The roulette wheel had no manufacturer listed other than "Made in Italy"; the wheel was approximately 12" (30 cm) in diameter.

⁸This method was used because the same number of predictions was wanted for the subjects and the program. (There was no bet that the subject made without a corresponding bet by program.) Of course, those trials in which the program was just placing the same bet as the subject should not be included in the analysis. But for this experiment, we had to do so because it was fixed in the methodological set-up prior to the experiments (to make the statistics easier). The hypothesis only stated that the program would have more hits in the majority of sessions. Under this hypothesis it does not matter if subject and program make the same bet for a while because the difference

the program started to calculate its own prediction (not taking the subject's actual bet into account) and made its bet after the subject had done so. The program always bet the same wager as the subject, no matter whether its prediction differed from the subject's prediction or not. Then the experimenter threw the ball. When a 0 appeared, the ball had to be thrown once more.

There were 2 hypotheses:

H1: In the majority of sessions the program's hit score will be higher than the subject's.

H2: In the majority of sessions the program's winnings⁹ will be higher than the subject's.

It was decided in advance that 1-tailed tests would be used, with 0.05 as the criterion level of statistical significance.

In 13 sessions, the program performed better than the subjects, and in one session, both obtained an equal number of hits. Only in 6 cases did the subjects perform better than the program. (H1: $p=0.084$)

In 14 out of 20 sessions the (hypothetical) winnings of the program were higher than those of the subjects (H2: $p=0.058$).

Though the hypotheses could not be accepted, the results looked promising. The program's hypothetical winnings were about 20 times higher than those of the subjects. The overall results are shown in detail in Table 1.

Table 1

Overall results for subjects and computer program

Total number of trials:	900
Chance expectation for hits: (<i>a priori</i> probability 1/2)	450
Program's hits: ($p = 0.27$, 1-tailed).	459
Synchronous result for subjects:	446

between the program's and the subject's hits in these cases equals 0.

⁹ Winnings = monetary prizes (toy money).

Surprisingly, the randomness of the cheap toy roulette device appeared to be very good. Using different tests, no significant deviations from chance expectation could be detected in the target series (compared using the χ^2 test), as shown in Table 2.

Table 2
Outcome of randomness tests of roulette device

	<i>p</i>
Frequencies	0.68
Sequences (Kugel 1980b)	0.58
Nonsequences (Kugel 1980b)	0.61
Transition frequencies	
lag of 1 between call and target	0.69
lag of 2 " " " "	0.91

The results are shown further broken down by session in Appendix 1.

Study 2 (Berlin Casino)¹⁰

In 1981 programmable pocket computers became available in Germany. A shorter version of the original program was developed (version 2, in BASIC) for the Sharp PC 1211 (with 1.4K memory). It was very difficult to fit the program into the very small memory, so only the most important calculations could be made. The actual decision about the prediction (on the basis of the 3 *U*-values) had to be done by the experimenter during the session. The computer's calculation time for one trial was about 90 seconds. In some sessions, because of the long calculation time, the game at the casino table ran faster than the calculation time and the session had to be terminated. Several program versions (2-4) for the Sharp PC 1211 were developed. Since 1982 a Sharp PC 1500 (with 10K memory) was used. The calculation time could be lowered to 7 seconds. For this computer, program version 5 was developed.

¹⁰This study was sponsored privately.

The subjects got their total money for wagers (300 DM, later 600 DM¹¹) from the experimenter. They had no financial risk, because they could not lose any of their own money. Additionally, they got half of the session's net win from the experimenter. The subjects could bet any sum on even chances. The experimenter fed all the data into the computer. When the program made predictions, the experimenter made this bet *after the subject had placed the bet*, thereby reducing the influence of the program on the subject.

Working with selected subjects at the Berlin Casino, 39 sessions were performed from 1981 to 1984. In 5 sessions the program could not make any predictions. In 6 of the remaining 34 sessions, program and subject had the same number of hits. From the now-remaining 28 sessions, in 15 cases the program performed better than the subjects, which is near chance. But the total number of the program's hits was, with 17 above chance expectation, significantly raised. See Table 3 for the results in detail.

Table 3
Overall results for subjects and computer program

Total number of program's trials:	313
Chance expectation for hits: 152.3 (<i>a priori</i> probability 18/37)	
Program's hits: (<i>p</i> < 0.03, 1-tailed).	169
Synchronous result for subjects	152

The program won 1,095 DM; the subjects lost 615 DM.

For random analysis, random numbers were reduced to 1 for *noir* or *manque*, 2 for *rouge* or *passe*, and 0. No significant deviations from chance expectation could be detected in the target series; see Table 4.

¹¹The amount of money the subjects could play with had to be raised because with only 300 DM, in some sessions, not enough games could be performed to use all predictions of the program: the session would be terminated because the subject went bankrupt.

Table 4
Outcome of randomness tests for casino roulette wheel

	<i>p</i>
Frequencies (χ^2)	0.53
Transition frequencies (using SPSS 'Crosstabs' and Pearson χ^2)	
lag of 1 between call and target	0.82
lag of 2 " " " "	0.80
lag of 3 " " " "	0.56
lag of 4 " " " "	0.87

The results are shown further broken down by session in Appendix 2.

Discussion

This report covers all roulette experiments ever carried out by the author. All original data and programs had been stored and were re-examined.

The experiments at the Technical University Berlin had to be terminated because the research project was closed in 1981. The casino sessions were terminated because the subjects became increasingly bored by the uncomfortable situation at the casino, which could be regarded as a form of 'optional stopping'. But the results are promising enough to demand for more research.

In both studies, subjects scored collectively slightly below chance, whereas the program scored above chance, and in the case of the second study even significantly above. This indicates that the algorithm implemented into the program was able to detect an influence of the target information on the subject's behaviour structures. The program was able to use these detected structures to generate its own predictions, which appeared to be more effective than the subject's own predictions.

Under the assumption that the results reported are not caused by chance, one could interpret the phenomenon observed as an information transfer from the future

into the past, detectable by a computer program. Another interpretation would be that the subjects may have influenced, via PK, the generation of the target numbers. However, under the latter assumption, subjects would have to focus their interest not on their own bets but on the program's bets, which they of course knew before the generation of each target. (They had to know it because otherwise the experimenter could not bet on the program's predictions at the casino.) To exclude this interpretation it would be useful to perform experiments in which the subjects do not know the program's predictions.

One could also argue that the program might simply have been good in picking up small nonrandom fluctuations in the target series, but in the case of the two studies reported, the randomness of the target series was excellent. There was not the slightest indication of nonrandom effects. Nevertheless, the author will soon perform 2 simulations where artificial response series will be computed as (a) a random series and (b) a highly structured series. Then the program has to generate its predictions. But the author expects the program's hit-scores to be near chance in both cases.

However, measurement of strategy application is necessary because of 2 reasons. First, it is useful for selecting subjects: since it is now fairly certain that personality structure affects ESP performance, it may be possible to investigate complex psychological structures via strategy patterns. Second, it would be necessary to structure statistical ESP experiments so as to fit the personal habits of the individual subject. This means that one has to measure the behaviour/strategy patterns of the subject prior to the experiment. Then the experiment should be set up to allow the subject to perform normal response patterns. This method should lead to a reduction of statistical noise and thereby to an improvement in measuring weak effects.

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KUGEL

Appendix 1

Study 1
(Technical University Berlin, 1980)

Subject	Date	SUBJECT - DATA				PROGRAM - DATA			
		<i>N</i>	<i>K</i>	<i>DM</i>	<i>k(NC)</i>	<i>NC</i>	<i>KC</i>	<i>DMC</i>	<i>V</i>
WK	01.10.80	50			Data lost ¹²				
BB	01.10.80	50			Data lost ¹²				
RT	02.10.80	50	19	-1850	19	50	22	-250	1
HV	02.10.80	50	28	3500	28	50	22	12500	1
HL	04.10.80	50	32	3300	32	50	22	-1700	1
CZ	04.10.80	50	30	-6000	30	50	19	6000	1
GN	04.10.80	50	29	2100	29	50	25	-1100	1
MJ	06.11.80	50	25	2150	25	50	28	2850	1
FB	21.11.80	50	17	21100	17	50	23	-30000	1
JD	21.11.80	50	22	-27000	22	50	30	61500	1
RS	24.11.80	50	20	-4000	20	50	25	-2000	1
NH	26.11.80	50	25	-65000 ¹³	25	50	35	130000 ¹³	1
DB	26.11.80	50	31	66000	31	50	27	-14000	1
GS	26.11.80	50	27	-10500	27	50	27	6500	1
GL	26.11.80	50	21	-1400	21	50	30	4000	1
SN	05.12.80	50	26	-2900	26	50	20	1200	1
RN	09.12.80	50	24	33435	24	50	25	74295	1
RM	10.12.80	50	22	-2000	22	50	28	6200	1
HX	10.12.80	50	22	1000	22	50	24	-2400	1
MG	15.12.80	50	26	2750	26	50	27	450	1
Total		900	446	14685	446	900	459	254045	

Key to table

- N*: number of subject's trials
- K*: number of subject's hits
- NC*: number of program's trials
- KC*: number of program's hits
- k(NC)*: number of subject's hits synchronous with program's trials
- DM*: subject's winnings or losses
- DMC*: program's winnings or losses
- V*: program version

¹²Data lost by communication error between host computer and satellite.

¹³Data lost by computer breakdown. Numbers of hits were notated. *DM* and *DMC* were estimated by the experimenter immediately after the computer failure.

AMPLIFYING PRECOGNITION

Appendix 2

Study 2
(Berlin Casino, 1981-1984)

Subject	Date	SUBJECT-DATA				PROGRAM-DATA			
		N	K	DM	k(NC)	NC	KC	DMC	V
RT	27.03.81	10	7	30	3	3	2	65	2x
RT	28.03.81	50	28	105	13	19	9	-45	2
RT	29.03.81	21	14	115	4	7	5	20	2
RT	30.03.81	30	16	40	3	8	5	65	2
RT	31.03.81	31	9	-140	3	12	5	-20	2
Total		142	74	150	26	49	26	85	
RT	09.05.81	29	14	-10	3	8	7	50	3
RT	11.05.81	35	18	20	3	12	9	250	3
RT	12.05.81	30	12	-50	5	16	11	520	3
RT	13.05.81	30	12	-120	7	20	12	80	3
RT	14.05.81	30	14	-140	8	20	15	740	3
RT	16.05.81	20	7	-210	5	14	4	-380	3
RT	17.05.81	24	10	-110	6	10	7	160	3
RT	18.05.81	30	13	-90	10	22	8	-310	3a
JD	19.05.81	30	14	-30	9	20	10	-30	3a
RT	20.05.81	26	10	-55	5	14	7	-60	3a
RT	21.05.81	13	3	-70	0	4	2	20	3a
Total		297	127	-865	61	160	92	1040	
RT	11.06.81	29	16	10	6	10	6	100	4
RT	12.06.81	24	12	0	3	5	4	150	4
RT	13.06.81	12	5	-10	0	0	0	0	4
RT	14.06.81	44	23	10	13	17	8	-40	4
RT	15.06.81	19	11	15	1	1	0	-40	4
WK	16.06.81	15	4	-45	0	2	0	-30	4
RT	08.07.81	55	30	55	9	11	3	-180	4a
RT	09.07.81	75	38	-10	9	18	7	-160	4a
RT	11.04.82	6	2	-10	0	0	0	0	4b
RT	12.04.82	9	5	0	1	1	0	-10	4b
RT	13.04.82	27	9	-50	2	7	5	25	4b
RT	14.04.82	13	5	-10	0	1	0	-15	4b
Total		328	160	-45	44	73	33	-200	
PF	05.09.82	20	11	125	4	4	4	130	5
PF	07.09.82	10	5	10	0	0	0	0	5
PF	08.09.82	13	6	-25	2	2	1	-20	5
RT	19.12.82	21	12	20	3	6	3	-40	5a
P	24.08.83	12	7	-10	0	0	0	0	5b
VF	24.08.83	20	11	0	2	5	4	300	5b
VF	25.08.83	21	9	-80	3	5	2	-100	5b
VF	26.08.83	20	15	200	6	6	4	200	5b
VF	29.08.83	12	5	-40	1	1	0	-100	5b
VF	31.08.83	12	6	-20	0	0	0	0	5b
VF	09.02.84	17	8	-35	0	2	0	-200	5b
Total		178	95	145	21	31	18	70	
Grand Total		945	456	-615	152	313	169	1095	

On *Rouge/Noir* was played with programs 1-4a, and 5; On *Manque/Passe* was played with program 4b and on 9/5/82 and 9/8/82.

Richard-Wagner-Str. 7
D-1000
Berlin 10
GERMANY

Amplifier la précognition: Deux expériences avec roulette

Résumé: Il est bien connu que des sujets à qui l'on demande de produire des décisions aléatoires montrent des préférences et des patterns dans leurs choix. Le présent article tente de prendre en compte de tels patterns chez des sujets jouant à des véritables jeux de roulette dans un casino. On a mis au point un programme informatique auquel on incorporait les choix antérieurs d'un sujet et leurs résultats, à partir de quoi il effectuait des paris sur certains essais une fois les critères statistiques appropriés rencontrés. Deux études ont été menées comparant le taux de réussite des sujets à celui du programme. Dans l'étude n°1 on utilisait un petit jeu de roulette dans des conditions informelles. Les gains hypothétiques du programme auraient dépassés ceux des sujets dans 14 sessions sur 20 ($p < .06$, unilatéral). L'étude n°2 a été menée dans un casino de Berlin. Le programme a fait beaucoup mieux que les sujets. Le taux de réussite du programme était positif de façon significative (169 essais sur 313, $p < .03$, unilatéral).

Enhancing Effects in Psi Experiments with Sequential Analysis: A Replication and Extension

Dean I. Radin

Institute for Advanced Study at Waltham
Waltham, Massachusetts, USA

Abstract: Several studies have demonstrated that small effects observed in psi experiments can be enhanced through the application of statistical averaging procedures such as majority voting. In one such study, a technique known as sequential analysis was used to increase the effective hit rate in a pseudorandom number generator experiment. Sequential analysis is an attractive alternative to the more common fixed length procedure, because it provides greater statistical power with fewer trials. The present study was a conceptual replication of the use of sequential analysis in a psi experiment, with the addition of a novel bit-weighting scheme designed to enhance further the obtained hit rate. The experiment, consisting of 5 blocks of 1,000 sequential analysis trials each, produced by a single subject, showed evidence for psi and replicated the finding that statistical averaging techniques can be used to enhance the 'raw' hit rate. The results also suggested that the use of a bit-weighting scheme in conjunction with sequential analysis may be a more promising area to explore in further amplifying hit rates in similar experiments.

Introduction

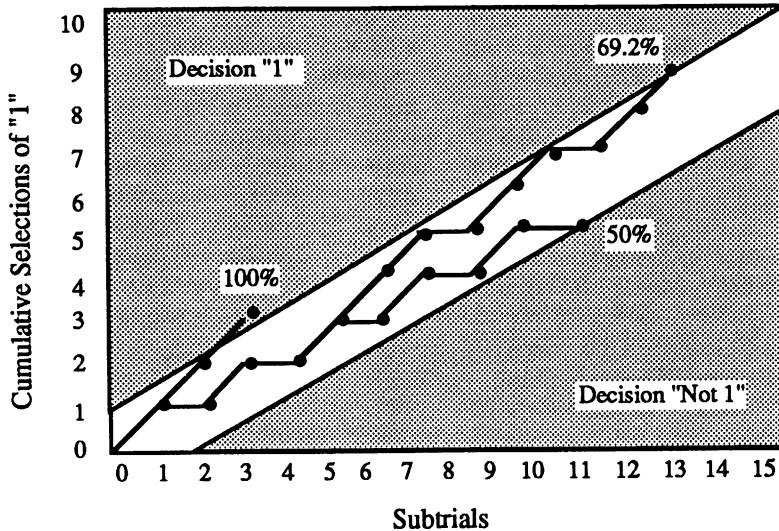
The essential goal of most psi experiments involving random number generators (RNGs) is to cause the RNG to generate outputs so that the resulting distribution mean is shifted from chance expectation in accordance with pre-assigned directions. Meta-analyses of the relevant experimental literature show that although the observed effect is statistically unequivocal, it seems to be stochastic in nature, and the magnitude of the mean shift is quite small, generally fractions of a percent over the chance hit rate (Honorton, 1978; May, Humphrey, & Hubbard, 1980; Radin & Nelson, 1989). Whereas a genuine psi effect of any magnitude is of theoretical importance, it seems likely that most practical applications of these effects, as well as greater scientific interest, will require results that are much greater in magnitude. Indeed, the following critic's opinion probably reflects that of many (uninformed but well-meaning) scientists:

Sensible people will simply say that as long as the effects are minuscule, there is in all probability some combination of perfectly ordinary causes producing the effects, and no matter what the p value is, and no matter how scrupulous the experimental controls are, there is no reason to take the paranormal seriously. (Glymour, 1987, p.590)

Higher hit rates may be accomplished in at least 3 ways: by gaining a better understanding of the stochastics of the psi process through further research, by selecting subjects who have a history of superior performance on similar tasks, and by applying statistical averaging procedures to extract a presumed 'signal' from noise. This paper addresses the third approach.

A number of researchers have investigated ways of applying statistical averaging procedures to boost effective hit rates in psi experiments (see Morgan & Morris, 1991). For example, one such attempt was reported by Ryzl (1966), who devised a complicated repeated-calling procedure

Figure 1
 Sequential sampling decision graph (after Puthoff, May, & Thomson, 1985, p.295)



that turned 19,350 binary trials, averaging a 62% hit rate¹, into the successful decoding of 15 decimal digits, without error. In the 1970s, Brier and Tyminski (1970a, b) and Dean and Taetzsch (1972) reported the use of averaging techniques to predict results in casino games. In the 1980s, Carpenter (1982) successfully transmitted the Morse code equivalent of the word 'peace', and Puthoff (1985) guessed hidden roulette-wheel spins and coin flips. All of these studies were reportedly successful, suggesting that psi effects - even small magnitude, stochastic effects - can be effectively amplified by the use of straightforward majority-vote averaging techniques.

More recently, Puthoff, May, and Thomson (1986) described the use of a less well-known statistical method, called 'sequential analysis' (Wald, 1973). This variable-length sampling procedure, first suggested by Taetzsch (1962) for use in psi studies, offers several advantages over conventional fixed-length tests such as those tested with *t* or *Z* scores. First, it provides the equivalent statistical power of

fixed-length tests with only about half the number of trials. Second, it allows one to define the *a priori* chance hit rate ($p_0=0.5$ for a binary task), the presumed 'psi' hit rate (typically $p_1>.5$ for a binary task), and the desired Type I (α) and Type II (β) error. And, third, the variable-length, but not optional stopping, nature of this technique may help to offset the undesirable psychological condition of 'performance anxiety', which may result from knowledge that a fixed-length test is nearing its end point.

Sequential Analysis

Because sequential analysis is not as well-known as conventional statistical tests, I will digress a bit to describe how it works. Prior to beginning a sequential analysis process, one selects *a priori* values for p_0 , p_1 , α , and β . These values are plugged into equations² that create a decision graph similar to that shown in Figure 1. In the following example, we decide to test the hypotheses, H_0 : '0 and 1 are equally likely

¹ By Pavel Stepanek.

² Appendix A shows the basic sequential analysis equations, from Wald (1973).

($p_0 \leq .5$) and H_1 : '1 is more likely ($p_1 \geq .7$)'. We also decide to use $\alpha = \beta = .3$.

Figure 1 illustrates the variable-length nature of sequential analysis and the 3 types of decision that result from this process. The term 'subtrial' in Figure 1 is equivalent to one binary sample. As shown, there are 3 regions produced in the sequential analysis decision curve. If the upper region is reached, sequential analysis determines that we are sampling from a distribution where $p \geq .7$ and $\alpha = \beta = .3$. That is, the decision '1' (or 'distribution is not chance') is concluded. If the bottom area is reached, sequential analysis determines that we are sampling from a distribution where $p \leq .5$ and $\alpha = \beta = .3$; the decision reached is therefore 'not 1' (or 'the distribution is chance'). While the accumulated data remain in the middle band, shown as the white diagonal in Figure 1, we continue to collect data, or 'pass' our decision on to the next trial.

The 3 lines shown in Figure 1 illustrate how these 3 choices can be reached. If 3 '1' bits are obtained in a row, this is viewed as a 100% hit rate (3 hits out of 3 trials). Because the third hit runs into the 'Decision "1"' region of the graph, sequential analysis concludes that we are sampling from a distribution with $p = .7$ or better. Thus, it declares the decision '1'. If the samples are more variable, but the cumulative hit rate remains above chance, sequential analysis will also conclude '1', although it will take more trials, e.g., the 69.2% hit rate shown in the graph took 13 trials before the line ran into the 'Decision 1' region. The third line shows that a pure chance hit rate (50%) took 10 samples to reach the decision 'not 1', or 'it seems we are sampling from a distribution in which $p = .5$ or less.'

In a binary psi experiment, a similar graph is also kept for the '0' bits. This results in four decisions: '1', '0', 'not 1', and 'not 0'. For simplicity, the last 2 results may be considered as 'I can't make up my mind' situation, in which sequential analysis

decides that the data collected so far are so close to chance expectation that it cannot unambiguously decide whether the sample comes from a distribution of mostly 1's or mostly 0's (where 'mostly' is defined as $p \geq .7$). By stopping the sampling process when a 'not 1' or 'not 0' decision is reached, sequential analysis in effect separates out the deviant binary sequences from sequences that hover more closely around chance. In other words, when a '1' or '0' decision is made, we can be fairly confident about that decision. And if we are fortunate, that decision will match the psi target.

An Experiment Using Sequential Analysis

In Puthoff, May, and Thomson's (1986) experiment, using the parameters $p_0 = .5$, $p_1 = .7$, $\alpha = .3$, and $\beta = .3$, 2 participants were asked to predict the outcome of future roulette-wheel spins. Each person contributed 500 trials using an HP41CX hand calculator that was programmed to generate one random bit per button press and to continue to collect data until a sequential analysis decision ('1', '0', or 'don't know') was reached. Puthoff, May, and Thomson's results showed that one participant performed near chance expectation, but the other person's scores improved from a 'raw' hit rate of 51.5%, to a significant 55.6% using sequential analysis applied to the sequence of bits.

Besides demonstrating statistical enhancement of a psi hit rate, Puthoff, May, and Thomson described an intriguing post hoc discovery. When they plotted hit rate (as a percentage) as a function of the number of binary subtrials to make a decision, they obtained significant positive slopes (with $p < .005$) for both participants' data. They speculated that this may have occurred because statistical power increases as the number of subtrials increases; thus a longer sampling sequence allowed for a smaller psi hit rate to be 'detected'. At first glance, this seems a plausible explanation, but because Puthoff, May, and Thomson used sequential analysis, the statistical power in their experiment remained con-

³The H_1 choice of $p = .7$ is arbitrary. It is set fairly high to help make a clear distinction between chance results and non-chance results.

stant regardless of the length of each trial. Thus, their discovery could not be explained by an effect related to statistical power. As an alternative, based on limited experimental work (Vassy, 1986, 1990; Radin, 1988), one might speculate that this effect occurred because the longer binary sequence lengths provided more information (in the sense of information theory [Shannon & Weaver, 1949]), and psi, envisioned as an informational process, may 'operate' more efficiently as the amount of information increases.

For example, imagine that you were asked to identify a photograph of a friend, but the photo had been cut up into a jigsaw puzzle. If you were allowed to see only a few pieces of the puzzle, it would be difficult to guess the identity of the person. But as more jigsaw pieces were added to the puzzle, it would become progressively easier to identify the person because more information was available to solve the problem. Perhaps psi, in a similar fashion, is more effective when there is more information that can be brought to bear on 'solving the problem'.

In any case, because of Puthoff, May, and Thomson's interesting finding about the relationship between hit rate and the number of subtrials to decision, I decided to replicate their procedure and explore an enhancement that might further improve the 'effort-to-decision' ratio. For example, in Puthoff, May, and Thomson's experiment, 15,421 individual subtrials were used to produce 1,000 sequential analysis 'decision' trials. The ratio of effort-to-decisions was therefore about 15:1. The other experiments mentioned above show similar effort-to-decision ratios, which is not surprising given the data-intensive nature of statistical averaging procedures.

Method

Procedure

Five blocks of 1,000 sequential analysis trials were planned in advance, with the author participating as sole subject. The calculator-based sequential analysis proce-

cedure used by Puthoff, May, and Thomson (1986) was recoded into C language and run on 2 types of UNIXTM workstations: Block 1 was conducted on a Silicon Graphics Iris 4-DTM, and Blocks 2-5 were conducted on a Sun Microsystems 4/260TM. Pseudorandom bits used in this experiment were generated by the pseudorandom number generator (PRNG) *drand48*, a standard, well-tested UNIXTM System V routine (Radin, 1985; Radin & May, 1987).

Data were collected over a period of a year and a half, beginning in early 1988 and ending in mid-1989. Usually 1 or 2 dozen trials were collected at a sitting. Sometimes many months passed before collecting new data; at other times a group of several hundred trials were completed in a single day.

No special mental preparation or disciplines were employed in conducting these trials. The usual approach was to take a few minutes out of my daily work schedule and use the test as a relaxing diversion. I presumed that to be successful at the task I would have had to select favourable times in which to press a button rather than mentally influence the computer in some way, but during the task my attention was entirely focused on obtaining positive feedback (as described below). I was not concerned about how that goal might be achieved.

Computer Program

When the program started, the PRNG was seeded with the current value of the system clock (with 60Hz resolution). The user entered the number of sequential analysis decision trials to be conducted that session, and upon pressing the return key, the trial number, the word 'aim', and the word 'HIGH' or 'LOW' was displayed (generated randomly, using *drand48* PRNG), as shown in Figure 2. The subject then pressed a button with the goal of producing a sequential analysis decision '1' or '0' so as to match the 'High' or 'Low' target, respectively.

Figure 2

Example of feedback used in the experiment. The percentages show the updated cumulative hit rate (chance expectation = 50%) when a hit occurs. At trial 14, there were 8 hits; at trial 16 there were 9 hits.

Trial 14: aim LOW ...
 Trial 14: aim LOW ...
 Trial 14: aim LOW ...
 Trial 14: aim LOW ... alright! [57%]
 Trial 15: aim HIGH ...
 Trial 15: aim HIGH ... -
 Trial 16: aim HIGH ...
 Trial 16: aim HIGH ... great! [56%]
 Trial 17: aim HIGH ...

When the button was pressed, the PRNG was re-seeded with the current value of the system clock, and pseudo-random bits were continuously generated⁴. As each successive bit was applied to the sequential analysis procedure, 3 possible, mutually exclusive decisions were being evaluated: that the sequence belonged to a '1' distribution, that the sequence belonged to a '0' distribution, or that no clear decision could be made (a pass). Because successive bits were automatically generated with a single button press, from the subject's per-

⁴The system clock on most UNIX systems is fairly fast, but on any multi-process system it is difficult to guarantee the computer's response time. For example, it is possible to press a button twice very quickly and for the values returned from the system clock to be identical. This can occur when the CPU is switching between the experimental program and several other processes. Because the CPU can only run one program at a time, but can continuously monitor primitive events like key-presses, when the CPU switches back to run the experiment, it first looks in an 'event buffer' to see if any key-presses had occurred while it was otherwise occupied. If it finds, say, that 2 button presses occurred, it will process those 2 events as fast as it can, which is much faster than the 60Hz system clock. The result will be 2 trials with identical results. To prevent this from happening, a software routine checked the current seed against the previous seed, and if they matched, the current seed was incremented by one. This ensured that consecutive button presses always resulted in different pseudorandom binary sequences.

spective each button press resulted in an immediate decision.

If a '1' or '0' decision resulted, and it matched the assigned target aim, an encouraging word appeared (see Figure 2), the terminal beeped, the cumulative hit rate for the session was displayed, a new target aim word was generated and displayed, and the next trial began. If the decision did not match the target, a '-' character indicated a miss.

If 'no decision' resulted, an ellipsis (...) appeared, indicating that the trial would be 'passed' because the sequence was insufficiently deviant for sequential analysis to make a firm decision. This provided a kind of built-in 'pass-this-trial' mechanism. The trial number remained unchanged, allowing the user to try the same trial again, with the same target. If the next button press also resulted in 'no decision', the same procedure was followed, giving the user another chance at this trial. This continued until a '1' or '0' decision was made.

In 'causal' terms, to be successful at this task one would have to (a) press the button at a time that would (b) seed the PRNG with a value that would (c) produce a binary sequence that would (d) produce a sequential analysis decision of '1' or '0' that would (e) match a target generated prior to the start of the current trial. Studies conducted by Braud and Shafer (1987), Lowry (1981), Radin and Bosworth (1985), Radin and May (1987), and others, indicate that statistically successful results can be observed in such experiments under well-controlled conditions.

However, the sampling procedure described here, and that used by Puthoff, May, and Thomson (1986), has an advantage over those used in prior experiments. By discarding 'pass' trials (i.e., those with hit rates close to $p=.5$), the procedure separates out deviant binary sequences from chance binary sequences. By accumulating only deviant binary sequences, the overall hit rate should be higher than if chance sequences were also included⁵. Likewise,

⁵This can be likened to separating the wheat from the chaff: the wheat are trials 'influenced' by psi, the chaff are trials that give chance results.

presuming that ψ can be used to select out desired deviant sequences, then the overall effect size and degree of statistical significance should also be higher.

With each button press, 5 data items were recorded and stored on the computer's hard disk: the final decision ('1', '0', or, for 'no decision', '-1'), the target ('0' or '1'), the number of '1' bits generated by the PRNG, the total number of bits used in the sequential analysis process, and the PRNG seed number used on that trial.

Effect Sizes

The primary measure of interest in this experiment was effect size (e). Four e 's were calculated, as follows. (a) A 'raw bits e ' was determined as $e_{rb} = Z/\sqrt{N}$, where N was the total number of bits generated by the PRNG in the experiment, including those trials in which the sequential analysis decision was 'no decision'. Z was determined in the usual way, based on the number of hits, where a hit occurred when a PRNG-generated bit matched the target bit in a given trial. (b) A 'filtered raw bit' effect size e_{frb} was determined in a similar way, except only using bits from sequential analysis trials that lead to a '1' or '0' decision. (c) A 'sequential analysis' effect size e_{ss} was based on the number of matches between sequential analysis decisions and targets, out of the pre-defined 5,000 trials in the experiment. (d) A 'weighted sequential analysis' effect size, e_{wss} , was calculated, also based on 5,000 trials, but using 'weighted' hits. Hits were defined as above; those trials in which the sequential analysis decision matched the target. The concept of 'weight', however, requires more explanation.

A Weighting Scheme

In sequential analysis, the average number of bits required to make a positive (i.e., '0' or '1') decision depends on the values chosen for α , β , p_0 and p_1 (Wald, 1973). With given values for these parameters, the probabilities associated with the number of samples required to make a positive decision declines approximately exponentially.

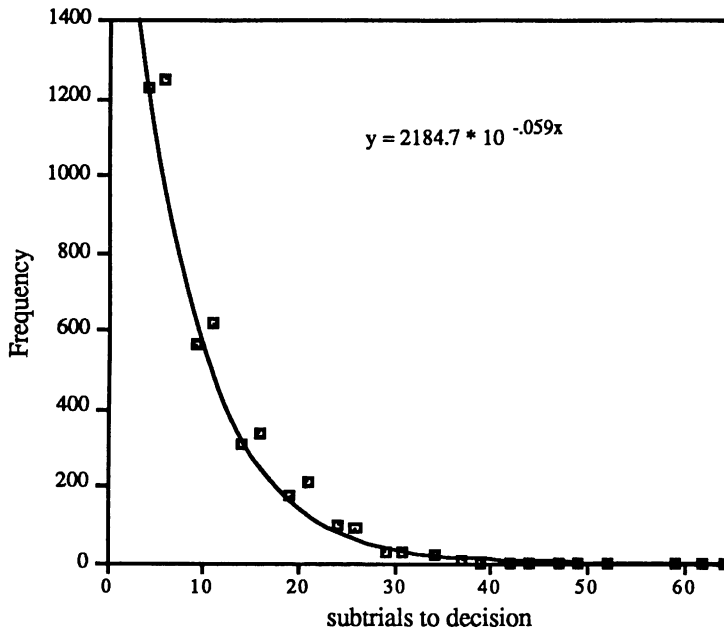
For example, with the parameters used in the present experiments ($p_0=.5$, $p_1=.7$, $\alpha=\beta=.25^6$), the minimum sequence length that can result in a positive decision is 4: the maximum sequence length is theoretically infinite, but, practically speaking, the upper limit is around 60 samples.

Figure 3 shows the results of a Monte Carlo study in which the number of binary samples-to-decision are plotted against frequency. The simulation consisted of 100 runs of 1,000 sequential analysis decisions each. These Monte Carlo runs were performed using the same software as in the experiment, except that instead of requiring individual button presses to reseed the PRNG, the PRNG was automatically used to generate a new seed on each successive trial, and there were no screen displays. In this way, 100,000 sequential analysis trials were generated without human intervention⁷. The exponential curve shown in Figure 3 is an excellent fit to the data, accounting for more than 98% of the variance.

⁶ As noted in Appendix B, the first 500 sequential sampling trials of the first block of 1,000 trials used $\alpha=\beta=.3$, to match the values used by Puthoff, May, & Thomson (1986). After trial 500, α and β were changed to .25 to increase slightly the number of 'I don't know' or 'pass' decisions.

⁷ This Monte Carlo study discarded trials resulting in a 'pass' decision (i.e., 'not 1' and 'not 0' decisions), resulting in approximately 50,000 '1' and '0' decisions.

Figure 3
Exponential curve fit to Monte Carlo Results



Now, given Puthoff, May, and Thomson's (1986) intriguing observation that hit rates increased as bits-to-decisions increased, I decided that if this observation were both genuine and generalizable, it would be desirable to weight sequential analysis results according to the number of bits used to make a decision: more bits, more weight. For my weighting factor, I took the inverse of the Monte-Carlo-determined exponential curve, that is, weight (x) = $1/(10^{-0.059x})$, where x = number of bits to make a decision. A weighted hit on a given trial is simply equal to the value of the weight, and thus the overall weighted hit rate is the sum of weights on those trials in which the decision matched the target (i.e., a hit) divided by the sum of weights on all trials.

What this technique does, in essence, is to create a moment arm about which to swing the hit rate. This acts as a kind of statistical amplifier that depends heavily on decisions made using many bits, and little on decisions made using fewer bits.

Another way of thinking about these weights is as confidence ratings on the individual decisions made by the sequential analysis process.

The mean and standard deviation of the resulting weighted hit rate was determined by Monte Carlo simulation of 100 runs of 1,000 sequential analysis decisions each, using the weighting scheme described above. The expected mean is still, of course, 50%, but the standard deviation is expected to be substantially larger than

$\sqrt{pq/N} = \sqrt{1/4000} = 1.58\%$, primarily because the amplification process is so sensitive to samples out on the 'tail' of the distribution shown in Figure 3. Results of 100,000 Monte Carlo trials⁸ showed the weighted mean = 49.458%, and standard deviation = 14.072%. This mean is slightly and non-significantly less than expected, but the standard deviation is about nine times larger than a non-weighted hit rate

⁸ This Monte Carlo study also discarded trials resulting in a 'pass' decision.

Table 1

Combined results of the experiment. There were a total of 10,202 button presses.

Condition	Hits	Samples	Z score ⁹	Hit(%)	Effect Size
Raw Bits	73,211	145,901	1.3640	50.2	0.004
FilteredRaw Bits	24,986	49,402	2.5645	50.6	0.012
SequentialAnalysis	2,540	5,000	1.1314	50.8	0.016
Weighted Seq. Sampling ¹⁰	3,330	5,000	2.6261	66.6	0.037
Sequential Analysis					
Aim Low	1,289	2,494	1.6820	51.7	0.034
Aim High	1,251	2,506	-0.0799	49.9	-0.002
Weighted Sequential Analysis					
Aim Low	1,473	2,492	1.4236	59.1	0.029
Aim High	1,774	2,506	3.2669	70.8	0.065

standard deviation. This large increase in variance is worth it (statistically speaking) only if the amplification process substantially improves the overall hit rate.

⁹The Z scores in Table 1 are calculated using $\sigma = \sqrt{Npq}$. For the sequential sampling and weighted sequential sampling conditions, this is a valid assumption. However, as shown later in Table 2, the actual variances for raw bits and filtered raw bits are somewhat greater than

$\sigma = \sqrt{Npq}$. This is because for raw bits the sequence lengths are not fixed length, and for filtered raw bits the trials by definition consisted of deviant (non-chance) binary sequences. Thus, the Z scores for raw and filtered bits are somewhat inflated. The hit rates are valid for all conditions.

¹⁰For simplicity in comparing results, in this table the N for weighted sequential samples is normalized to the total number of sequential sampling trials, N=5,000. The actual value for N is somewhat larger because it is the sum of inverse exponentials (i.e. total

score = $\sum_{i=1}^{5000} (1/10^{-.059x} i)$, where x_i is the number of bits to decision in trial i, and total hits equals the same summation, but only for those trials in which the sequential sampling decision matched the target.) The actual weighted values are shown in Appendix B.

Hypotheses

Three hypotheses were examined:

Hypothesis 1 was that evidence of psi would be found in the scores for raw bits, filtered raw bits, sequential analysis trials, and weighted sequential analysis trials in the overall experiment of 5,000 sequential analysis trials. Of course, these scores are not independent of each other.

Hypothesis 2 was that the slope of a weighted linear regression between the number of bits-to-decision (on the abscissa) and the hit rate (on the ordinate) would be positive, where the weight was the square root of the number of samples at each point along the abscissa (i.e., the denominator of the fraction used to determine the hit rate). This examination was performed to see whether Puthoff, May, and Thomson's (1986) observation about a positive slope could be replicated¹¹.

¹¹ I should point out that Puthoff, May, and Thomson (1986) used an unweighted linear regression on their data. This gave an inflated estimate of the slope because the number of samples (and therefore the variance) of points along the abscissa is not uniform, but declines exponentially.

Hypothesis 3 was that effect sizes would increase as greater degrees of statistical averaging were applied. In particular, if *Hypothesis 1* showed evidence of ψ , we would expect to find that effect sizes for raw bits (e_{rb}), filtered raw bits (e_{frb}), sequential analysis trials (e_{ss}), and weighted sequential analysis trials (e_{wss}) would increase in that order, i.e., $e_{rb} < e_{frb} < e_{ss} < e_{wss}$.

Results

As seen in Table 1, evidence for ψ was observed in 2 of the 4 conditions: filtered raw bits and weighted sequential analysis. Thus *Hypothesis 1* was partially confirmed. Note that $e_{frb} < e_{ss}$ but was statistically more significant because the sample size was larger. Results for each block of 1,000 sequential analysis trials are listed in Appendix B.

The weighted linear regression between

hit rate and bits-to-decision resulted in the relationship, $\text{hit rate} = 48.902 + 0.192 (\text{bits})$, and the test for a zero slope resulted in $t(28)=2.085$, $p=.037$ (2-tailed). Thus, *Hypothesis 2* is confirmed. Figure 4 shows this regression graphically.

Figure 5 graphically shows the results for effect size. As predicted by *Hypothesis 3*, effect sizes become progressively larger as we go from raw bits to weighted sequential samples.

Discussion

These results provide partial support for the 3 main hypotheses, and support the results of previous experiments employing statistical averaging procedures. This suggests that further development along these lines, including the use of weighted averaging techniques, may lead to improved methods of enhancing of the ψ 'signal to noise' ratio.

Figure 4

Weighted linear regression between hit rate and number of subtrials-to-decision

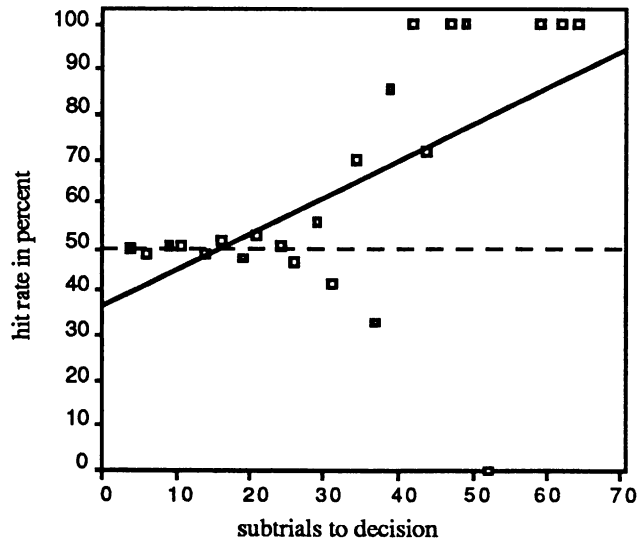
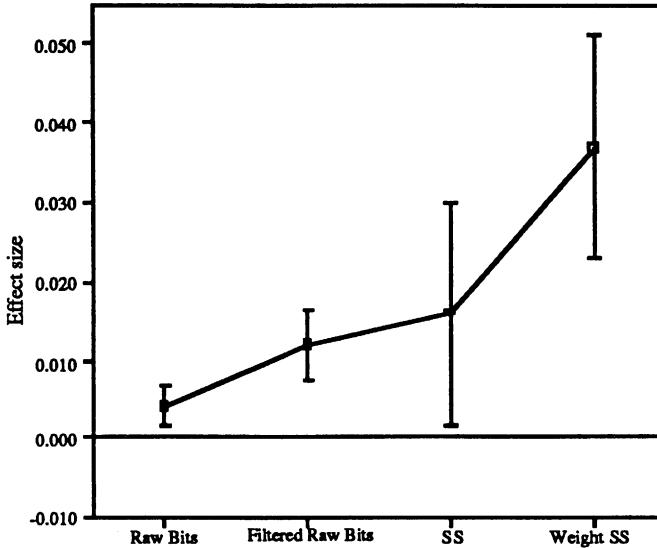


Figure 5

Effect size point estimates and standard error bars for raw bits, filtered raw bits, sequential analysis trials, and weighted sequential analysis trials



How do we know that the sequential analysis procedure operated properly in the present study, and that the PRNG was unbiased? Although the literature reports satisfactory randomness tests of *drand48*, there is always the possibility that the PRNG was used improperly in a specific application. To address this possibility, a Monte Carlo simulation of the experiment was conducted in which blocks of 1,000 sequential analysis trials were re-generated 10,000 times.

Given that each experiment produced about 25,000 bits to make 1,000 decisions¹², this means that the simulation was based upon approximately a quarter of a trillion pseudorandom bits. Means and standard deviations were calculated for raw bits, filtered raw bits, and sequential analysis hit rates (see Table 2). The theoretical standard deviations shown in Table 2 assume that N

$\approx 30,000$ for raw bits and $N \approx 10,000$ for filtered raw bits. However, because sequential analysis is not a fixed sequence-length procedure, the actual number of bits used to produce the 1,000 sequential sampling (SS) decisions varied from one decision to another. Thus, as seen in Table 2, the simulation standard deviations are larger than the (assumed fixed length) theoretical standard deviations.

Table 2

Monte Carlo simulation of a 1,000 trial sequential analysis experiment, with 10,000 repetitions

	Raw bits	Filtered raw bits	SS
mean hit rate	50.002	50.005	50.014
s.d.	0.294	0.821	1.589
σ_{theorv}	0.288	0.500	1.581

¹²The number of bits is different from one simulation run to the next because of the variable-length nature of sequential sampling.

A similar Monte-Carlo simulation was performed to examine the mean hit rates and standard deviations produced by the weighted hit scheme. The simulation consisted of 100 runs of 1,000 weighted sequential analysis (WSS) decisions. Table 3 shows the results, which were within chance expectation.

Table 3
Monte Carlo simulation of 1,000 trial weighted sequential analysis experiment, with 100 repetitions. Overall (WSS), and low and high aim results are shown. Note that the simulation randomly determined low and high aim decisions.

	WSS	Low aim	High aim
mean	49.458	48.926	50.360
s.d.	14.072	14.246	14.194
N	100	52	48

To check that the PRNG did not bias the bits-to-decision vs. hit rate relationship, I conducted yet another Monte Carlo simulation, consisting of 100 runs of 1,000 sequential analysis trials. At the end of each run of 1,000 sequential analysis trials, a weighted regression was performed to find the slope. Thus, 100 such slopes were examined to find the mean and standard deviation. The simulation resulted in $\mu = -0.021$, $\sigma = 0.336$, indicating that the PRNG and weighted sequential analysis process showed no inherent bias towards positive slopes.

Overall, this experiment indicated that after 5,000 sequential analysis decisions, the hit rate increased from a raw bit rate of 50.2%, to 50.6% after culling out the biased from non-biased sequences, to 50.8% after applying sequential analysis, and to 66.6% after applying a weighting scheme to the sequential analysis trials.

More importantly, the 5,000 trials required a total of 10,202 button presses, thus the required effort-to-result ratio was

only about 2:1 to amplify a 50.2% hit rate to 66.6%¹³. Contrast this to the 10:1 or 15:1 effort-to-result ratios reported in other studies using statistical averaging methods. Of course, the reason for the 2:1 ratio is that most of the work (i.e., generating the raw bits) is performed automatically by the PRNG. Given that it seems doubtful that psi 'works' on an individual bit level, or that bit-by-bit efforts necessarily increase overall psi efficiency or final hit rates (see Vassy, 1986, 1990), variations on the present method may eventually produce hit rates adaptable to practical psi tasks.

In closing, I should point out that since I was the subject in this experiment, and the results of the experiment conformed to my expectations (as formalised by the hypotheses), it remains to be seen if the present technique generalises to other subjects.

Conclusion

Several psi experiments have demonstrated that small hit rates can be enhanced with statistical averaging procedures. Puthoff, May, and Thomson (1986) conducted such an experiment using sequential analysis techniques. They also discovered an interesting relationship between the hit rate and the number of bits used to make a sequential analysis decision. The present study attempted to replicate both the sequential analysis method and the Puthoff et al. finding, and a bit-level 'weighting scheme' was also explored. Results of the experiment, consisting of 5 blocks of 1,000 sequential analysis trials each, replicated the finding that when psi is present, sequential analysis can enhance the hit rate. Results also suggested that the use of a bit weighting scheme may be a promising area to explore in further amplifying psi hit rates.

¹³It should be noted that the 10,202 button presses actually generated 145,901 bits, as shown in Table 1.

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Appendix A: Sequential Analysis Equations

The equations for the upper and lower lines shown in Figure 1 are as follows (see Wald, 1973):

$$y_1 = d_1 + SN$$

$$y_0 = -d_0 + SN$$

where d_1 and d_0 are the y -intercepts, S is the slope, and N is the number of subtrials. In terms of p_0 , p_1 , α , and β :

$$d_1 = \log[(1-\beta)/\alpha] / \log[p_1(1-p_0)/p_0(1-p_1)]$$

$$d_0 = \log[(1-\alpha)/\beta] / \log[p_1(1-p_0)/p_0(1-p_1)]$$

$$S = \log[(1-p_0)/(1-p_1)] / \log[p_1(1-p_0)/p_0(1-p_1)]$$

ENHANCING PSI EFFECTS WITH SEQUENTIAL ANALYSIS

Appendix B: Experimental results in 1000-trial blocks

Note: Five hundred trials of Block 1 were performed with $\alpha=\beta=.3$ to match the values used by Puthoff, May, and Thomson (1986). All subsequent trials in Block 1, as well as all trials in the 4 other blocks, used $\alpha=\beta=.25$. The change in α and β accounts for the lower numbers of raw bits and filtered raw bits seen in Block 1. 'BP' refers to the number of button-presses in the block.

BLOCK 1		Condition	Hits	Samples	Z	% hit rate	effect size
1989 BP		Raw Bits	13,023	25,580	2.9136	50.9	0.018
		Filtered Raw Bits	4,651	8,828	5.0448	52.7	0.054
		SS	546	1,000	2.9093	54.6	0.092
		Low SS	292	525	2.5750	55.6	0.112
		High SS	254	475	1.5141	53.5	0.069
		Weight SS	9,600	16,031	0.7019	59.9	0.022
		Low SS	4,435	7,559	0.6082	58.7	0.027
		High SS	5,165	8,471	0.7722	61.0	0.035

WEIGHTED LINEAR REGRESSION (hit rate vs. bits-to-decision):

$$y = 51.703 + 0.328x, t(22)=1.686$$

BLOCK 2 2034 BP

	Raw Bits	15,038	29,778	1.7269	50.5	0.010
	Filtered Raw Bits	5,268	10,277	2.5549	51.3	0.025
	SS	522	1,000	1.3914	52.2	0.044
	Low SS	249	464	1.5784	53.7	0.073
	High SS	273	536	0.4319	50.9	0.019
	Weight SS	22,847	31,123	1.6628	73.4	0.053
	Low SS	8,388	11,768	1.4925	71.3	0.069
	High SS	14,460	19,354	1.7400	74.7	0.075

REGRESSION: $y = 50.384 + 0.177x, t(17)=0.963$

BLOCK 3 2053 BP

	Raw Bits	14,689	29,736	-2.0761	49.4	-0.012
	Filtered Raw Bits	4,795	9,924	-3.3528	48.3	-0.034
	SS	464	1,000	-2.2768	46.4	-0.072
	Low SS	224	495	-2.1125	45.3	-0.095
	High SS	240	505	-1.1125	47.5	-0.050
	Weight SS	7,807	19,436	-0.6982	40.2	-0.022
	Low SS	3,348	7,489	-0.3271	44.7	-0.017
	High SS	4,460	11,947	-0.8921	37.3	-0.040

REGRESSION: $y = 44.570 + 0.184x, t(16)=0.869$

RADIN

BLOCK 4
2019 BP

Raw Bits	14,899	29,724	0.4292	50.1	0.002
Filtered Raw Bits	5,219	10,347	0.8946	50.4	0.009
SS	506	1,000	0.3795	50.6	0.012
Low SS	251	482	0.9110	52.1	0.041
High SS	255	518	-0.3515	49.2	-0.015
Weight SS	26,050	34,829	1.7611	74.8	0.056
Low SS	3,947	8,455	-0.2327	46.7	-0.011
High SS	22,103	26,374	2.3806	83.8	0.105

REGRESSION: $y = 49.689 + 0.088x$, $t(18)=0.379$

BLOCK 5
2107 BP

Raw Bits	15,562	31,083	0.2326	50.1	0.001
Filtered Raw Bits	5,053	10,026	0.7990	50.4	0.008
SS	502	1,000	0.1265	50.2	0.004
Low SS	273	528	0.7833	51.7	0.034
High SS	229	472	-0.6444	48.5	-0.030
Weight SS	18,030	25,271	1.5162	71.3	0.048
Low SS	6,902	10,452	1.1252	66.0	0.049
High SS	11,128	14,819	1.7668	75.1	0.081

REGRESSION: $y = 47.120 + 0.307x$, $t(18)=1.633$

Dean I. Radin
Institute for Advanced Study at Waltham
253 Bishops Forest Drive
Waltham, MA 02154
USA

Augmenter les effets dans les expérimentations psi à l'aide de l'analyse séquentielle: Replication et prolongement

Résumé: Différentes études ont mis en évidence que les petits effets observés dans les expérimentations psi peuvent être augmentés en appliquant des procédures de moyennage statistique tel que le vote de majorité. Dans une de ces études, une technique appelée analyse séquentielle a été utilisée afin d'augmenter le niveau de réussite effectif dans une expérience avec générateur numérique pseudoaléatoire. L'analyse séquentielle est une alternative séduisante à la procédure plus répandue à nombre d'essais fixe, car elle fournit un plus grand pouvoir statistique avec moins d'essais. La présente étude est une réplication conceptuelle de l'utilisation de l'analyse séquentielle dans une expérimentation psi, avec l'addition d'un nouveau schéma de pondération-de-bit en vue d'augmenter encore plus le niveau de réussite obtenu. L'expérience, consistant en cinq blocs de 1,000 essais d'analyse séquentielle chacun, produits par un même sujet, a mis en évidence un effet psi, ce qui réplique le résultat que les techniques de moyennage statistiques peuvent être utilisées afin d'augmenter le niveau de réussite 'brut'. Les résultats suggèrent aussi que l'utilisation d'un schéma de pondération-de-bit conjointement avec l'analyse séquentielle pourrait constituer un domaine prometteur à explorer afin d'amplifier encore plus les niveaux de réussites dans les expérimentations similaires.

Some PK Attitudes as Determinants of PK Performance

Loftur Reimar Gissurarson
Beykihlid 13, 105 Reykjavik, Iceland

Abstract: This paper examines data from 96 subjects for a possible connection between scores on a psi-RNG computer test, and self-rated belief in, and reported experience of, PK and self-perceived success. Both the belief and success factors demonstrated nonsignificant correlations in different directions with psi score, depending on whether the score had been obtained under psychokinesis (PK) or precognition (PC) instructions. Frequency of reported prior experience of a PK event showed a slight, nonsignificant but positive correlation with psi score for both PK and PC groups. A meta-analysis carried out on this trend, combining the 5 studies reported by Gissurarson and Morris (1991a) with the present results, yielded a weighted z of 3.15 (with combined probabilities of $p=.0008$, 1-tailed). Significant correlations were found between the belief, success and experience factors and 3 scales intended to measure self-reported determination, volitional intensity and engagement in volitionally-related activity.

Differences have been reported between the ESP performance of those who accept the possibility of ESP ('sheep'), who tend to score above chance, and those who comparatively reject the possibility of ESP ('goats'), who tend to score below chance (see Palmer, 1971; 1972). Although this so-called sheep-goat effect has received a lot of experimental attention for ESP, there has been much less focus on comparable effects for PK.

In surveying the literature, Gissurarson and Morris (1991a) found 7 studies that tested the relationship between belief in PK and performance on some PK test (Dale, 1946; Mischo & Weis, 1973; Nash, 1946; Van de Castle, 1958; Weiner, 1979, 1982a, 1982b). Only Weiner (1982a) demonstrated a significant, positive PK effect related to belief. In their own work, Gissurarson and Morris found a positive relationship between PK scoring and belief in PK, as measured by a subset of questions (revealed as a factor, by factor analysis) on a Psychokinetic Attitude and Perceived Experience Questionnaire (PAPEQ), in 3 of 5 studies. The correlation between PK score and the sheep-goat factor was, however, only significant in one of the 3 studies, ($z=3.58$, $p<.001$, 2-tailed), and in 2 of the 5 studies the relationship was virtually nil.

Furthermore, they found that the more subjects reported having 'had a psychokinetic experience' on PAPEQ, the higher their PK scores tended to be, in all 5 studies (meta-analysis combining individual z scores yielded $z=3.03$, $p=.001$, 1-tailed). Additionally, the more subjects rated themselves on PAPEQ as successful in life and able to influence the PK test, the higher the PK scoring they tended to get in 3 of the 5 studies. This relationship was, however, nonsignificant for all 3, and disappeared entirely in 2 of the 5 studies.

Studies looking for and obtaining correlations between psi scores and scores on other tests are important for the following reasons. First, the correlations can be considered as evidence for PK. A consistent relationship between scores on a psi test and scores on some other test may even yield more convincing evidence for psi than a simple deviation from mean chance expectation (MCE), in that it may be more difficult to dismiss such a correlation as fraud by the subjects, or as equipment faults. Second, if a systematic correlation between psi scoring and some measurement of individual differences, such as that provided by a psychological scale, was found, then the scale could easily be applied to predict performance on psi tests

and to select subjects who would be potential high performers. Third, the specific findings would be important for theory construction regarding the possible processes involved in intentional PK.

The present study attempted to follow up and explore further the possible connection between PK, belief in and reported experience of PK, and self-rated success. The data reported here were collected at the University of Edinburgh during an experiment that was conducted, in 3 phases from June to September 1991, in order to develop a means of measuring some components of volitional behavior. A manipulation of precognition and psychokinesis instructions and high- and low-aim conditions was attempted in the study for exploratory purposes. Although the psi score results and some of the volitional questionnaire results are summarized in this paper, they are presented in more detail elsewhere (Gissurarson & Morris, in preparation). The main focus here is on examining the replicability of the PAPEQ results.

Method

Subjects

Overall, 119 subjects participated in the volition experiment. Ninety-six of them (39 males and 57 females, aged between 15 and 69 years) completed an abbreviated form of PAPEQ (see Appendix) in addition to the tests aimed at measuring some aspects of volition; 23 subjects took part in the first of the 3 phases of the volition experiment, but did not fill in the PAPEQ). The 96 subjects were mainly recruited through advertisements placed around the university campus and in a Scottish newspaper, but some were people who had taken part in the author's earlier experiments.

Apparatus

The abbreviated form of PAPEQ included 6 fixed alternative questions, 3 aimed at measuring belief in PK (items 1-3), 2 aimed at measuring self-perceived

success in life in general and on the PK task ahead (items 4-5), and one about prior experience of a PK event (item 6). Question 6 was followed by an open-ended question (item 7) for those who reported having had a PK experience, asking about the nature of this event.

Other questionnaires employed were the Volitional Styles Inventory (VSI), the Volitional History Questionnaire (VHQ) and the Wishing Intensity Questionnaire (WIQ) (details supplied by Gissurarson & Morris, in preparation). The VSI is a 26-item scale measuring self-reported tendency to behave with self-determination. The following question is an example of the VSI items: 'When you have made up your mind to do something, have you intended to stick to your choice even if it meant that you would lose close friends?' The VHQ is a 34-item scale measuring involvement in volitionally related activity. The VHQ items are divided into 2 parts, A and B; the first 11 items are specific to experiences when growing up, and the latter 13 are specific to experiences in the present, respectively. Examples of the questions on the VHQ are, 'Did you enjoy challenging others (when growing up)?' and 'Do you have many long-term goals (in present)?'. The WIQ is a 10-item scale measuring self-rated volitional intensity. Each item consists of a wish and is rated twice: first, for the confidence that the subject can make the particular wish come true, and second, for the intensity of the subject's concentration while willing the wish to come true. An example of an item from the WIQ is: 'Make a wish in your mind to get rid of an annoying behavior you feel you have. Now, in your mind "will" it to change.'

The psi task was a computer test called 'Synthia', run on an IBM XT machine in a sound-attenuated room in the Psychology Department. 'Synthia' has been described in detail elsewhere (Gissurarson, 1989; Gissurarson & Morris, 1990; 1991b), and works as follows. For each trial, 4 windows appear on the screen. A random number generator (RNG) selects one of the windows as a target at the start of every 10-trial block of a 40-trial run. The RNG cannot

select the same target window consecutively, but otherwise it is left to chance which window becomes the target; an arrow appears on the screen beneath the designated window to show that it is the target.

The task consisted of attempting to get the computer to select the designated target window each time the subject pressed the space-bar, thereby initiating the RNG once more. Each trial was tallied as a 'hit' or a 'miss', depending on whether or not the number selected matched the assigned target number. The present version of the computer program includes only a true RNG (for details see *User's Guide Random Bit Generator RBG 04CA-S*, 1988), based on an analog noise generator that produces wide band noise (reversed biased PN-junction noise, or recombination noise, often called 'Zener' noise).

Two types of randomization tests were run in the absence of subjects, before, during and after the study. Firstly, these included tests of the RNG for large series of numbers, run in series of 500,000 each time and using the same algorithm ($p=1/4$) as the Synthia program. The numbers generated by the RNG were thus divided into 4 groups, with the expected frequency for each group being 125,000 per run. A total of 7,500,000 trials were done, the overall difference between the 4 groups being nonsignificant; $\chi^2(3)=3.06$ ($p=.39$). Secondly, the study was simulated by a computer program, in which, for each trial, the RNG was accessed and a random number produced which was then compared to a target number (also generated by the RNG for every 10-trial block of a 40-trial run). Overall 51 studies were run; 3 were significant at the $p=.05$ level, 2-tailed, 2 above MCE and one below, which is about what one might expect by chance. The number '51' was not set in advance and the cut-off point at which to stop running depended on variables such as the time available.

There were 2 levels of feedback available. In the high-aim mode, when subjects would be asked to produce hits, trial-by-trial feedback was provided by a beep sound immediately a hit was made, with

the screen lighting up into a blue star with blue rays radiating from behind. In the low-aim mode, when subjects would be asked to avoid hits, trial-by-trial feedback was provided by a 'misfire' sound immediately a hit was made, and the screen lit up into a green star with green rays radiating from behind.

Procedure

The subjects each completed all the questionnaires (the WIQ, VHQ and PAPEQ at home at their leisure, and the VSI as the first thing in the experimental session) before being given a demonstration of both the high- and the low-aim modes of the computer test. Alone in the sound-attenuated room, the subject then completed 2 40-trial runs on the test, with a break between runs. A flip of a coin decided whether the first subject who attended a session received precognition (PC) instructions to guess or predict the occurrence of the target number throughout the session, or psychokinesis (PK) instructions to will the target number to occur^{1,2}. The second subject to attend got the other instructions, and so on, continuing to alternate for the rest of the subjects. Another flip of a coin decided whether the first subject did the first computer test run with the high-aim or with the low-aim mode (and the second run with the other mode). The second subject to attend had the same order of high- and low-aim modes as the first subject. The third and fourth subjects had the modes in the reverse order to that of the first 2 subjects, and so on in alternation for the rest of the subjects.

If instructions to subjects matter and there are 2 different mechanisms behind PC

¹ Because of the similarity in manifestations of the alleged phenomena of precognition and psychokinesis on a micro level, and the fact that discussions of one issue often bring up the subject of the other, it seemed appropriate to abbreviate the 2 concepts in a similar fashion.

² The PC condition was not intended to examine the ESP sheep-goat effect. Although the PC instructions emphasized the operation of ESP, all PAPEQ items were concerned with PK attitudes.

and PK, instructions to use PC might lead to the operation of PC and result in a 'PC score', whereas instructions to use PK might lead to the operation of PK and result in a 'PK score'. For the sake of argument, if PK performance is affected by belief in the phenomenon, we might expect a positive relationship between ratings on a PK sheep-goat scale and scores on a PK test (obtained via the instructed use of PK). There is no empirical reason to believe that ratings on a PK sheep-goat scale should be related (positively or negatively) to scores on a PC test (obtained via the instructed use of PC), and, to the best of the author's knowledge, no prior research has been carried out on this. However, since we can use both PK and PC to achieve things or avoid things, get a certain random number (high-aim condition) or avoid getting a certain random number (low-aim condition), a possible relationship between a PK sheep-goat scale and psi score should show the same direction for both conditions if the relationship was real rather than spurious. In other words, we should observe a positive relationship between belief in PK and the absolute degree of deviation from chance (i.e., irrespective of whether the score was obtained for high- or low-aim conditions).

The instructions, read out loud in the sound-attenuated room for PC subjects after the demonstration runs, before they started the high-aim mode, involved suggestions that they should, for each trial, use their insight and decision-making ability to choose the right time to press the space-bar to get the blue star to appear. The instructions read for PC subjects before the low-aim mode involved suggestions that they should, for each trial, use their insight and decision making ability to guess the right time to press the space-bar to keep the green star from occurring. The instructions read out loud before PK subjects started the high-aim mode involved suggestions that they should, for each trial, use their 'will-power' to make the blue star appear when they pressed down the space bar. The instructions read for PK subjects before the low-aim mode involved suggestions that

they should for each trial use their will-power to keep the green star from occurring when they pressed down the space-bar.

Results

Overall, the 96 subjects obtained 22 hits below chance ($z=-.58$), that is, in the opposite direction to that which they had been instructed to aim for; +11 hits in the high-aim condition ($z=+.41$), and -33 hits, that is 33 hits above MCE in the low-aim condition ($z=-1.23$). In the PK condition, 4 hits below chance ($z=-.15$) were obtained; +2 in the high-aim condition ($z=+.11$), and -6 in the low-aim condition ($z=-.32$). In the PC condition, 18 hits below chance were obtained ($z=-.67$); +9 in the high-aim condition ($z=+.47$), and -27 for the low-aim condition ($z=-1.42$). Differences between conditions were nonsignificant.

Psi score did not correlate significantly with any of the PAPEQ factors for any of the conditions, according to the Spearman rank order correlation coefficient (r_s) (see Table 1)³. Interestingly, however, both the sheep-goat factor and the success factors tended to correlate in different directions with psi score, depending on whether the score had been obtained under PK or PC instructions. For the PK group, the sheep-goat factor and psi score (combined high- and low-aim conditions) demonstrated a nonsignificant r_s of +.18, and the success factor and psi score a nonsignificant r_s of +.17. For the PC group, the sheep-goat factor and psi score (combined high- and low-aim conditions) showed a nonsignificant r_s of -.13, and the success factor and psi score a nonsignificant r_s of -.17. Question 6 of PAPEQ, regarding prior experience of a PK event, correlated positively for both PK and PC groups, $r_s = +.09$ and $r_s = +.14$, respectively. Only the overall PAPEQ score came close to correlating significantly with psi score for the PK group, where $r_s = .29$ ($p = .06$, 2-tailed). For

³ Three PK subjects did not complete PAPEQ, resulting in $N=45$ for the PK group, and $N=48$ for the PC group.

Table 1

Spearman rank order correlation coefficients between PAPEQ factors and psi score, for psychokinesis (PK) and precognition (PC) instruction groups

	High-aim		Low-aim		Combined	
	Hits	p^e	Hits	p	Hits	p
PK group:						
Belief ^a	+0.10	0.51	+0.24	0.11	+0.18	0.24
Success ^b	+0.10	0.52	+0.14	0.36	+0.17	0.26
Experience ^c	+0.05	0.73	+0.06	0.67	+0.09	0.57
PAPEQ total ^d	+0.23	0.13	+0.18	0.25	+0.29	0.06
PC Group:						
Belief	-0.10	0.51	-0.17	0.26	-0.13	0.37
Success	-0.12	0.43	-0.23	0.12	-0.17	0.25
Experience	+0.03	0.83	+0.13	0.39	+0.14	0.36
PAPEQ total	-0.08	0.58	-0.16	0.28	-0.11	0.47

^a 'Belief' refers to the sheep-goat factor on PAPEQ (combined score for items 1-3)

^b 'Success' refers to the success factor on PAPEQ (combined score for items 4-5)

^c 'Experience' refers to the question about prior PK experience on PAPEQ (item 6)

^d PAPEQ total refers to combined score for items 1-6 on PAPEQ

^e All p -values are reported as 2-tailed.

comparison, the total PAPEQ score demonstrated a nonsignificant, negative correlation with psi score for the PC groups, $r_s = -.11$.

Interestingly, all volition scales yielded positive relationships with the PAPEQ factors, although these correlations did not always reach significance (see Table 2). The belief factor correlated significantly with the WIQ, $r_s = .20$, a relationship mainly due to Part B of the WIQ (intensity of concentration on wishes), which demonstrated a significant correlation of .23 with the belief factor. The prior-PK-experience question only correlated significantly with Part A of the VHQ (volitionally-related experiences when growing up). As one might have expected of all the PAPEQ factors, the success factor showed the biggest relationships with the volition scales, and correlated significantly with all of them. Of the 2 success items, the one relating to success in life (item 5) demonstrated bigger correlations,

and with the VSI, WIQ and VHQ, the correlations were $r_s = .38$ ($p < .001$), $r_s = .30$ ($p < .003$), and $r_s = .41$ ($p < .001$), respectively.

The relationship between reported PK experience and psi score was positive for both PK and PC instruction groups, but very weak. Overall, 47 subjects (about 50%) reported 'never' having experienced a PK event; 24 (about 25%) reported having 'very rarely' encountered such an event; 14 (about 15%) reported having 'rarely' encountered one; 7 (about 7%) reported having 'occasionally' had such an experience; and 1 (about 1%) reported having had it 'often'. In short, about 50% of the sample reported having experienced a PK event of some sort. Of those, for whatever reasons, only about half (24 subjects) decided to describe their experience in response to item 7 on PAPEQ. The reported experiences can be divided into the following categories (some subjects reported more than one experience).

Table 2

Spearman rank order correlation coefficients between the PAPEQ factors and the Volitional Styles Inventory (VSI), Wishing Intensity Questionnaire (WIQ), and Volitional History Questionnaire (VHQ)

	VSI		WIQ		WIQ-A ^a		WIQ-B ^b		VHQ		VHQ-A ^c		VHQ-B ^d	
	<i>r_s</i>	<i>p</i> ^e	<i>r_s</i>	<i>p</i>	<i>r_s</i>	<i>p</i>	<i>r_s</i>	<i>p</i>	<i>r_s</i>	<i>p</i>	<i>r_s</i>	<i>p</i>	<i>r_s</i>	<i>p</i>
Belief	.18	.06	.20	.03	.11	.17	.23	.02	.11	.17	.08	.23	.09	.21
Success	.34	.001	.35	.001	.35	.001	.29	.004	.43	<.001	.39	<.001	.39	<.001
Experience	.10	.19	.13	.13	.11	.17	.14	.10	.17	.07	.19	.04	.06	.31
PAPEQ total	.27	.009	.26	.011	.18	.054	.26	.009	.30	.005	.28	.004	.23	.03

^a Part A of WIQ: rating for confidence that the subject could make the particular wish come true

^b Part B of WIQ: rating for the intensity of the subject's concentration when willing the wish to come true

^c Part A of VHQ: items specific to experiences when growing up

^d Part B of VHQ: items specific to experiences in the present

^e All *p*-values are reported as 2-tailed

ESP events: There were 8 instances that can be placed under the heading of ESP: alleged knowledge of something that the subjects should not have known about, and purported experiences of telepathy. Four of these involved a crisis of some sort; 3 were premonitions of deaths (2 in dreams), and one allegedly involved knowledge of a serious illness in somebody close to the subject (who was in a different country when she had this experience). The average hit deviation from MCE of this group was 1.43 ($z=.97$).

Influencing people/games/nature: There were 5 instances where subjects believed they had possibly and intentionally exerted PK influence. Two subjects reported having been able to influence games. One subject reported being successful at staring at the back of people in front of him and 'willing' them to turn around. Another subject wrote that she had been able to make a man drive a tractor off the road by her will-power after he had killed a kitten of hers. Finally, one subject believed that he had on one occasion been able to bring about a 'gale-force' storm when standing on the top of a hill in Edinburgh. The average hit deviation from MCE of this group was -2.6 ($z=-1.50$).

Healing reports: Four reports involved 'healing' or changes in the subject

him/herself. For instance, one subject reported curing a chest illness by mountaineering, after being told by a doctor that it was medically impossible for him to have a full recovery. Another reported healing her right hand, after writing too much, by imagining white light around it, and one nurse reported being able to increase or decrease her pulse rate as displayed on a pulse meter. The average hit deviation from MCE of this group was 1.25 ($z=.65$).

Power of positive thinking: There were 4 reports where subjects believed that positive or optimistic thinking had changed things or events for the better. For instance, one subject reported being able to obtain things by intense prayer, and another reported evoking a positive attitude and visualizing a parking space for his car before setting out on errands, and upon arriving at the destination he would frequently find only one parking space available. The average hit deviation from MCE of this group was 0.5 ($z=.26$).

Malfunctioning equipment: Three reports involved anomalous, sudden and temporary malfunctioning of electrical equipment. One subject explained this in terms of heavy static that she was able to produce, another pointed out that this accompanied 'acute emotional distress' in her life.

The average hit deviation from MCE of this group was -1.0 ($z = -.44$).

Other reports: One subject reported having witnessed a PK event by seeing a friend switch on a broken gas ignition which ordinarily did not work; the friend claimed to have been able to put out candle flames by the power of thought only. Another subject reported that she believed that she had had headaches that could have been the result of having been 'bewitched' by others. One subject reported PK experiences involving objects being mysteriously displaced, despite her being certain of where they had been left. Another subject reported that 'following a flippant remark challenging "God" to prove His existence, myself and friends experienced an overpowering smell of perfume which became a sort of force-field, temporarily blocking entrance to another room in my house.'

Discussion

Attitudes Towards PK and Psi Performance.

The implications of the belief and success factors correlating positively with psi score for the PK group, and negatively with psi score for the PC group, remain unclear for the present, because these relationships turned out nonsignificant. For the sake of the argument, however, let us suppose they represent weak but real effects. Probably the most simple way to look at these differential effects would be to suggest that PC instructions lead to 'precognition scores' and PK instructions result in 'psychokinesis scores'. Some positive attitudes germane to PK would be positively related to PK performance, whereas the same attitudes towards PK might not necessarily be related to PC performance. Should future research yield significantly different relationships between questionnaire score and psi performance for PC and PK instruction subjects, one might argue that, at least in some experimental situations, we can and must distinguish between precognition and psychokinesis performance.

How do the relationships reported here add to the evidence so far reported in the literature that PK performance can be partially predicted if we know subjects' belief in PK, their possible prior experience of a PK event, and how successful they feel they are?

When assessing the size of the effect and the combined probabilities for all PK sheep-goat studies on record we get a non-significant weighted r of .10, with a corresponding z of 1.38, $p = .08$, 1-tailed (see Table 3)⁴. For comparison, unweighted $z = 2.40$ for the 14 studies, and on closer inspection of Table 3 we can see that both studies with N larger than 50 subjects demonstrated negative correlations, and no results were reported for the single study where $N = 50$. Dividing the studies between the US ($N = 202$) and Edinburgh ($N = 255$) shows that whereas the US studies are at chance, ($r = -.04$, $z = -.04$), the Edinburgh series manages to produce $r = .15$, $z = 1.75$ ($p = .04$, 1-tailed). All Edinburgh studies were conducted with one version or the other of the computer program 'Synthia'. What these results would seem to suggest is that the number of subjects is important for a study of the sheep-goat effect for PK. However, a conclusion to the effect that a study with N larger than 50 risks increasing the effect of the experimenter's fatigue on subjects' attitudes and/or performance cannot be justified, because some of the smaller studies also demonstrated negative results. Besides, the number of studies may be too small to base any firm conclusions

⁴ Combining Z scores and weighting them by N was done using the procedure described by Rosenthal (1984):

$$Z = (W_1 Z_1 + \dots + W_k Z_k) / \sqrt{(W_1^2 + \dots + W_k^2)}$$

where W = weight. Rosenthal also describes the procedure to obtain product moment correlation from z as follows: $r = Z / \sqrt{N}$. (See also Hunter & Schmidt, 1990, for a recent and more detailed description of meta-analytic techniques.) Combining correlations across studies was done by transforming each to its Fisher's z equivalent, and then the weighted z was obtained by the formula: $Z = (W_1 Z_1 + \dots + W_k Z_k) / (W_1 + \dots + W_k)$, where W = weight. The weighted Fisher's Z was then transformed to its corresponding r .

Table 3
Summary of PK sheep-goat studies and meta-analysis results

Investigator	N	z	Fisher z	r	p ^e
US Studies					
Nash 1946	9	.64 ^c	.21	.21	-
Dale 1946	54	-.50 ^c	-.07	-.07	-
Van de Castle 1958	22 ^b	.48 ^c	.10	.10	-
Mischo & Weis 1973	50	.00 ^d	.00	.00	-
Weiner 1979	26	.00 ^d	.00	.00	-
Weiner 1982a	13	2.60	.91	.72	.005
Weiner 1982b	28	-.95	-.18	-.18	.17
	202	-.04	-.04	-.04	-
Edinburgh Studies					
G & M 1991a	10	.46	.15	.15	-
G & M 1991a	40	2.90	.50	.46	.002
G & M 1991a	10	1.42	.49	.45	.08
G & M 1991a	20	-.18	-.04	-.04	-
G & M 1991a	90	-.32	-.03	-.03	-
MacLulich 1990 ^a	40	1.25	.20	.20	.11
Present study	45	1.19	.18	.18	.12
	255	1.75	.15	.15	.04
Combined Studies: Weighted by N	457	1.38	.10	.10	.08

^a Following the studies reported in Gissurarson and Morris (1991a), MacLulich (1990) conducted an additional study at the University of Edinburgh, which looked at the PK sheep-goat relationship employing the computer program Synthia to measure PK performance.

^b N in this study does not include subjects who did not state whether they believed in PK.

^c z_{diff} was calculated from the limited information available in these reports.

^d No significant relationships were reported in these studies, and no information was available.

^e Only $p < .25$ 1-tailed reported.

on. The difference between the US and Edinburgh studies remains unclear for the present.

A meta-analysis was carried out on the trend observed between PK performance and responses to the question about prior PK experience in PAPEQ. The 5 studies reported by Gissurarson and Morris (1991a) were combined with the present PK (N=45) and PC (N=48) group results. When weighting all the studies by the number of subjects participating, we get an overall z of 3.15 (with combined probabilities of $p = .0008$, 1-tailed). The combined weighted estimate of the size of the effect yielded an r

of .22, with a 95% confidence interval of $r = +/- .13$.

An overall analysis carried out on the success factor trend, combining results from the 5 earlier studies with the present PK group, yielded a nonsignificant weighted z of only 1.04 ($p = .15$, 1-tailed).

Some Volitional Determinants of PK Attitudes

It would seem that some attitudes towards PK may be related to self-reported volitional behavior. The higher the subjects scored on the volitional determination,

intensity and activity parameters, the higher they tended to rate themselves on the belief, success and experience subscales on PAPEQ. Of the 3 volition scales, the WIQ proved to be the best indicator of belief in the existence of PK, and the VHQ proved to be the best indicator of self-perceived success and reported prior experience of PK. In other words, it would seem that the more intense the subjects felt their volitions were, the more ready they were to accept the existence of PK, and the more the subjects reported engaging in volitionally-related activities, the more confident they were that they could influence the task ahead and the more they reported having experienced a PK event. It must be emphasized that although total PAPEQ scores were significantly related to the VSI, WIQ and VHQ, these positive relationships were to some extent due to high correlations between the 3 volitional scales and the self-perceived success in life item on PAPEQ, which, on its own, can hardly be considered an attitude towards PK.

Analysis of Reported PK Experiences

Interestingly, subjects reported various instances of possible PK experiences, many of which seem to involve cases of coincidence, and trivial attributions to the paranormal of some ordinary events which hardly seem to merit any paranormal explanation. The variety in responses suggests that subjects were not sure whether their experience could be labelled as a PK experience, or that they had understood the question to be inquiring about paranormal events in general, not necessarily related to PK. This could help explain why the psi score correlation with question 6 was positive for both PK and PC instruction groups, in that subjects were not only reporting their experience of PK events but of ESP instances as well. Although highest psi scoring was associated with reported ESP experiences, this observation, unclear as it is, must remain tentative for the present. The number of reported experiences was quite low within each category, and only about half of the subjects who reported

having encountered PK actually described their experience.

Summary

It would seem that the present findings merit further research, especially into the relationship between reported psi experience and actual psi performance, and into the PK sheep-goat effect. Further research would seem to be needed to explore the experimental design introduced in this paper as a possible means of differentiating between psychokinesis and precognition.

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Appendix

The abbreviated form of PAPEQ used in the present study.

- 1) Do you think that the existence of psychokinesis is:
- | | |
|-----------------|------------|
| (1) Impossible? | (Score: 0) |
| (2) Unlikely? | (" : 1) |
| (3) Uncertain? | (" : 2) |
| (4) Likely? | (" : 3) |
| (5) Definite? | (" : 4) |
- 2) Do you think that some people may be able to affect physical conditions (or move objects or influence other people) with their 'minds'?
- | | |
|---------------------|------------|
| (1) Definitely. | (Score: 4) |
| (2) Probably. | (" : 3) |
| (3) I don't know. | (" : 2) |
| (4) Probably not. | (" : 1) |
| (5) Definitely not. | (" : 0) |
- 3) Do you believe that you can demonstrate the psychokinesis effect (i.e. affect physical conditions or move objects or influence others with your 'mind')?
- | | |
|---------------------|------------|
| (1) Definitely not. | (Score: 0) |
| (2) Probably not. | (" : 1) |
| (3) I don't know. | (" : 2) |
| (4) Probably. | (" : 3) |
| (5) Definitely. | (" : 4) |
- 4) Which of the following statements best describes how you feel about the task that you are about to participate in?
- | | |
|--|------------|
| (1) I will definitely not be able to influence the test. | (Score: 0) |
| (2) I will probably not be able to influence the test. | (" : 1) |
| (3) I don't know. | (" : 2) |
| (4) I will probably be able to influence the test. | (" : 3) |
| (5) I will definitely be able to influence the test. | (" : 4) |

PK ATTITUDES AND PERFORMANCE

5) In general, how successful do you consider yourself to be?

- | | |
|--|------------|
| (1) I am definitely not a very successful person | (Score: 0) |
| (2) I am not as successful as others. | (" :1) |
| (3) I am average. | (" :2) |
| (4) I think I am a reasonably successful person. | (" :3) |
| (5) I am definitely a very successful person. | (" :4) |

6) Have you ever had an experience which you think may have been due to psychokinesis?⁵

- | | |
|---------------------------------|------------|
| (1) Never. | (Score: 0) |
| (2) Uncertain, but very rarely. | (" :1) |
| (3) Likely, but rarely. | (" :2) |
| (4) Occasionally. | (" :3) |
| (5) Often. | (" :4) |

7) If you reported having possibly had what you might consider a psychokinetic experience, and would not mind relating it, could you please write down a short summary of one of these experiences on the space below.

Beykhlid 13
105 Reykjavik
ICELAND

Rôle déterminant de certaines attitudes PK sur la performance PK

Résumé: Cet article examine les données de 96 sujets afin de trouver un lien possible entre leurs scores à un test psi informatisé avec générateur numérique aléatoire et l'évaluation de leur croyance à la PK, leur expérience de la PK et la perception de leur propre succès. Les facteurs de croyance et de succès n'ont montré aucune corrélation significative dans différentes directions avec le score psi, que le score ait été obtenu sous consigne de psychokinésie (PK) ou de précognition (PC). La fréquence d'expérience antérieure d'un événement vécu PK a montré une légère corrélation non-significative mais positive avec le score psi pour les deux groupes PK et PC. Une méta-analyse de cette tendance combinant les cinq études rapportées par Gissurason et Morris (1991a) et les présents résultats a produit une valeur z pondérée de 3.15 (probabilités combinées $p=.0008$, unilatéral). On a trouvé des corrélations significatives entre les facteurs de croyance, de succès et de vécu, et les trois échelles destinées à mesurer la détermination de soi, l'intensité volitive et l'engagement dans une activité liée à la volition.

⁵ Question 6 was intended to measure the frequency of spontaneous PK whilst allowing for those events that subjects were not absolutely sure of. The degree of certainty that an event was paranormal was associated with the rarity of the phenomenon. The more scarce an event is, the more likely we are to question its reality. In future, researchers may wish to distinguish between these 2 aspects with separate questions.

The Effect of Retro-Active Distance Healing on *Babesia Rodhani* (Rodent Malaria) in Rats

F.W.J.J. Snel & P.C. van der Sijde

Ned. Ver. v. Parapsychologie, Bilthoven, The Netherlands

Abstract: In this paper the results of the healing activities of a paranormal healer who worked, using photographs, from a distance on an experimental disease in rats (rodent malaria), are discussed. Significant differences in the absolute number of infected red blood cells between experimental and control groups (both for the female and male rats) were found, possibly the result of a retroactive influence by the healer. The differentiation of the white blood cells showed a variety of significant differences; all but one were within the normal range of values for that parameter. The authors emphasize the vital importance of establishing useful clinical parameters for research into paranormal healing.

Introduction

Paranormal or psychic healing is the term used to describe the practice of treating illnesses without a known physical curative agent, i.e., the practice of alleviating physical or psychological discomfort in a paranormal way: the effect appears to be psi-mediated. The effects and the effectiveness of paranormal healing are notoriously difficult to measure (Solfvin, 1982). This is partially caused by the fact that paranormal healing refers to an ill-defined category of phenomena. The term is used for the laying on of hands, distance healing, psychic surgery and faith healing. The clinical parameters needed to study the effects of these different ways of healing on the anatomy and pathology of (diseased) body tissue are as yet not established. Only a few reports concerning research with (experimentally induced) diseases and with investigating these fundamental issues have been published. Snel & Hol (1983) examined differences between 2 groups of hamsters with an experimental disease for haematological and enzymatical differences after distance treatment by a healer. In this study the leucocyte differentiation appeared to be a useful dependent variable for further research. The 'percentage of infected red blood cells' appeared to be an

interesting variable in a study by Solfvin (1982) who investigated the psi-expectancy (the expectation of subjects that a psi-factor will have an effect) of veterinary students concerning malarial mice. Other studies of healing with animals focused on wound healing (Grad, 1961, 1965) and the resuscitation of anaesthetized mice (Watkins & Watkins, 1971; Wells & Klein, 1972). They did not study the clinical variables involved, which is the aim of this study.

Here we report on the results of a paranormal healer who, through retroactive distance healing, tried to exert his psi to prevent the spread and multiplication of blood parasites (malaria) in red blood cells of rats. This experiment is part of a series in which the effects of different kinds of healing are studied; the choice was made to study retroactive distance healing. In this case, distance healing was the only possible method of healing we could use: the animal-caretaker is the only person allowed to enter the part of the laboratory where the animals are kept, to prevent transmission of human infections (e.g., influenza or the common cold).

It was assumed that:

a) when the healer was to have an effect on the infection rate, the mean absolute

number of infected red blood cells should be less in the experimental groups, and

b) the differentiation of the white blood cells in the experimental groups should indicate a less severe reaction to the parasitic infection when compared with normal values for this strain of rats. The leucocyte differentiation percentage should be lower for the experimental groups than for the control groups.

Method

Experimental animals: 20 Nu/Nu (homozygous nude, athymic rats, 12 females and 8 males (N.B.: One female rat died during the experiment). Nu/Nu (nude = no hair) rats do not have a thymic gland, and consequently the immune system is impaired, having no T cells; this allows the possibility of a more distinct effect of the healer's efforts. Twelve days before the start of the experiment (adaptation time for the parasites) the rats were injected with *Babesia rodhani* (rodent malaria). The animals used were bred at the Department of Veterinary Pathology of the Faculty of Veterinary Medicine, University of Utrecht. They were housed in plastic cages at a room temperature of 23 + 2°C and at a 50-60% relative humidity. Food (Muracon I, Pellets, Trouw & Co., Putten, the Netherlands) and tap-water were constantly available, and night and day hours were regulated by clock on a 12-hourly basis. All animals were part of ongoing research in the veterinary laboratory.

Babesia rodhani: *Babesia rodhani* are organisms (of the genus *Babesia*, Protozoa of the order Sporozoa) that parasitize on the erythrocytes of a wide variety of vertebrate hosts, multiplying in the erythrocyte by means of binary fission. *Babesia* are found in the circulating erythrocytes of mammals as pyriform or ovoid bodies and measure approximately 3 microns in length (Smith et al., 1972). In our experiments, a rodent version of the malarial blood parasite, *Babesia rodhani* (Antwerp strain) was used as a stabilate, that is, a population of

organisms preserved in a viable condition (Zivkovic et al., 1983-1984,a; Zivkovic et al., 1983-1984,b). Briefly, the stabilate was stored in liquid nitrogen as a 50% dilution of heavily infected blood (40% parasitaemia) in RPMI 1640 (a sterile standard medium) containing 8% dimethyl sulphoxide. Intraperitoneal inoculation was done with 0.1 ml of the stabilate diluted 1:10 with RPMI 1640. Fresh parasitized blood (with a rising parasitaemia between 20 and 40%) was obtained after first passage of the stabilate and used to inoculate the rats in this experiment. This was done by laboratory staff members who were blind to the nature of the experiment. The course of the infection was monitored, according to standard laboratory procedures, by examining Giemsa stained blood smears on days 0, 14, 28 and 42 (sampling days). Parasitaemias are usually expressed as the percentage of erythrocytes infected. In this experiment we used the mean absolute number of infected red blood cells.

Method of counting infected red blood cells: A drop of fresh blood was smeared uniformly on a glass microscope slide. After drying, the slides were treated with a tint (Giemsa) to make the red blood cells stand out. This was done by members of the veterinary pathology group staff who were not otherwise involved in the experiment. The slides were read in the following manner: a slide was put into a microscope with a special ocular to simplify the counting procedure. The ocular delineated the microscope viewing field into 2 concentric circles, the outer one enclosing exactly 10 times the area of the inner one. First, all the infected red blood cells in the outer circle were counted. Then the total number of red blood cells in the inner circle are counted, whether infected or not. This last number was multiplied by 10 to approximate the number in the outer circle. This procedure was repeated 3 times for each slide, each time focusing on a different section of the blood smear (Solfvin, 1982). The average absolute number of infected cells was then calculated.

Leucocytes: Different blood smears were used for leucocyte differentiation, that is, differentiation of the several kinds of white blood cells, i.e. eosinophil, basophil, neutrophil polymorphonuclear; band and segmented forms, B-lymphocytes and monocytes.

Healer: One professional healer took part in the study. This healer was chosen because of his interest in healing research with animals. According to his own claims, he was able to heal from a distance. He lived 20 miles from the laboratory and had one year of experience as a professional healer. He received from us no written information, only photographs, and was unaware of the experimental conditions.

Procedure

In this description of our procedure, we refer to rats in the experimental and control situation although the assignment to the 2 conditions was made after the completion of the experiment. For the sake of clarity we describe the conditions as if known from the start. The male and female rats were randomly divided over cages: cages 1 and 2 contained 3 female rats in the experimental and 3 female rats in the control condition; cages 3 and 4 each contained 2 male rats in the experimental and 2 in the control condition. Male and female rats were housed separately according to sex to prevent possible complicating factors, such as pregnancies. Every rat was individually marked and recognizable. A photograph was taken of all 4 cages and sent to the healer by mail. The healer was asked to prevent the spread and multiplication of blood parasites (malaria) in the red blood cells of the target rats.

Blood smears were obtained on day -12, to establish normal values for the differentiation of the leucocytes in this group of rats, and on days 0, 14, 28 and 42. The blood smears were analysed after the completion of the experiment.

During the experiment, which lasted 6 weeks, the healer tried to exert his psi (at his own chosen time) for about 10-15 minutes every evening.

An effect of the healer on the dependent variables was here defined as the significant difference between the values measured in the experimental and control groups. The dependent variables were the mean absolute number of infected red blood cells and the percentage of different white blood cells (leucocytes).

Assignment to the conditions: the rats were assigned to the experimental and the control conditions using a random number table. The columns were read down; the odd numbers indicating rats in the experimental condition and the even numbers the control condition. Assignment to the conditions was done by a third person who was otherwise unconnected with the study, and took place after day 42.

Results

In addition to our overall analyses, we decided to break down the results by sex, to see if the results for females differed from those for males.

Absolute number of infected red blood cells: Table 1 shows the mean absolute number of infected red blood cells counted. The overall count of infected erythrocytes should show a steady increase of the absolute number of infected red blood cells. Table 1 shows exceptions to this expectation:

- All animals: on day 28 the absolute number of infected red blood cells for the rats in both conditions show a decrease which continues till day 42 in the control group.

- Female rats: the counts in both groups show a decrease of the absolute number of infected red blood cells on day 28.

- Male rats: the counts show a decrease for the experimental group on day 28 and the expected pattern in the control group.

RETRO-ACTIVE DISTANCE HEALING

Table 1

Mean absolute number of infected red blood cells in Nu/Nu rats in the experimental and control conditions

	Experimental mean (s.d.)		Control mean (s.d.)	
All animals				
day 0	0.0	(0.0)	0.0	(0.0)
day 14	15.7	(31.9)	60.3	(64.6) ^a
day 28	3.3	(4.6)	45.6	(77.4) ^b
day 42	16.0	(27.8)	9.0	(8.3)
Females				
day 0	0.0	(0.0)	0.0	(0.0)
day 14	20.3	(40.8)	64.2	(70.9)
day 28	1.5	(2.3)	8.8	(7.6) ^c
day 42	21.3	(35.5)	11.4	(9.5) ^b
Males				
day 0	0.0	(0.0)	0.0	(0.0)
day 14	8.8	(12.9)	55.5	(66.0)
day 28	6.0	(6.1)	91.5	(104.1)
day 42	8.0	(8.6)	5.0	(5.0)

^a $p=.02$, ^b $p=.015$, ^c $p=.04$

Further, the counts of infected red blood cells in the experimental groups are generally lower than those in the control groups, except on day 42. The differences on days 14 and 28 are significant according to a Mann-Whitney test.

Differentiation of leucocytes: Tables 2 to 7 show the results of the leucocyte differentiation. A number of interesting observations can be made from these tables:

- all values mentioned in the tables (except for one value in Table 2 on day 42) are within the normal range;

- significant differences of the parameters are differences between one group at the low end of the normal range and the other group at the high end of the normal range;

- 3 parameters (eosinophils, lymphocytes and monocytes) already differ significantly on day 0.

Discussion

In this experiment we studied distance healing. Neither the healer nor the experimenters knew which rats were in which condition, a situation not often experienced by healers. Generally, healers cooperate in experiments with known targets. Some of the counts of the mean absolute number of infected red blood cells can be explained by 'normal' factors. For instance, the results of all animals in the control condition that show a decrease instead of an increase in infected cells, can be explained by the progression of the infection, which is accompanied by an increasingly severe anaemia resulting in a decreasing number of red blood cells. The effect on day 28 is different from expectation and can only be 'explained' by assuming that the process of infection in animals with an impaired immune system is different from those

Table 2

Results of leucocyte differentiation in Nu/Nu rats: EOSINOPHILS

Day	Females		Males		All animals	
	Experiment mean (s.d.)	Control mean (s.d.)	Experiment mean (s.d.)	Control mean (s.d.)	Experiment mean (s.d.)	Control mean (s.d.)
0	1.8 (1.6)	1.2 (1.0)	2.0 (0.8)	0.5 (0.6) ^a	1.9 (1.3)	0.1 (0.9) ^b
14	2.3 (1.2)	2.4 (1.1)	2.8 (1.9)	1.8 (2.1)	2.5 (1.4)	2.1 (1.6)
28	2.0 (1.9)	3.2 (2.2)	2.8 (1.0)	1.5 (1.3)	2.3 (1.6)	2.4 (1.9)
42	1.2 (0.8)	4.8 (0.8) ^c	1.0 (1.4)	1.0 (0.0)	1.1 (1.0)	3.4 (2.1) ^d

Normal value: 0-4% ^a $p=.03$, ^b $p=.05$, ^c $p=.002$, ^d $p=.01$

Table 3

Results of leucocyte differentiation in Nu/Nu rats: BASOPHILS

Day	Females		Males		All animals	
	Experiment mean (s.d.)	Control mean (s.d.)	Experiment mean (s.d.)	Control mean (s.d.)	Experiment mean (s.d.)	Control mean (s.d.)
0	0.2 (0.4)	0.7 (0.8)	1.0 (0.8)	0.5 (1.0)	0.5 (0.7)	0.6 (0.8)
14	0.0 (0.0)	0.4 (0.9)	0.5 (1.0)	0.8 (0.5)	0.2 (0.6)	0.6 (0.7)
28	1.0 (0.6)	0.2 (0.4) ^a	0.8 (0.5)	2.0 (1.4)	0.9 (0.6)	1.0 (1.3)
42	0.2 (0.4)	0.2 (0.4)	0.8 (1.0)	1.0 (1.0)	0.4 (0.7)	0.5 (0.8)

Normal value: 0-3% ^a $p=.04$

Table 4

Results of leucocyte differentiation in Nu/Nu rats: BAND FORMS

Day	Females		Males		All animals	
	Experiment mean (s.d.)	Control mean (s.d.)	Experiment mean (s.d.)	Control mean (s.d.)	Experiment mean (s.d.)	Control mean (s.d.)
0	0.7 (0.8)	1.2 (1.3)	1.8 (1.7)	0.0 (0.0)	1.1 (1.3)	0.7 (1.2)
14	0.8 (1.0)	0.4 (0.5)	0.0 (0.0)	0.3 (0.5)	0.5 (0.8)	0.3 (0.5)
28	0.3 (0.5)	0.6 (0.5)	0.5 (1.0)	0.8 (1.0)	0.4 (0.7)	0.7 (0.7)
42	2.2 (2.4)	2.0 (1.0)	0.8 (1.0)	1.0 (1.0)	1.6 (2.0)	1.6 (1.1)

Normal value: 0-2%

RETRO-ACTIVE DISTANCE HEALING

Table 5
Results of leucocyte differentiation in Nu/Nu rats: SEGMENTED FORMS

Day	Females		Males		All animals	
	Experiment mean (s.d.)	Control mean (s.d.)	Experiment mean (s.d.)	Control mean (s.d.)	Experiment mean (s.d.)	Control mean (s.d.)
0	55.8 (9.8)	53.2 (6.2)	63.3 (7.4)	48.8 (12.0)	58.8 (9.3)	51.4 (8.6)
14	48.0 (2.8)	49.2 (13.7)	44.3 (12.3)	54.0 (13.1)	46.5 (7.7)	51.3 (12.8)
28	35.8 (10.4)	51.2 (7.4) ^a	52.0 (7.7)	51.5 (9.8)	42.3 (12.2)	51.3 (8.0)
42	35.5 (7.7)	32.4 (7.0)	49.5 (3.1)	53.7 (17.2)	41.1 (9.4)	40.4 (15.3)

Normal value: 30-64% ^a*p*=.02

Table 6
Results of counts of leucocyte differentiation in Nu/Nu rats: LYMPHOCYTES

Day	Females		Males		All animals	
	Experiment mean (s.d.)	Control mean (s.d.)	Experiment mean (s.d.)	Control mean (s.d.)	Experiment mean (s.d.)	Control mean (s.d.)
0	17.0 (3.4)	10.7 (4.4) ^a	14.0 (5.7)	11.5 (3.9)	15.8 (4.4)	11.0 (4.0) ^a
14	13.7 (6.6)	21.8 (5.5) ^b	21.5 (7.3)	14.8 (5.7)	16.8 (7.7)	18.7 (6.4)
28	31.2 (7.2)	22.6 (5.4) ^c	22.3 (6.1)	24.8 (10.6)	27.6 (7.9)	23.6 (7.6)
42	19.0 (3.5)	25.4 (4.7) ^d	20.3 (6.0)	17.5 (9.0)	19.5 (4.4)	21.9 (7.7)

Normal value: 3-39% ^a*p*=.01, ^b*p*=.04, ^c*p*=.03, ^d*p*=.02

Table 7
Results of counts of leucocyte differentiation in Nu/Nu rats: MONOCYTES

Day	Females		Males		All animals	
	Experiment mean (s.d.)	Control mean (s.d.)	Experiment mean (s.d.)	Control mean (s.d.)	Experiment mean (s.d.)	Control mean (s.d.)
0	24.5 (8.9)	33.2 (5.5)	18.0 (5.6)	38.8 (12.1) ^a	21.9 (8.1)	35.4 (8.6) ^b
14	35.2 (6.5)	25.8 (10.1)	31.0 (5.5)	28.5 (8.7)	33.5 (6.2)	27.0 (9.0)
28	29.7 (7.7)	22.2 (4.6) ^c	21.8 (3.2)	19.5 (4.9)	26.5 (7.3)	21.0 (4.6) ^d
42	42.0 (4.9)	35.2 (7.1)	28.5 (5.9)	22.5 (7.8)	36.6 (8.6)	31.5 (8.2)

Normal value: 18-40% ^a*p*=.03, ^b*p*=.002, ^c*p*=.02, ^d*p*=.05

with a normal immune system. The differences in counts between the experimental and control groups on days 14 and 28, however, cannot be explained by an impaired immune system and/or the progression of the disease. The healer

appeared to be able to affect the number of infected red blood cells: on days 14 and 28 the rats in the experimental condition had significantly fewer infected red blood cells. Although differences between the conditions were to be expected, they were not

supposed to be significant. Parasite counts show a reversed effect on day 42. This might be explained by the duration of the experiment; 6 weeks is a long period for a healer to treat from a photograph, and his interest could have been waning. The healer called twice during the experiment to 'comment' on the state of the rats. He reported shivering, lame hind legs and stiff heads as feelings he experienced when treating the rats. None of these 'symptoms' could be confirmed when checked with the animal caretaker. Only one rat died during the experiment, of an intercurrent disease: the common cold. This is remarkable in itself: normally rats infected with *Babesia rodhoni* do not survive as long as the animals in our experiment.

The results of the leucocyte differentiation show that all values are within normal range. Three of the parameters (eosinophils, lymphocytes and monocytes) already showed a significant difference on day 0. This could be explained in 2 ways. The normal explanation would be that the adaptation of the parasite (from day -12 to day 0) was different in individual rats because of normal physiological differences (e.g., between sexes) and individual reactions after an infection; another explanation would be that these effects were caused by the healer.

We suggest that the significant differences between the groups were due to the retroactive influence of the healer by his psi, although we are aware that much further work must be done before a firm conclusion is warranted, given the small number of animals and the complexity of the results.

In the experiments of Snel and Hol (1983) it appeared that the bandform (young segmented cells) counts in the differentiation could be used as an indicator of a better (healthier) condition, following paranormal healing. In this study we did not find an effect for this parameter although we did find effects for the segmented cells on day 28. This is consistent with the effect on the mean absolute number of infected red blood cells (parasitaemia). The final measurements on

day 42 showed significant differences for the eosinophilic and lymphocytic counts; the female rats in the experimental condition generally were in a better (healthier) condition than the rats in the control condition. The absolute number of infected red blood cells is a sensitive parameter, but its use is very limited because of the nature of the disease. The leucocyte differentiation also appears to be an interesting and useful variable. It is essential for the clinical use of both variables, to be aware of the normal values and ranges for the parameters in the animals studied and to be able to distinguish between normal (physiological) and paranormal effects.

This experiment illustrates the importance of studies in paranormal healing in which fundamental changes in biological systems are systematically researched; it is the only way to further our knowledge with respect to the mechanisms of paranormal healing. General and/or psychological experiments have great value, but the study of paranormal healing has everything to gain from research in the fundamental variables of biological systems, in order to establish the effectiveness of paranormal healing.

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Ned. Ver. v. Parapsychologie
Postbus 271
3720 AG Bilthoven
THE NETHERLANDS

L'effet d'une guérison rétro-active à distance sur la *Babesia Rodhani* (Rodent Malaria) chez les rats

Résumé: On examine dans cet article les résultats des activités de guérison d'un guérisseur paranormal travaillant à distance à partir de photos, dans le cas d'une maladie expérimentale chez des rats (rodent malaria). On a trouvé des différences significatives dans le nombre absolu de globules rouges infectés entre groupes expérimentaux et contrôles (chez des rats mâles et femelles), résultant peut-être d'une influence rétro-active du guérisseur. Le même examen pour les globules blancs a montré une variété de différences significatives, sauf une à l'intérieur de l'échelle normale des valeurs pour ce paramètre. Les auteurs soulignent l'importance vitale d'établir des paramètres cliniques valables pour la recherche sur la guérison paranormale.

Subjective Random Generations and the Reversed Sheep-Goat Effect: A Failure to Replicate

Tony R. Lawrence

Department of Psychology, University College, London

Abstract: Thirty-eight students volunteered in an experiment to replicate Lovitts' (1981) 'reversed sheep-goat effect'. Lovitts found a significant interaction between subjects' belief in ESP (as measured by the Bhadra Sheep-Goat Questionnaire (1966)) and the particular experimental test situation that was described to them. Subjects were told that they were taking a test that would either (a) prove ESP, or (b) disprove ESP. The apparatus and procedure for both groups were the same. Lovitts' design was replicated exactly in the present study, but no significant main effects of belief or test situation were found. Moreover, the expected interaction between belief and test situation did not materialise. A significant association between sex and belief was found, along with a very uneven distribution of sexes in the 4 groups of subjects. The present author discusses how this might have affected the data. Secondly, randomisation tests for target and subject response sequences were computed to test Brugger et al.'s (1990) finding of a sheep-goat effect (SGE) in repetition avoidance. In both the 'prove' and 'disprove ESP' conditions, sheep made more repetitions than goats (significantly so for the 'disprove ESP' condition); a failure to replicate. The evidential status of Brugger et al.'s findings is questioned in the light of 2 other failures to replicate (Blackmore et al. 1991; Broughton, 1991). Finally, several recommendations are made for future SGE research.

Ever since the early pioneering research of Schmeidler (1943), studies examining the relationship between belief and scoring in ESP tests have provided some of the most compelling process-oriented evidence for ESP as a real anomalous phenomenon. These so-called 'sheep-goat effect' (SGE) studies have shown that those who believe in ESP - the 'sheep' - score, on average, higher than those who do not believe in ESP - the 'goats'. Seventy-six per cent of the standard tests of the SGE up to 1971 have shown results in the expected direction; 38% of these experiments were statistically significant (for a review, see Palmer, 1971, 1972, and, more recently, Palmer, 1982).

The only explicit explanation of the SGE is that put forward by Palmer (1972) in his 'need for vindication' theory. For Palmer, the subject in the SGE experiment is presented with a chance to vindicate his or her views with respect to ESP. Palmer argues that a subject in the ESP test who believes in ESP and who subsequently fails to score above chance will come to the conclusion that they have failed to assume the correct opinion with respect to the existence of ESP. This failure is compounded by its taking place in front of a 'competent' person - the experimenter. Hence, the need to vindicate one's beliefs in the experimental situation is high.

A particularly pertinent SGE study using a non-standard design was reported by Lovitts (1981). Lovitts succeeded in making sheep score like goats and vice versa (i.e., sheep scored poorly and goats scored well). Forty subjects were given the Bhadra Sheep Goat Questionnaire (BSGQ)

The author wishes to thank Professor Robert Morris, Dr Alastair McClelland, Dr Adrian Furnham, Dr Chris MacManus, and Brian Duffy for their useful help, advice, and encouragement.

(Bhadra, 1966), and then told either that they were (a) taking part in an experiment to demonstrate ESP ability, or (b) taking part in an experiment to provide support for subliminal perception as a legitimate (non-paranormal) theory of ESP. In every other respect, the test situation, materials, and apparatus were the same for both groups of subjects. Using a tachistoscope attachment on a slide projector, Lovitts presented stimuli to subjects at the threshold of perception. The stimuli used were quite novel. Five slides (pseudo-targets) were made up, each one a composite of 5 symbols; O, +, L, X, and =. On the slides, which differed slightly from one another, each symbol appeared once, overlapping the others to a greater or lesser degree. Each subject saw these 5 slides presented in sequence (1, 2, 3, 4, 5, 4, 3, 2, 1, etc.) for a total of 125 presentations. Upon each presentation, the subject was required to give one response only from the 5 possible symbols on their record sheet. There also existed, in a separate location, and unknown to any of the subjects, a list of ESP target symbols, one of the 5 symbols for each of the 125 trials.

After the experiment, subjects' responses to the BSGQ were collated, and subjects were split about the sample empirical mean into groups of sheep or goats. The aim of Lovitts' study was to manipulate the subjects' expectancies, by varying the instructions provided to them, so as to determine whether there was any change in their response accuracy in the experimental situation. This was done by telling half the subjects that they were taking a novel test for ESP ability using a tachistoscope (which would be expected to elicit a positive reaction from believers and a negative reaction from goats), and the other half that they were taking an experiment to test a legitimate normal theory of ESP success using the principles of subliminal perception (hence the use of the tachistoscope). Of course the deception occurred only for the subjects in the non-ESP context, because the subjects in the ESP context really were taking an ESP test. The non-ESP context was meant to elicit a posi-

tive reaction from sceptics (who should prefer a normal theory of ESP to a paranormal one), and a negative or neutral reaction from believers. Thus, in both the 'disprove ESP' and 'prove ESP' conditions, the tachistoscopic set-up was in fact a decoy or cover for the actual target list. The difference was that subjects in the 'prove ESP' condition (ESP context) were aware of this fact, although they were not aware of the location of the actual target list.

Lovitts' results were quite surprising. No significant main effects were found for belief or test situation, but a significant interaction between belief and test situation was found. This confirmed the view that sheep and goats' typical direction of scoring would reverse when presented with an experimental situation that was counter to their beliefs about the existence of ESP. Thus, sheep in the 'prove ESP' condition scored 9 hits above chance and goats 23 hits below chance, whereas sheep in the 'disprove ESP' condition scored 5 hits below chance and goats 23 hits above chance. None of the deviation scores of the conditions achieved statistical significance on a Critical Ratio test. Lovitts' experiment provides valuable evidence for the view that belief and the influences upon it are strong determinants of scoring in an ESP test. The view that belief is a determinant of true ESP scores in SGE experiments has, however, more recently been questioned by sceptical authors.

Brugger, Landis, and Regard (1990) have recently proposed that the standard SGE is the product of a biased target generation procedure coupled with the biased response generations made by subjects who believe their responses to be random (termed 'subjective random generations' by Brugger et al., 1990). On the basis of their own research, they claim to have established an SGE of repetition avoidance; that is, the tendency to produce fewer double elements ('==', '++', 'OO', etc.) in a response sequence than would be given by mean chance expectation. They found that sheep made significantly fewer repetitions than goats in both ESP and non-ESP contexts. Both sheep and goats produced signifi-

cantly fewer repetitions than mean chance expectation (repetition avoidance is a very common effect in the psychological literature; see Wagenaar, 1972, or Budescu, 1987). Arguing from studies that show the avoidance of repetitions to be a pervasive human and animal phenomenon (Dember & Fowler, 1958), Brugger et al. imply that there is a large biological component to the bias in subjective random generation; this view is in line with the models of Neuringer (1986). The biological component is likely to be an 'endogenous source of variability' (Neuringer, 1986), which is sampled from an early age when opportunities for production or interpretation of chance events arise. Biases in this system's output are likely to influence the development of a cognitive representation of randomness in later years, and hence give rise to the biases commonly found in research on subjective random generations and evaluations. In the case of random sequence evaluation, such biases may give rise to the illusions of control and 'chance baseline shift' for believers in ESP discussed by Blackmore and Troscianko (1985). The corollary of all this is that, via the mediation of a cognitive representation of randomness, there is a strong biological determinant of the SGE in repetition avoidance. It can then be assumed that the sheep's representation of randomness is more biased than that of the goats'. Brugger et al. argue that the more biased productions of sheep coupled with a generally non-random target sequence might well give rise to extra-chance scoring for the sheep. Furthermore, Brugger et al. argue that the SGE in repetition avoidance should be independent of the experimental situation, presumably because the bias is *cognitively mediated* by a *biologically determined* source of variability. Thus one's cognitive system may *accommodate* the bias but not *eradicate* it. The endogenous source of variability that underlies the bias is therefore not open to the influence of cognitions and beliefs; it is 'cognitively impenetrable' so to speak (Fodor, 1983), and motivational factors should not influence the production of repetitions. In so far as Lovitts' study

shows belief-mediated motivational changes in scoring patterns, it would seem the 'reverse SGE' provides a refutation of the strong biological implications of Brugger et al.'s study. If this is so, then doubts must also be raised about the possibility that extra sensory perception can be explained as an *Effect of Subjective Probability*.

But the refutation, as it stands, rests upon the results of only one study. It is in fact quite lamentable that in 10 years no replication of the Lovitts study has been reported, despite Palmer's (1986) suggestion that the Lovitts paper was the kind of research that future studies should seek to replicate. The hypothesis involved in replicating Lovitts' study is that sheep and goats, when presented with a description of the experimental rationale that is opposed to their own beliefs, will reverse their typical direction of scoring. The hypotheses relevant to Brugger et al.'s study are that both sheep and goats will significantly avoid repetitions, and that sheep will do so to a significantly greater extent than goats, in both 'prove ESP' and 'disprove ESP' conditions (because the SGE in repetition avoidance is assumed to be independent of the experimental situation).

In line with the recommendations of Brugger et al. for future SGE studies, randomization tests were to be conducted in the present study for both global and local bias in the target lists. Of course, if the randomization tests showed no significant departures from randomness, then the data would not permit explanation in terms of Brugger et al.'s theory.

Method

Subjects

Thirty-eight University College London psychology students were recruited for the experiment, with 19 subjects in each experimental condition ('prove ESP' and 'disprove ESP'). It had been planned to run 40 subjects, but room allocation difficulties prevented this; the lab had to be given over to other postgraduate research, in order to meet deadlines beyond the author's control.

After the BSGQ was analysed there were 11 subjects classified as sheep and 8 as goats in the 'prove ESP' condition, and 8 sheep and 11 goats in the 'disprove ESP' condition. The subject population comprised 15 men and 23 women, ranging in age from 17 to 42 years. The mean age was 23.4 years; the median was 19 years. Subjects volunteered, and were usually enlisted by finding them during their coffee breaks or lunch in the psychology department common room.

Materials

Target lists were produced 3 weeks prior to the experiment by the experimenter in his home. Forty target lists were made by printing 100 target symbols (+, =, O, L, or X) in random order on each target sheet. The target generator was the random function of the experimenter's Atari 520STFM 16-bit computer. Each target list was folded and sealed inside a numbered envelope, the number corresponding to the subject number. During the experiment, the experimenter inserted the target envelope into a departmental internal mail envelope. Upon the envelope was attached the subject's response sheet.

Subsensory target slides were made by transferring gold-coloured Letraset letters onto paper. The transfers were printed so that the 5 symbols were overlapping in a composite cluster. Five such clusters were made and reduced in size by 64% directly onto acetate transparencies. Each individual acetate cluster was cut down to fit a slide projector slide-mount casing, producing 5 slides, each of a different cluster numbered 1 to 5 (see Appendix for 2 examples of a target slide).

The Bhadra Sheep Goat Questionnaire (Bhadra, 1966) was used to determine the belief of subjects with respect to ESP, both personal and abstract/theoretical. Classification of subjects was based upon the last 6 questions, which are given scores from 1 to 3 each. Thus the highest possible sheep

score was 18 and the lowest goat score was 6. The Bhadra questionnaire was used simply because Lovitts used it in her own study and direct comparisons could therefore be made.

Apparatus

Three Kodak slide projectors with 80 slide capacity were used, 2 for background illumination balance, and one fitted with a tachistoscopic shutter device. A timer box, set to 8ms exposure, was used to time the opening and closing of the tachistoscopic shutter device. A relay switch box coordinated timer, shutter and computer commands, and a BBC microcomputer initiated target presentations and automatically moved slides backwards or forwards.

Design

The independent variables were belief (with 2 levels; sheep or goats) and experimental situation (with 2 conditions - 'prove ESP' and 'disprove ESP'). The dependent variables were subjects' ESP scores and the number of doubles (repetitions of the same element) in an experimental run of 100 responses.

Subjects were assigned in alternation to the 'disprove ESP' or 'prove ESP' conditions; that is, the first subject was allocated to the 'prove ESP' condition, the second to the 'disprove ESP' condition, and so on. Sheep or goat classification was determined post-experimentally from an analysis of the BSGQ scores of subjects, who were divided into groups about the empirical mean of their collective scores. The mean of subjects' belief scores was 10.8. and so sheep were those who scored 11 or above and goats were those who scored 10 or below. In Lovitts' study the mean was 11.8; sheep were those who scored 12 or more, and goats, 11 or less. This difference means that some of my sheep would have been goats in Lovitts' study.

Table 1
Results for randomization tests

List Number	Equiprobability			Inter-trial Independence		
	<i>df</i>	χ^2	<i>p</i>	<i>df</i>	χ^2	<i>p</i>
1-10	4	6.60	n.s.	16	20.77	n.s.
11-20	4	2.03	n.s.	16	15.87	n.s.
21-30	4	1.88	n.s.	16	6.87	n.s.
31-38	4	5.94	n.s.	16	6.19	n.s.
Global	16	1.68	<0.5	64	49.70	<0.5

Two types of randomization test were conducted; one for equiprobability, and one for inter-trial independence. Tests for equiprobability of target symbol frequencies were performed to test for local bias (by analysing the target lists in 3 groups of 10 and one of 8) and global bias (by taking the target list sequences as a whole). For intertrial independence the target lists were analysed in a contingency matrix for three groups of 10 lists and the last group of 8. The individual χ^2 values and degrees of freedom of these analyses were then summed to give a global analysis of randomness. Target list groups for local analyses were constructed for both analyses of randomness as follows; lists 1-10, lists 11-20, lists 21-30 and lists 31-38. Randomization test results are shown in Table 1.

As one can see, there are no significant departures from randomness in any of the equiprobability or inter-trial independence tests. These target sequences, therefore, do not permit explanation of any extra-chance target-response matchings in terms of Brugger et al.'s (1990) theory.

Procedure

The subject was invited into the experimental lab, and sat down to the left of the slide projector sitting some 6 feet from the projection screen; a brief and informal exchange with the experimenter then took place. A large wooden board obscured the experimental apparatus, including the computer and experimenter from the subject's view. The projector carousel was covered so as not to display

the 5 slides for presentation and hence give away the deception required in the experiment. The room was illuminated by artificial lighting, and at no time were the lights allowed to be turned off.

The BSGQ was administered and the subject's questions and problems with the questionnaire were dealt with. After the questionnaire was completed the subject was instructed, according to the group in which they had been placed, using almost exactly the same instructions as in Lovitts' study¹. The experimenter read the following instructions, specific to each group:

'Disprove ESP Group'

Subsensory perception is a method by which information is unconsciously perceived. It involves having information presented below the threshold of perception. Although this information is not consciously seen, it can be brought to awareness, but not 100% of the time.

People who call themselves parapsychologists attempt to prove the existence of what they call extrasensory perception. However, by reviewing the literature, a great many similarities can be

¹ Minor changes were made to the description of the experiment that Lovitts provided to her subjects. In Lovitts' study, for subjects in the 'disprove ESP condition, the comment about 'direct experimenter fraud' was attributed to its author, C.E.M. Hansel (1960). My subjects were unlikely to know who Hansel was and so the comment was said to be one made 'often' by 'sceptical psychologists'. Other phrase changes in the instructions common to both groups reflected simple attempts at clarification.

REVERSED SHEEP-GOAT EFFECT

found between supposed ESP and subsensory perception. This leads me to believe that high ESP scoring could be achieved by subsensory perception of targets as a result of sloppy experimental design or possibly direct experimenter fraud, as sceptical psychologists so often point out. This experiment is designed to show that subsensory perception is a viable alternative to the ESP hypothesis.

I have attempted to stick as closely as possible to the methodology and controls used by parapsychologists and that is why I had you fill out that questionnaire. The only difference between this and a regular ESP test is that I will use a tachistoscope and flash the targets to you on that screen over there for a duration of less than 1/125th of a second. You are not expected to clearly see the target. Just sit back, relax and look at the screen. After the target has been presented record the first image that comes into your mind on the answer sheet provided.

'Prove ESP Group'

The field of parapsychology studies such phenomena as ESP. In the course of well-constructed experimental research, parapsychologists have found striking similarities between subsensory perception and ESP. For example, in both instances information is unconsciously perceived, yet can have a behaviour-altering effect. General information is transmitted more often than specific information. In ESP the best communication of information is in an altered state, whereas in subsensory perception the threshold of perception is lowered when the subject is in a relaxed state. There is some evidence in ESP to suggest that emotion-laden target material is perceived at a higher rate than target material that is not so emotive. Similarly, in subsensory perception, the emotional weight or impact of the target material affects accurate perception rates. ESP is information gathering of target material at seemingly zero intensity, while subsensory perception is information gathering of target material of near zero intensity. In fact, there were even some properties of subsensory perception that

were first found in ESP studies, like high and low aim scoring, terminal salience, unconscious target avoidance and clustering.

I am going to use a tachistoscope to flash images to you for a duration of less than 1/125th of a second. These images will not be consciously perceived but can be brought to awareness, but not 100% of the time. Within these images is embedded an ESP target. Your job is to sit back, relax and look at the screen. After the first presentation (flash) record the first image that pops into your mind on the answer sheet provided.

Subsequently, both groups received the same set of instructions:

Common Instruction Sheet

Inside the envelope is simply a blank sheet of paper and a carbon. The targets are slides of the five symbols; =, +, L, O & X. These symbols on the slides have been derived by a random computer process. By random I mean that they are arranged in a totally arbitrary way; 3, 4, 5 or more of the same symbol may appear consecutively. Likewise a particular symbol may appear only once or twice in the whole run. Do not attempt to guess any particular target order, simply guess freely at the first image that comes into your mind. Also, if you feel particularly confident that you perceived the correct target image put a tick next to that answer.

I will take you through 4 runs of 25 trials each. Please signal me by an OK, NEXT, or DONE that you are ready for the next target. It is unnecessary to spend more than 5 or 10 seconds trying to image the target. Before we start I will give you 10 practice trials. Are there any questions?

The experiment then progressed when the subject was ready. On each of the 10 practice trials, the subjects in both conditions saw the same slides that were to be displayed in the experiment proper, presented at the rate of 1/125th of a second. In contrast, Lovitts' subjects saw simpler, single-symbol target slides presented for

1/37th of a second, half the experimental speed, for both conditions. Subjects in the practice trials received a positive evaluation of their performance after each presentation (e.g., 'excellent', 'good', 'that's fine'), implying to the subject that there was a correct answer and that the experimenter knew it).

After the practice trials, the experimental trials began. The subject was given the response sheet with the target envelope beneath it. The envelope was described as containing a carbon to provide duplicates of the subject's responses; the subject was thus not explicitly oriented to the actual target material, as was also the case for Lovitts' subjects. The experimenter presented a slide by pressing any key on the computer keyboard, whereupon the subject made a written response (in the practice trials the response was verbal and was not recorded). The slide projector commenced with slide number 1 and progressed in sequence back and forth thus: 1, 2, 3, 4, 5, 5, 4, 3, 2, 1... until all 100 trials were over and the responses recorded. During the experiment the experimenter sat by the computer which was placed to the right of the subject. Once all experimental sheets had been filled in for the subject's details, the subject was debriefed and any questions answered. Subjects were all informed of the deception in the 'disprove ESP' condition. The subject's record sheet was coded for the particular condition and then placed with the relevant target envelope and questionnaire for later analysis upon completion of the whole experiment. Each subject's participation from beginning to end took about 15 minutes.

At the end of the experiment the target

envelopes were opened and the lists evaluated for randomness. The analysis of the experimental data proper then began.

Results and Discussion

The Lovitts Replication Data

From Table 3 we can see that there is no overall significance of the main effect of belief. However, there is a suggestive main effect of test situation: $F(1,34)=3.72$, $p=0.059$. This rather suggests the possibility that a weak effect may indeed have influenced the scoring in terms of subjects' reactions to the experimental context, especially in the case of the goats. But, more importantly, there is no significant interaction effect. Hence, this study has failed to replicate Lovitts' study. The pattern of results is made clearer in Table 2a where we see that only the difference between sheep scores in the 'prove ESP' and 'disprove ESP' conditions makes any theoretical sense; this difference is non-significant however ($t[15]=0.699$). The counter-hypothetical difference between goats across test conditions is also non-significant though ($t[15]=1.67$). Dividing the data by sex in Table 2b begins to suggest some intriguing possible influences upon the failure to replicate. Firstly, and most obviously, there are no female goats in the 'prove ESP' condition, and no male sheep in the disprove ESP condition. This observation rather suggests an uneven association between belief and sex in this sample. Indeed, Lovitts found a post hoc suggestive correlation between sex and belief ($c^2(1)=3.26$, $p=.076$) and concluded that the significant effects in

Table 2a
Sheep and goat scores in the 'prove ESP' and 'disprove ESP' conditions

	Prove ESP					Disprove ESP				
	MCE	Hits	Dev.	z	p^a	MCE	Hits	Dev.	z	p^a
Sheep	220	223	+3	0.19	0.84	160	150	-10	-0.88	0.38
Goats	160	170	+10	0.84	0.40	220	195	-25	-1.85	0.06

^a All probabilities are 2-tailed.

her study stemmed from the women in the 'prove ESP' condition and the men in the 'disprove ESP' condition. Thus a post hoc phi correlation coefficient analysis was computed and gave the following result: $\chi^2(1)=5.4$, $p(2-t)<0.05$, $r=-0.377$. There is a significant association between sex and belief, such that women tended towards belief and men towards scepticism. The absence of sheep and goats in those conditions where their performance can provide a baseline for comparison with the belief-favourable conditions (i.e., goats in the

'prove ESP', sheep in the 'disprove ESP' condition) might well explain the results in Table 2 (and hence Table 3). However, the significant psi-missing found for the female goats in the 'disprove ESP' condition remains a mystery. It may be that the female academics in this study had a tendency to water down their belief on paper, possibly betraying a more intuitive feeling for ESP which subsequently manifested itself as psi-missing. But this is really just pure speculation and I shall attempt no further examination of the SGE data.

Table 2b
Sheep and goat scores in the 'prove ESP' and 'disprove ESP' conditions, separately for men and women

	Men					Women				
	MCE	Hits	Dev.	z	p^a	MCE	Hits	Dev.	z	p^a
	Prove ESP									
Sheep	80	71	-9	-1.06	<0.29	140	152	+12	1.09	<0.25
Goats	160	170	+10	0.84	<0.4	---	---	---	---	---
	Disprove ESP									
Sheep	---	---	---	---	---	160	150	-10	-0.88	<0.38
Goats	60	60	0	0	---	160	135	-25	-2.16	<0.03

^a All probabilities are 2-tailed

Table 3
ANOVA for the reversed SGE data

Source	df	SS	MS	F	p
Belief	1	0.01	0.01	<1	n.s.
Test	1	59.06	59.06	3.72	n.s.
Belief*Test	1	9.36	9.36	<1	n.s.
Error	34	539.36	15.86		
Total	37				

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Table 4.

Percentage responses for Bhadra Sheep Goat Questionnaire (1966); figures in brackets are the equivalent data for Lovitts (1981).

	Yes	No
1. Have you ever come to know in advance that you are going to receive a particular letter on a particular day?	50.0% (42.5%)	50.0% (57.5%)
2. Have you ever come to know in advance that someone whom you have not thought of in years is going to call on you?	34.2% (27.5%)	65.8% (72.5%)
3. Have you ever had a dream which later came true?	47.4% (50.0%)	52.6% (50.0%)
4. Are you consistently lucky at whatever you do?	18.9% (27.5%)	81.1% (72.5%)
5. Have you ever suspected a person will fall sick or meet with an accident or die, and this has actually happened?	24.3% (22.5%)	75.7% (77.5%)
6. Have you ever tried in a card game or playing with dice to score in a definite way, i.e. expecting to get one, two or three and succeeded?	55.3% (60.0%)	44.7% (40.0%)
7. If you have ever observed some of the above incidents coming true, please tick any of the reasons given below which you think is correct.		
	Mere chance or luck	26.3% (17.5%)
	Coincidence	63.2% (40.0%)
	Special gift or ESP	10.5% (35.0%)
8. If you try to get more correct scores - for example, out of 25 trials you score 10 or 15 as correct, and this is repeated consistently throughout your trials - what reason can you give?		
	Mere chance or luck	47.4% (42.5%)
	Coincidence	44.7% (27.5%)
	Special gift or ESP	7.9% (30.0%)
9. If some people only get chance scores, some others still less, and some others still more than chance, what reasons can you give for the people who score more than chance expectation?		
	Mere chance or luck	39.5% (42.5%)
	Coincidence	42.1% (27.5%)
	Special gift or ESP	18.4% (30.0%)
10. Do you consider that the existence of ESP or the special gift is -		
	Impossible	7.9% (7.5%)
	Possible	81.6% (80.0%)
	Certain	10.5% (12.5%)
11. Which statement is the best expression of your belief about your own ESP ability?		
	No possibility that I have ESP	50.0% (27.5%)
	Possibly I have ESP	42.1% (67.5%)
	Believe I have ESP	7.9% (5.0%)
12. If you take the card test, can you score -		
	Below chance	5.2% (0.0%)
	At chance	94.7% (95.0%)
	Above chance	0.0% (5.0%)

The Bhadra Sheep Goat Questionnaire (1966)

Because this and Lovitts' study share the same questionnaire, it seems good sense to offer a side-by-side comparison of the results from both studies. Indeed, on post hoc comparison some interesting similarities and differences come to light.

One can see from Table 4 that there is a strong similarity between the responses of the subjects in my study compared with the Lovitts study. Thus a considerable consistency exists in the sample responses to the Bhadra questionnaire given by Lovitts to subjects in the U.S. over 10 years ago, when compared with this British sample. This finding, taken as a whole, suggests that the samples were representative of a consistent range of attitudes towards ESP. The most interesting single difference between responses was for Question 11 where 50.0% of my sample denied outright that they might have ESP whereas only 27.5% of Lovitts' subjects did so, 67.5% preferring to admit the possibility that they might have ESP. Also interesting is the fact that my subjects tended not to favour the ESP hypothesis for Question 8, despite the fact that 10 or 15 hits produced consistently is almost certainly not chance scoring! Yet over 92.0% preferred chance or coincidence as an explanation, which suggests that they perhaps did not comprehend some of the questions. Similarly, Lovitts' subjects seemed to be more willing to infer psi effects in Question 9, where the question describes a near to normal distribution and hence an inference of chance is more correct; 30.0% of her subjects did this.

Repetition Avoidance Replication

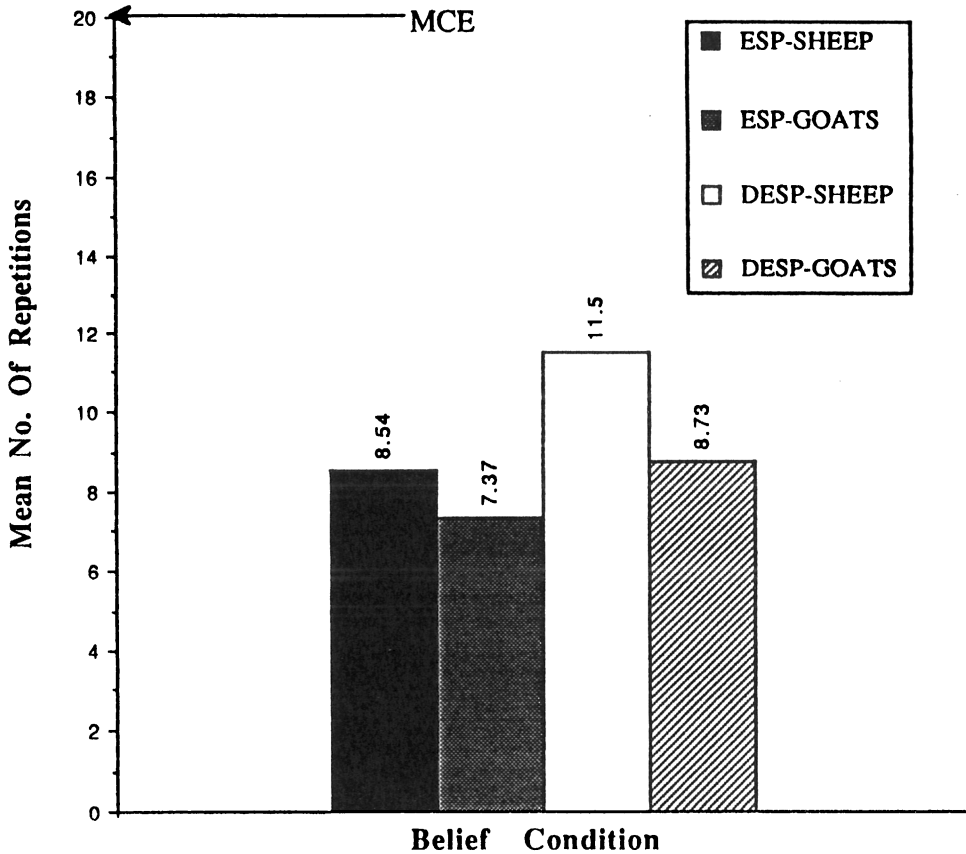
Subjects' total production of doubles (repetitions of paired symbols: ==, ++, OO, etc.) were summed for each group. Higher order serial dependencies (triplets, quadruplets, etc.) were not analysed because

Brugger et al. had no analysis for these response contingencies either and they are not crucial to their theory.

Before we discuss the Brugger replication data, it should be noted that all groups of subjects significantly avoided repetitions: 'prove ESP' Sheep ($t[10]=13.924$, $p<0.0005$), 'prove ESP' Goats ($t[7]=7.293$, $p<0.0005$), 'disprove ESP' Sheep ($t[7]=5.58$, $p<0.0005$) and 'disprove ESP' Goats ($t[10]=4.282$, $p<0.005$).

In Figure 1 we can see that, contrary to Brugger et al.'s hypothesis, sheep showed more repetitions of doubles than goats in the 'prove ESP' condition. This difference is not significant ($t[17]=1.32$, n.s.). Furthermore, we see that sheep showed a marked difference in their production of repetitions, in the direction of less bias than goats. This difference is significant, however; $t(17)=2.92$, $p<0.025$. We see here, then, a total failure to replicate Brugger et al. (1990). This failure to replicate is not, it appears, the only one. Two additional failures to replicate have been reported at the 1991 Parapsychological Association Convention in Heidelberg, Germany (Blackmore, Galaud, & Walker, 1991; Broughton, 1991). With 3 studies now failing to show a sheep-goat effect in repetition avoidance, the evidential status of Brugger et al.'s theory is beginning to look weak. One reason why the effect is not manifest in the above studies may be that subjects were not explicitly asked to produce random responses (very important when the study is concerned with randomization of subject responses; Wagenaar, 1972), which may be crucial in determining whether subjects sample their 'endogenous source of variability'. However, Blackmore's research was conducted with explicit requests to give random responses and still failed to replicate. The simplest conclusion must be that the current evidence does not support the view that there is a consistent sheep-goat effect in repetition avoidance.

Figure 1
 Mean number of repetitions for subjects in all four conditions



General Conclusion

This study has failed to replicate the findings from both Lovitts' (1981) and Brugger et al.'s (1990) studies of the sheep-goat effect in repetition avoidance. There is evidence to suggest that the failure to replicate Lovitts' reversed SGE was influenced by a very uneven distribution of men and women in the various experimental conditions, compounded by a significant association between belief and sex (also found to a non-significant extent by Lovitts [1981]). The failure to replicate stemmed, in the final analysis, from the goats' scoring becoming even worse in the 'disprove ESP' condition; and this

difference was entirely due to the dismissing of the female goats, $p < 0.03$). The fact that Lovitts' subjects were entered into a money prize lottery and mine were performing without monetary reward might also have been a factor, although my subjects generally expressed an interested enthusiasm for the experiment not consonant with reduced motivation.

The failure to replicate the results of Brugger et al. is more conspicuous, however, given the implied biological basis of their effect and the failure to replicate in 2 other studies. At the least, the biological basis of Brugger et al.'s theory is highly questionable, but given the fact that the theory, as currently stated, cannot even

explain the consistent difference in scoring in the SGE studies, it remains for Brugger et al. to propose a theory which actually does attempt to explain the SGE and does not merely manipulate possibilities that fall far short of explanation. Furthermore, Broughton (1991) criticises the Brugger et al. study on 3 points. First, the ESP test used by Brugger et al. was not typical of most tests, and so Broughton advises caution in generalising the findings to ESP studies using more familiar formats. Second, as has already been noted, the instructional set given in ESP tests may give rise to very different response strategies than the instructional set given to subjects in studies of subjective random generation. Finally, Broughton notes that the particular phrasing of sheep-goat questions can also affect results (Palmer, 1972) and that the Brugger et al. questionnaire for determining belief is again atypical.

Some recommendations for future SGE research seem appropriate at this point. Firstly, in concurrence with Palmer (1986), the reversed SGE design employed by Lovitts and myself should really become an important feature of SGE research in the nineties. This is precisely because a significant result using this design is very hard to interpret as anything other than a belief-mediated motivational change. What is more, this design does not depend upon significant main effects of belief or experimental situation but upon the pattern of the interaction between these factors. This type of study is therefore well tailored for process-oriented research. Secondly, all future studies in the SGE field should really be incorporating randomization tests and doubles analyses in subject sequences as standard. The incorporation of randomization tests involves no considerable increase in data analysis and provides a more secure base for the interpretation of any significant extra-chance effects. Thirdly and finally, the finding of a significant post hoc sex difference in beliefs held (women-sheep, men-sceptics) raises the old view of women's 'intuition' and men's 'hard-headed scepticism'. If this is indeed the case then future studies may well wish to address the prob-

lems presented by uneven distributions of men and women between conditions; this study did not, and quite possibly suffered as a result.

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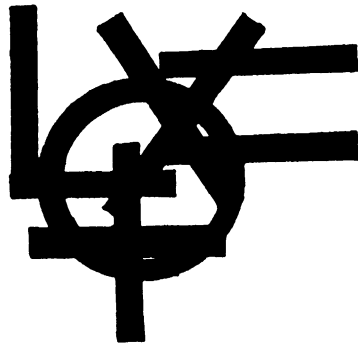
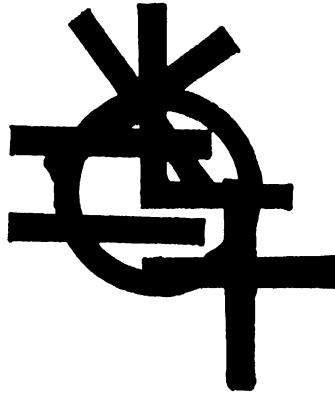
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Appendix

Two examples of pseudo-target slide stimuli for comparison



39 Tidworth Road
Ludgershall
Andover
Hants SP11 9QE
ENGLAND, UK

**Génération aléatoire subjective et effet mouton-chèvre inversé:
Une réplique échouée**

Résumé: Trente-huit étudiants ont participé volontairement à une expérience en vue de répliquer 'l'effet mouton-chèvre inversé' de Lovitts (1981). Lovitts a trouvé une interaction significative entre la croyance de sujets à l'ESP (mesurée par le Questionnaire Mouton-Chèvre de Bhadra (1966)) et la situation de test expérimental particulière que leur avait été décrite. On avait dit aux sujets qu'ils prenaient part à un test qui, soit (a) prouverait l'ESP, soit (b) réfuterait l'ESP. L'appareil et la procédure étaient les mêmes pour les deux groupes. La présente étude a répliqué exactement le paradigme expérimental de Lovitts mais aucun effet significatif de la croyance ou la situation test n'a été trouvé. De plus, l'interaction attendue entre croyance et situation test ne s'est pas produite. On a trouvé une association significative entre sexe et croyance allant de pair avec une distribution très inégale des sexes dans les quatre groupes de sujets. L'auteur examine en quoi cela a pu affecter les résultats. Ensuite, des tests d'aléatorisation ont été calculés pour les séquences des cibles et celles des réponses des sujets, ceci afin de tester le résultat obtenu par Brugger et al. (1990) d'un effet mouton-chèvre dans l'évitement de répétition. Dans les deux conditions de preuve et réfutation de l'ESP, les moutons ont montré plus de répétitions que les chèvres (de façon significative pour la condition de réfutation de l'ESP), ce qui constitue une réplique échouée. On s'interroge sur le statut de preuve des résultats de Brugger et al. (1990) à la lumière de deux autres répliques échouées (Blackmore, 1991; Broughton, 1991). Pour finir, on fait plusieurs recommandations concernant la recherche à venir.

Guidelines for Reporting Details of Experimental Procedure for Papers Published in the European Journal of Parapsychology

Authors submitting reports of experimental studies to the *EJP* are requested to include the methodological information described below whenever possible. The journal editors are aware that some authors may think these guidelines are asking for excessively detailed information. However, until a greater understanding of apparent psi phenomena has been obtained, one cannot know what aspects of an experiment are relevant to its outcome. Such information will help provide the reader and the potential replicator with a better understanding of the work which has been conducted. Also, knowledge of such details may prove important in future meta-analyses.

In recent years, parapsychology has profited from the use of meta-analysis, a set of techniques for examining the pattern of results in groups of similar studies. Such analyses can provide a clearer picture of the consistency of results from study to study. They can also give us better information regarding the study characteristics consistently linked with positive psi scoring. In order for such analyses to be most effective, however, the original research reports must clearly describe as many potentially important characteristics of each study as possible, without being unrealistic. It is thought that the level of detail described below would prove helpful to future meta-analytic work.

The Publication Policy of the *EJP* is printed at the end of each issue, and authors are reminded to obtain information about how to report experimental results, and various formatting information, from this source. Authors are requested to contact the editors if they have any questions about the *EJP* publication policy or these reporting guidelines.

Participant Information

When using human subjects, the following information about the subject and/or agent population is requested when available: (a) number and sex of participants; (b) age range (include median and average where appropriate); (c) general occupational information and educational background; (d) means of recruitment, including how the study and its purpose was presented to potential participants; (e) relationship with experimenter and with other experimental participants (e.g., subject/agent relationship); (f) any personality traits considered in selection; and, (h) participants' prior experience of and their attitudes towards psi, including expectations of success in the study.

When using animals, please try to report, when known: (a) their age and sex; (b) their lab history (e.g., participation in previous studies?); (c) breeding background (e.g., reared in lab.?); and, (d) familiarity with handlers/experimenters.

Please provide the following information about the experimenter(s): (a) prior psi experimental experience; (b) expectation of success in current study; (c) comments regarding the experimenter's perception of their interactions with the subjects; and, (d) any mental strategies, rituals, or other practices enacted by the experimenter for purposes of assisting the well-being of the study.

Preparation of Subjects

Please include as much of the following information as is appropriate: (a) the introduction of the subject to the test environment and testing procedures (including information as to who provided this infor-

mation); (b) who the subject interacts with, and the degree of involvement of this interaction, prior to the actual session (e.g., does the subject meet with different experimenters, subjects, etc.); (c) any written instructions given to subjects (e.g., volitional strategies to elicit a psi outcome, testing procedures, judging instructions, etc.; these may be included in an appendix); (d) how much the subject knew about the study's hypotheses, various conditions, etc.; and, (e) whether the subjects were presented with any assessment of their likelihood of demonstrating psi in the study, or examples of others' successes?

Setting

Given the possible importance of the subject's psychological orientation and impressions of the testing situation and environment, please provide these details when using human subjects: (a) describe the building housing the study (e.g., University, office building, private house, new or old, well-maintained or not, etc.); (b) location of building and ease of access to it; (c) ease of access to the experimental area once within the building; (d) the size, general decor, furnishing and ambience of the experimental areas the subject will encounter; (e) how isolated the subjects are from the normal everyday running of the lab (e.g., will they meet other staff not involved in the experiment?, etc.) and what impression will they gain about the lab (e.g., busy and fast-paced or calm and relaxed; formal or informal, etc.); (f) incidental noise level (e.g., traffic noises, voices or footfall from a corridor, etc.); (g) the time of day the sessions were held (was this determined by subject preference or other considerations?); (h) when the study was run (during which weeks/months); and (i) how many sessions and/or subjects were run a day.

If conducting animal work, please describe: (a) the testing environment, emphasising its similarities and differences from the animal's usual housing; and, (b) the animal's familiarity with the testing environment.

Apparatus and Materials

Equipment

Please describe any mechanical and/or electronic equipment used in the study, including information about: (a) the age and state of repair of any equipment seen by the subject; (b) equipment the subject sees that they might desire information about, other than that used in the study; (c) all details of the equipment which are important to the running of a study (e.g., in a PK/RNG study, describe the source, sampling frequency and randomness checks on the RNG, the screen display seen by the subject, and actions/computer expertise required of the subject, etc.; in an ESP ganzfeld study, describe the light source including details of the colour and wattage of the bulb, and its distance from and warmth upon the subject's face, describe the source, frequency range, and volume of the acoustical noise, and any devices used to record the subject's mentations, etc.).

Props

Please describe any non-electronic equipment used such as reclining chairs, halved ping pong balls (for ganzfeld procedures) and how they are attached, and so on.

Questionnaires

Information which should be provided includes: (a) details of any questionnaires used which are not standard and easily available to other researchers (should be reproduced in the text or in an appendix); (b) when the questionnaire was completed, in relation to the psi task (e.g., some time before or after the test session, immediately before or after the testing, etc.); (c) who administered the test and what information was provided about the questionnaire; (d) the conditions and circumstances surrounding the administration of the

questionnaire (e.g., completed at their home, at their leisure, or in the lab with an experimenter waiting for them to finish it; and, (e) when the subject received feedback about the results from their questionnaire.

Targets

In PK tests, please provide information about: (a) the physical parameters of the target material; (b) what information the subject is given about such; (c) full details of how the target material is represented to the subject (e.g., computer display, etc.); (d) methods of ensuring proper control over the target system (e.g., in a RNG PK task, describe the randomisation methods (if not reported elsewhere); and (e) in both micro and macro-PK, describe the security precautions which protect the target material from normal influences.

In ESP tests, please provide information about: (a) type of target material; (b) any guidelines employed in choosing these (e.g., themes, content, aesthetic qualities, etc. which were especially sought after or avoided); (c) manner of presentation (e.g., if pictorial material was used, whether or not it was mounted on a backing, what kind of envelope it was in, where it was placed in the room, etc.); (4) the randomisation process, including who conducted it, and at what time with reference to the psi task; and, (e) whether duplicate targets were used for judging.

Procedure

The procedure section should include enough information about what occurs from when the subject (and agent, where appropriate) enters the experimental building/environment to when they leave, to enable another investigator to replicate the study. Excluding details which have

been provided elsewhere in the paper, all interactions which occur among everyone that is involved in the session, should be described. The actions of the subject and experimenter (and agent, if appropriate) throughout an experimental session should be described.

Full details should also be provided about the judging of the data in ESP studies. Information should be provided about who performed the judging and their previous judging experience, if any. If there were independent judges, how many were used and how many sessions did each judge. If the judges were selected according to any criteria such as prior training and experience, this should be described. The instructions given to the judges about how to perform the judging task should be described, and copies of any written instructions should be placed in an appendix. Information should be given regarding how long it took to judge each session, and, if one judged multiple sessions, how long it took them to do all of their allotted judging. The time at which judges were given feedback as to the target identity should also be described.

Information should also be provided about when and how the subjects received feedback about their individual performance in the study, and the outcome from the study as a whole.

The above is meant to suggest possible aspects of experimental procedure which an author may wish to report. These guidelines are not exhaustive and individual authors may choose to report information not specifically requested above, and may also choose to omit some factors mentioned above, as they think most appropriate. If authors have any questions about the reporting of their procedure, they should contact the editors of the journal.

Erratum

Addendum and correction to Deborah L. Delanoy's article 'An examination of subject and agent mentation in the ganzfeld', published in *EJP* Vol. 7, No. 2-4, pp. 135-168, 1988-1989.

Addendum

The outcomes of the analysis examining whether certain subject mentation characteristics conveyed target-related information more frequently than other types of responses (reported on page 151 and in Table 2, page 152) differ from those originally reported in Delanoy's PhD thesis (Delanoy, D.L., 1986, *The training of extrasensory perception in the ganzfeld*. University of Edinburgh, Edinburgh, Scotland).

While preparing this work for journal publication, apparent errors were found in this particular set of analyses. These analyses were re-conducted and the outcomes reported in this journal are believed to be correct (no errors were discovered in any of the other analyses). The results from the re-analysis do not substantially change the conclusions drawn in the original report (Delanoy, 1986).

Correction

On page 150, the line under the title of Table 1 reading 'Characteristics using a Wilcoxon Test' was a typographical error and should not have appeared in the Table.